

The Effect of Bank Account Ownership on Credit and Consumption:  
Evidence from the United Kingdom

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Abstract

An electronic transfer mandate in the U.K. Child Benefit program increased bank account ownership rates for families with children. I use this exogenous change to estimate the causal effect of account ownership on access to credit and consumption. When a family transitions into account ownership, I find a 17 percentage point increase in credit card ownership and an increase of 3 household appliances. I do not find robust evidence of changes in vehicle ownership, loan use, or weekly expenditures. My findings suggest that an important benefit of account ownership is access to credit which enables consumption of durable goods.

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## I. Introduction

For families that do not own a bank account – approximately 10 percent of U.S. and U.K. families in 2003 (Aizcorbe et al., 2003; Treasury Committee, 2006) – the consequences of forgoing account ownership may be greater than simply lacking the security and convenience of storing money in a bank. For the unbanked, that is families that own neither a transaction nor a savings account, the lack of a bank account can severely reduce their ability to establish credit and access mainstream borrowing opportunities.

With less access to mainstream credit sources, the unbanked may rely on alternative financial providers, such as pawn brokers, check cashers, and rent-to-own outlets, for short-term credit to finance current consumption. These services charge interest rates much greater than mainstream credit sources – often rates are over 100 percent (HM Treasury, 2004). Policymakers fear that the frequent use of these providers may reduce the economic well-being of low-income families by creating unmanageable debts, as well as diminish the government’s social and equity objectives (HM Treasury, 2004; McGray, 2008). Policy responses typically range from increased regulation to outright bans on their operations (Durbin, 2009; McGray, 2008; HM Treasury, 2004).

In addition to efforts aimed directly at alternative financial providers, policies may also seek to address the demand for these services. For example, California instituted a state policy to encourage banking among low-income families and several large U.S. cities, such as Boston, Miami, and New York, have followed their lead (Clinton and Schwarzenegger, 2008). Although the U.S. does not have a comprehensive federal policy, the U.K. implemented a major, national effort to reduce the number of unbanked families. This effort primarily consisted of an electronic transfer requirement for all public benefits, supplemented with the creation of low-cost bank accounts. The government hoped that the requirement would induce unbanked families to own

an account and, by transitioning families into the banking system, encourage the substitution away from alternative financial providers in favor of mainstream credit options (HM Treasury, 2004).

The related normative question is whether governments *should* encourage bank account ownership. Some policymakers suggest that account ownership is below the socially optimal level and advocate for policies to encourage account ownership, in part, because bank accounts are assumed to encourage the substitution of alternative financial services for affordable credit sources. There is, however, little evidence to support this claim. If account ownership does encourage access to affordable credit, bank accounts may improve the economic well-being of their owners by helping to smooth income shocks, as well as invest in human and physical capital. Knowing how bank accounts affect the use of mainstream credit sources will help to answer this normative question and evaluate the welfare effects of these policies.

In this paper, I examine an electronic transfer mandate in a large, nearly-universal public benefit program, the U.K. Child Benefit. Specifically, I use the modified administration of the Child Benefit – changing electronic transfer from a payment *option* to a payment *requirement* – as a natural experiment to determine the effect bank account ownership has on the financial behavior of less educated families with children.

I first estimate the magnitude of the mandate’s effect on ownership of bank accounts, defined as ownership of a transaction account and/or a savings account. Employing a quasi-experimental design, I find that the mandate boosted bank account ownership by 4 to 5 percentage points (5 to 6 percent) for less educated families with children relative to those without children.

The increase in bank account ownership created by the mandate provides an approach to investigate the causal relationship between bank accounts and use of credit. To my knowledge,

no previous work examined how account ownership may affect the use of credit in a quasi-experimental setting. The implementation of electronic transfer for recipients of the nearly-universal Child Benefit ensures that unobserved preferences for credit are uncorrelated with program participation. With an instrumental variable approach, I measure the effect of owning a bank account on owning a credit card and using a bank loan. In addition, I consider how owning a bank account affects measures of consumption, including vehicle ownership, number of household appliances in a family's home, and level of weekly expenditures. After three years of full implementation of the mandate, I find that owning a bank account has larger effects on the ownership of credit cards than the use of loans. Moreover, account ownership is related to an increase in household appliances, but has no effect on vehicles or weekly expenditures.

The paper proceeds as follows: Section II provides institutional details on the U.K. Child Benefit, the electronic transfer mandate, and account options for Child Benefit recipients; Section III provides a conceptual background for the implementation of the mandate, policies to increase account ownership, and previous work examining the relationship between bank account ownership and other financial behavior. Section IV provides the methodology for the instrumental variable approach. Section V presents the data and measures used in the analysis. Section VI presents the results for each stage of the model. Section VII concludes.

## II. Institutional Details

### *The U.K. Child Benefit*

The Child Benefit is a tax-free, universal benefit received by virtually every family with children (Brewer, 2000). The mother receives benefits monthly on behalf of each eligible child, although single parent and lower income families can opt to receive weekly payments. A child is

eligible if either under the age of 16 or under the age of 19 and enrolled full-time in non-advanced education or an approved training program.

Child Benefits depend only on the number of children in a family. Benefits, set as weekly rates, are determined each fiscal year (April to March). One rate applies to the family's first child and a lower rate applies to each additional child. Measured in constant (2005) pounds, the first child rate increased during the 1990s and remained stable thereafter, while additional child rates have remained stable since 1990. By April 2008, parents received £18.80 a week for their first child and £12.55 a week for each additional child.

For many families, Child Benefits are a significant source of income. Using the Family Resources Survey (FRS), a nationally representative household survey, I compare Child Benefits to family income in the 1999 to 2003 period. Child Benefits average six percent of income for median income families and 19 percent of income for families at the lowest income decile. With the importance of Child Benefits to the budgets of lower income families, a mandate in the Child Benefit program is a substantial inducement to change behavior.

#### *Electronic Transfer Mandate*

The government announced the electronic transfer mandate for all public benefits, including the Child Benefit, in May of 1999.<sup>1</sup> The stated goal of the requirement was not only to prevent benefit fraud and reduce administrative costs, but also to promote the “financial inclusion” of all its citizens (Select Committee on Trade and Industry, 2003). The government considered the financially excluded to be those that lack access to financial services, particularly banking services, but also affordable credit, savings opportunities, insurance, and financial advice.

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<sup>1</sup> All government cash transfers were affected. I focus on the Child Benefit because it has no income limitations and nearly full take-up. Childless families could be eligible other cash transfers, including unemployment benefits, disability benefits, and some means-tested benefits.

The government phased in the mandate for Child Benefits between April 2003 and March 2005, ending the widely used order book (coupon book).<sup>2</sup> With an order book, recipients exchanged benefit coupons for cash at the Post Office each payment period. The mandate also effectively ended the less often used giro cheque (check) form of receipt where beneficiaries received checks that could be cashed at either the Post Office or a bank. The mandate placed no new requirements on those already receiving an electronic transfer directly into a bank account.

By April 2005, the government required all Child Benefit recipients to designate a conventional bank account, a new Basic Bank Account, or a new Post Office Card Account (POCA), to receive their payments. Without providing information on their designated account to the Benefits Agency, claimants would not receive their benefits.<sup>3</sup> In effect, the mandate used the leverage of the public benefit system to transition families out of the cash-only economy and into the modern financial system.

#### *Available Accounts*

Each type of account (conventional bank accounts, Basic Bank Accounts and POCAs) eligible to receive Child Benefits differs in the features it offers consumers. A conventional bank account, defined as an account that earns interest and/or allows for additional deposits, provides the most functionality for owners. Commercial banks offer many different types of conventional bank accounts, including both transaction accounts and savings accounts, with varying levels of services and fees. Transaction accounts are meant for daily use, providing ready access to funds,

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<sup>2</sup> Recipients were not subject to the mandate until they received a letter from the Department of Work and Pensions (DPW) explaining their new account options. DPW staggered these letters over the phase-in period (BBC News, March 13, 2003, <http://news.bbc.co.uk/2/hi/business/2844423.stm>, accessed September 30, 2009). Recipients could also adapt to the new regime before they received a notification letter.

<sup>3</sup> Rare exceptions to this rule exist. For example, if a parent is ill or disabled, a legal appointee can collect their Child Benefits. Additionally, extreme hardship allows the electronic transfer requirement to be temporarily waived.

deposit of additional monies, and bill payment via check or direct debit. Savings accounts have less functionality for daily activities and are meant primarily to store funds for later use.

Commercial banks also offer Basic Bank Accounts, which are low-cost accounts with limited functionality. Basic Bank Accounts became available in 2003 to satisfy government interest in the private provision of low-cost accounts to complement the electronic transfer mandate. Basic Bank Accounts allow deposits; electronic transfers; cash withdrawal at Post Offices, bank branches, and ATMs; no charges for routine banking activities; and, direct debit services. Their distinguishing feature is that they do not provide owners the ability to write checks.

Compared to conventional bank accounts and Basic Bank Accounts, POCAs have the least functionality. Intended for those unable or unwilling to open a bank account, POCAs function like the previously available order books. POCAs only allow benefit receipt over-the-counter at a Post Office. POCAs are not bank accounts because they do not provide the ability to pay bills or allow deposit of additional funds. Moreover, funds accessible via a POCA do not earn interest.

Holding a conventional bank account does not prohibit ownership of a Basic Bank Account or a POCA, just as bank account owners before the mandate could (and did) opt to receive benefits via a coupon or check.<sup>4</sup> I tabulate how families received their Child Benefits using FRS data in Table 1. In the pre-mandate period shown in Panel A, 56 percent of all recipients opted for a coupon book. Additionally, more than half (52 percent) of bank account owners chose a coupon book rather than a direct deposit into their account. Finally, Panel A shows how families at different education levels chose to receive their benefits: 70 percent of families with adults who left full-time education at or before the age of 16 (less educated) chose the coupon book

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<sup>4</sup> Conventional bank account owners may open a Basic Bank Account because they do not value the additional services that conventional bank accounts provide. Conventional bank account owners may open a POCA because, as anecdotal reports suggest, they want to receive benefits at a Post Office or want to separate Child Benefit income from other sources of income (BMRB Social Research, 2006).

while 31 percent of families with adults who left full-time education at or after the age of 19 (more educated) chose payment via a coupon book.

Panel B of Table 1 shows the dramatic change in Child Benefit administration after the mandate was fully phased in. The data do not distinguish between electronic payment into a conventional bank account, Basic Bank Account, or POCA, but, as Table 1 shows, after the mandate's implementation nearly all households (98 percent) report electronic receipt.<sup>5</sup> Differences in payment method by education level also virtually disappeared after the mandate.

### III. Conceptual Background

Prior to the mandate, roughly 10 percent of U.K. families were unbanked (Treasury Committee House of Commons, 2006). Tabulations of banked and unbanked families from the FRS in the pre-mandate period suggest that financial hardship is correlated with unbanked status. Unbanked families are less likely to work and, conditional on working, earn substantially less. The unbanked also have lower levels of mean and median financial assets than banked families. The unbanked are more likely to be single, nonwhite, and less educated. Finally, unbanked families tend to have slightly younger adults than those in banked families. These characteristics are consistent with studies of the unbanked in both the U.S. and the U.K. (Barr, 2004; Carbo et al., 2007; Dunham, 2001; U.S. GAO, 2002; Hogarth and O'Donnell 1997; Hogarth et al., 2004; Klawitter and Fletschner, 2006; Treasury Committee, 2006; Vermilyea and Wilcox, 2002).

The government justified its intervention by pointing to market failures for bank accounts, as well as concerns over equity in access to the financial system (HM Treasury, 2004). A potential source of market failure arises from asymmetric information between banks and consumers.

Banks may expect that applicants with low current income, poor credit history, or previous

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<sup>5</sup> Families that do not report receiving an electronic transfer may occur when there is misreporting or an extreme hardship case. Additionally, some recipients could still be using an order book because the last order books issued contained coupons valid until July 2005.

problems managing an account pose more risk because they are more likely to overdraw their account. As a result of the uncertainty that applicants with these characteristics produce, banks charge more for these accounts, pricing these consumers out of the market, if accounts are available at all.

Families may not have bank accounts for reasons not related to adverse selection. The high fixed costs of designing, marketing, and offering services specifically to low- and moderate-income individuals may dissuade banks from offering these services (Barr, 2004). Some consumers may not apply for an account because they assume they will not qualify or may be unaware of appropriate accounts. Minimum balance requirements and fees may make accounts more expensive than some consumers are willing to pay. Banks may locate in commercial or upper income areas, providing less access to non-urban or lower income residents. Finally, the unbanked may lack the financial education to fully understand the benefits of account ownership.

Account ownership levels may be lower than socially optimal because bank accounts could produce positive externalities that are not fully captured in a private cost to benefit comparison. Ownership allows electronic transfer of benefits, reducing administrative costs and benefit fraud (Barr, 2004; HM Treasury, 2004).<sup>6</sup> If account ownership serves as a gateway to affordable credit or assists families to accumulate financial assets, account ownership could reduce dependence on the social safety net by providing resources to smooth consumption over time, allow investment in human and physical capital, and finance retirement (Barr 2004; HM Treasury, 2004).

To capture these perceived externalities and meet its equity objectives, the U.K. enacted two related policies. First, to stimulate demand for bank accounts, they implemented an electronic transfer requirement for all public benefits. They also increased the supply of low-cost accounts

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<sup>6</sup> The estimated cost of transmitting benefits via an order book is £0.68, while the estimated cost of an electronic transfer is £0.01 (Herbert and Hopwood Road, 2006).

by working with the banking industry to develop Basic Bank Accounts. Basic Bank Accounts should appeal to families that were unbanked because available accounts were too expensive or had undesirable features. These policies addressed both the supply and demand for bank accounts in an effort to transition unbanked recipients into the financial system.

In general, such policies are not adequately studied to either determine their effect on reducing the number of unbanked families or understand the effects of account ownership on other economic behavior. For instance, research examining the response of electronic transfer policies on account ownership consists primarily of evaluations of volunteers in small-scale projects, such as local voluntary tax assistance programs (Beverly et al., 2004; Linnenbrink et al., 2008; Schreiner et al., 2002). While this work provides evidence of a positive effect on account ownership, the highly selected samples used limit their generalizability.

Washington (2006) provides the only use of a large, cross-sectional sample to examine a policy to increase account ownership. Washington studies laws requiring banks to offer low-cost accounts and concludes that these laws increase account ownership rates among minority households after two to three years. If this delayed response does not indicate that other factors explain changes in account ownership, her work suggests that demand responds slowly, as consumers adapt to new account offerings. In the context of the U.K., these findings indicate that the creation of Basic Bank Accounts alone should not have large effects on the banking behavior of unbanked consumers, particularly in the short term.

Although the mandate should influence the unbanked to transition into account ownership, reducing the number of unbanked families was not the sole objective of the government. Rather, by raising bank account ownership levels, the mandate was also intended to increase access to affordable credit and encourage asset accumulation. The government tried to provide substitutes

to alternative credit services by creating and then expanding the Social Fund, which provides small loans and grants to low-income families to meet one-time expenses. Still, many families, particularly low-income families, face high costs in accessing credit (HM Treasury, 2004).

Despite correlations between bank account ownership and use of affordable credit services, the relationship is not well understood. Lower income populations may face difficulties accessing mainstream credit sources, such as credit cards and bank loans, due to their low income levels, their unbanked status, or other characteristics. Alternatively, lower income populations may prefer the options available in the alternative credit market to those in the mainstream market (McGray, 2009). A new literature examining the use of alternative credit services is emerging, but only Rhine et al. (2006) explicitly examine the relationship between owning a bank account and the use of these services. After modeling the decision to not own a transaction account and the decision to use a check casher, Rhine et al. conclude that these decisions are jointly determined. This study provides insight into why families may not own an account but it does not answer what forms of credit newly banked families access, a particularly important question because not all customers of alternative credit services are unbanked.

#### IV. Methodology

In principle, the decision to own a bank account is likely related to other financial decisions, including unobservable preferences for credit. Studying these decisions without correcting for the endogeneity of the ownership decision may lead to estimates that are biased by these unobservable preferences. My estimation strategy uses the electronic transfer mandate in the Child Benefit program as a source of exogenous variation in bank account ownership to estimate the causal effect of account ownership on credit and consumption. This strategy disentangles unobservable preferences for credit from unobservable preferences for program participation.

I adopt a two-stage least squares (2SLS) approach.<sup>7</sup> The first stage is a difference-in-differences equation that compares the change in account ownership for an affected group (less educated families with children) relative to an unaffected group (less educated families without children) after the implementation of the electronic transfer mandate. Although any family could own a newly available Basic Bank Account, the exposure of families with children to the mandate in the Child Benefit program isolates the mandate's effect.<sup>8</sup> Focusing on less educated families creates a sample that should be significantly affected by the mandate due to the correlation between education and account ownership, as well as the importance of Child Benefits to the budgets of lower income families with children. Comparing the response of less educated families with and without children focuses on two groups that face similar labor market and financial environments. In effect, my treatment and control groups are chosen so as to be similar on unobservable factors.

Identification of the first stage requires that no contemporaneous shock differentially affects families with and without children. The major threat to identification arises from the reform of the Family Credit, a wage subsidy to lower income parents working at least 16 hours a week. In 1999, the Working Families Tax Credit (WFTC) replaced the Family Credit. Subsequent reforms in 2003 split the WFTC into the Working Tax Credit (WTC) and Child Tax Credit (CTC). These reforms increased wage subsidies, expanded eligibility to higher income levels, and, eventually, in 2003, provided wage subsidies to those without children.

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<sup>7</sup> Although some specifications include a limited dependent variable in both stages, I choose 2SLS as my primary specification based on Angrist (2001). I also report bivariate probit estimates, where appropriate.

<sup>8</sup> Families without children could be exposed to the mandate if they received certain other public benefit programs. I do not control for these programs because the participation decision is endogenous. Therefore, if the mandate also affected families in my comparison group, my first stage estimates are biased downwards. Approximately 15 percent of my sample of childless families receive at least one the three largest benefit programs (Disability Living Allowances, Incapacity Benefit, and Income Support) subject to the mandate.

Although the creation of the WFTC occurred in the first year of my analysis, if responses occur after a lag, it could differentially affect the response of those with children through two mechanisms. First, increased employment could spur demand for bank accounts because most employers in the U.K. require direct deposit of wages (Collard, 2007). Supplementary credit income could also increase consumption of all goods and services, including bank accounts. The WTC in 2003 could have similar effects for those without children. As a result, specifications that do not include these policies leads to bias, with the direction of the bias depending on the relative magnitudes of the response from the 1999 and 2003 reforms.<sup>9</sup> To control for these reforms, I include a covariate for the maximum value of Family Credit, WFTC, or WTC that a family is eligible for based solely on demographic characteristics.

The second stage of the 2SLS approach isolates exogenous variation in bank account ownership to identify its' effect on credit and consumption outcomes. The mandate meets the two tests of a suitable instrument: it was both orthogonal to a family's preferences for credit and consumption and it increased bank account ownership for less educated families with children. In using this variation, I identify the effect of account ownership on the financial behavior of families with children who, absent the mandate, would have not owned a bank account. This is the local average treatment effect (LATE) described by Imbens and Angrist (1994).

My approach is summarized by the following set of equations:

$$1) \text{ BankAccount}_{it} = \alpha + \beta_1 KIDS_i + \beta_2 POST_t + \beta_3 KIDS * POST_{it} + \beta_4 X_{it} + \varepsilon_{it}$$

$$2) \text{ Outcome}_{it} = \phi + \delta_1 KIDS_i + \delta_2 POST_t + \delta_3 \hat{\text{BankAccount}}_{it} + \delta_4 X_{it} + \eta_{it}$$

where *Outcome* is the credit and consumption outcomes I consider (i.e., credit card ownership, loan use, durable good consumption, and expenditures); *KIDS* is a dichotomous variable

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<sup>9</sup> Brewer et al. (2006) estimate that the WFTC increased employment of single mothers by 5.1 percentage points. Mulheirn and Pisani (2008) conclude that the WTC increased employment of childless workers by 2 to 3 points.

indicating the presence of children and, therefore, eligibility for the Child Benefit program; *POST* designates observations after March 2005 (rather than before April 2003);  $X$  are observable characteristics; and  $\varepsilon$  and  $\eta$  are error terms. I include the same covariates in both equations so, to meet the exclusion restriction required for identification, I exclude the difference-in-differences variable, *KIDS\*POST*, from the second stage equation.

## V. Data

Information on ownership of bank accounts is not in the same dataset as information on credit and consumption. Therefore, I use two complementary datasets, the FRS and the Expenditure and Food Survey (EFS), each of which are cross-sectional, nationally representative surveys of the U.K. population.<sup>10</sup> The FRS provides detailed demographic, income, and bank account information. The EFS is an expenditure survey with a smaller sample size that provides information on weekly expenditures, access to credit markets, and an inventory of household durable goods.<sup>11</sup> I combine this information in my estimation strategy by adopting a two-sample IV approach, first suggested by Angrist and Krueger (1992, 1995).

I keep only less educated families by selecting those families where all adults report leaving full-time education at (or before) the current minimum schooling age 16.<sup>12</sup> I limit my samples to pre-mandate (1999-2003) and post-mandate (2005-2008) survey years, excluding observations

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<sup>10</sup> In 2001, the Family Expenditure Survey was renamed the Expenditure and Food Survey. I use the new name when referring to this survey

<sup>11</sup> The EFS does ask respondents if anyone in the household has a “current account or budget account with a bank or building society.” The wording is such that respondents with only a savings account would be recorded as not owning an account. Additionally, respondents owning a Basic Bank Account could also interpret the question as not including Basic Bank Accounts and, therefore, report not owning a bank account. Indeed, comparing responses to this question to responses in the FRS shows that levels of bank account ownership differ between the two surveys, particularly in the period before the mandate. Therefore, the FRS rather than the EFS is used to estimate the first stage of the model.

<sup>12</sup> Prior to 1972, the legal school leaving age was 15. Adults may report leaving school before age 16 because they are immigrants, dropped out of school, or misreport their education.

from the phase-in period of the mandate (2003-2005).<sup>13</sup> I drop all families where the oldest adult is older than 55 years old because these families could be subject to the electronic transfer mandate in the state pension program. I also drop families from Northern Ireland because data was not collected until the 2002-2003 survey year. I adjust all income, benefit, and loan values to constant 2005 pounds using the Retail Price Index (RPI).

I measure account ownership with the variable *Bank Account*. This dichotomous variable takes on a value of one if at least one adult in the family reports owning a transaction account and/or a savings account. Transaction accounts are accounts where money can be deposited, easily withdrawn, and access to funds is (nearly) immediate via an ATM or checkbook. Basic Bank Accounts are transaction accounts. Savings accounts are not meant for daily transactions but still provide ready access to funds. Savings accounts do not include any type of savings bond, stock, or other account that requires a maturation period. POAs are not considered a bank account.

Because the lack of a bank account is thought to limit the mainstream credit choices available to the unbanked, forcing these families into the alternative credit market to smooth consumption, I consider two measures of access to credit available in the EFS: ownership of a credit card and receipt of a loan from a bank. Credit card ownership includes any credit card or store card account on which interest can be charged. Bank loans include any loan received from a bank, finance house, or credit union. I include several measures of bank loans, including receipt of a loan in the last year, total number of loans currently held, and weekly loan payment amount.

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<sup>13</sup> In 2006, the EFS moved from a fiscal year basis (April through March) to a calendar basis. Therefore, observations from the first quarter of the 2006 survey are dropped because they are identical to the last quarter of the 2005-2006 survey. Unlike the EFS, the FRS treats the fiscal year as the survey year for all sample years. This results in the last FRS observations coming from March 2008 while the last EFS observations are from December 2007.

Credit cards and loans may be difficult for those without a bank account to obtain and are thought to provide lower cost credit than sources in the alternative credit market.

To consider how families may use credit, I also estimate consumption responses with information available in the EFS. I examine measures of durable goods, including vehicles and household appliances, as well as the family's level of weekly expenditures. I use two measures of vehicle ownership: whether the family owns at least one car and whether the family purchased a car in the past 12 months. Vehicles can overcome any spatial mismatch between jobs and housing, while still providing some liquidity through the market for used cars. I also create a measure of household appliances with the number of the following in the family's residence: a freezer, a washing machine, a drier, a dishwasher, a microwave, a computer, and a central heating unit.<sup>14</sup> Household appliances reduce time spent performing household tasks, thereby, increasing opportunities for both work and leisure. Both vehicles and household appliances are useful measures because each of these goods represents large purchases for lower income households and are likely to require the accumulation of funds or use of credit.<sup>15</sup>

Finally, I use weekly expenditure levels taken from the average weekly expenditures of each adult in the family over the survey's two week diary period. This consumption measure reflects the material well-being of the family and takes into account that access to affordable credit can free up resources for daily consumption needs, as well as provide a tool to finance consumption levels that are greater than current income.

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<sup>14</sup> I only count if a family has at least one of these appliances. For example, if a family has two freezers, I determine that the family has a freezer. The maximum value of this variable, therefore, is seven.

<sup>15</sup> With the exception of a microwave, these items tend to cost at least £100 for the lowest priced model at U.K. retail outlets I found on the internet. The cost, however, varies substantially by the item and quality of the appliance. For instance, washing machines ranged from £150 to £1,000. Additionally, central heating can be quite costly with installation at least £1,300 and the components ranging from £50 to £400. Unfortunately, the EFS does not provide information on the estimated value of the household's durable goods, nor does it provide detailed information on the price paid for specific appliances.

I present pre-period observable characteristics for my samples in Table 2. The FRS sample is reported in Columns 1 and 2 of Table 2. Although observable characteristics significantly differ between families with and without children, the presence of children could explain many of these differences. For example, families with children are less likely to be employed than those without (73 percent versus 76 percent) and, conditional on working, the main earner works fewer hours. The necessities of caring for children or the realization of greater productivity from home production are possible explanations for these differences. Similarly, families with children are less likely to be headed by one adult than families without children (37 percent versus 72 percent), either because families with children are married (82 percent of non-single parents) or cohabitating.<sup>16</sup> Higher cohabitation and marriage rates explain the larger earned income of working families with children, but the difference is only £60 more per week. Owing to demographic trends in fertility, families without children are slightly less likely to be nonwhite, as well as either younger (under 25) or older (age 50 to 55) than those with children. Finally, families with children have lower rates of homeownership than families without children (55 percent versus 63 percent) but this is partially driven by the very low rates (12 percent) for parents under age 25.

Table 2 also compares families in the FRS and EFS. There are significant differences for the labor market characteristics of families in the FRS and the EFS. The EFS measures relatively similar employment levels but slightly lower earned income levels (measured in hundred of pounds). Many other observable characteristics are not statistically significantly different between EFS and FRS families and those that do differ are small in economic terms. For

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<sup>16</sup> Adults in the U.K. do not have to be married to be treated as a family under the benefit system. The EFS and FRS both provide information on which members of the household are considered part of the same “benefit unit.” Because the characteristics of the benefit unit determine its eligibility for Child Benefits, as well as other benefits, I use this benefit unit definition to construct my sample families.

example, the FRS records a slightly smaller portion of nonwhite families without children. The age distribution of families between the FRS and EFS differs as well, most notably for families without children. Families without children in the FRS are more likely to be very young families (25 percent under age 25) than families in the EFS (31 percent under age 25).

I show the pre-period outcomes for each sample in Table 3. The first row of Columns 1 and 2 show the low rates of account ownership for less educated families and suggest there is substantial potential for a behavioral response. The ownership rate for families with children is roughly 84 percent. The rate for those without children is almost 2 percentage points greater.

Pre-period outcome variables for credit market access are provided in Columns 3 and 4 of Table 3. Families without children report higher rates of credit card ownership than families with children (58 percent to 51 percent). Although loan use is low for both types of families, families with children are more likely to report receiving a bank loan than families without children (15 percent to 9 percent). Conditional upon having a loan, families with children have a similar average number of loans (1.26 to 1.23) and experience identical weekly loan payments (£45).

Finally, consumption measures are also provided in Columns 3 and 4 of Table 3. Both types of families report similar vehicle ownership and vehicle purchase rates. Families with children report higher consumption levels on other measures – more household appliances (5.3 versus 4.9 appliances) and higher weekly expenditures (£232 versus £147) – than families without children.

#### IV. Results

##### *First Stage Estimates: Bank Account Ownership*

I first estimate how the mandate affected ownership of bank accounts by estimating equation 1. The coefficient of interest is the difference-in-difference estimator,  $\beta_3$ , which measures the relative differences in bank account ownership for families with and without children after the

electronic transfer mandate. The vector  $X_{it}$  controls for other characteristics that may be associated with financial behavior. These include: a cubic in the family's weekly earned income (net of taxes and deductions) in hundreds of pounds; the family's eligible Family Credit, WFTC, or WTC based solely on demographic characteristics; categorical variables for an employed adult in the family, a single adult family, a nonwhite family, and age (in five year age groupings) of the oldest adult in the family. I include a linear time trend to control for secular trends that may affect account ownership, particularly the diffusion of automated technology. Finally, government region effects control for geographic differences in banking and financial services. All regressions are weighted and robust standard errors are clustered by the number of children and time period (Bertrand et al., 2004).

I begin with raw difference-in-differences, reported in Table 4. Ownership of bank accounts increased 5 percentage points among families with children, compared to families without children. Moreover, this relative increase can be explained by the new Basic Bank Account offerings because transaction account ownership shows a similar increase. With pre-period ownership rates of 84 percent and 79 percent for bank accounts and transaction accounts, respectively, these raw differences indicate increases of approximately 6 percent.

I measure the mandate's effect on banking behavior, controlling for observable characteristics in Table 5. The linear probability estimates in Column 1 are slightly smaller than the raw estimates. As a check, I also estimate each outcome with a probit approach in Column 2. The mean marginal effects from probit estimation virtually replicate the raw estimates. Both suggest that observable characteristics have little effect in explaining the relative changes in account ownership. Bank account ownership rates increases 4 to 5 percentage points (5 to 6 percent) for families with children compared to those without children.

In Columns 3 and 4 I show similar estimates for transaction account ownership. Depending on the specification, the probability of owning a transaction account increases by 5 to 7 points (6 to 8 percent). The increase in transaction accounts shows that Basic Bank Accounts and other transaction accounts were a more popular choice amongst Child Benefit recipients than families without children (who could also own a new Basic Bank Account). This is evocative of Washington's 2006 conclusions: expanding the supply of low-cost bank accounts, like the Basic Bank Account, is not enough to substantially boost account ownership in the short-run. Mandating the ownership of an account capable of receiving an electronic transfer for benefit recipients while offering a low-cost option, however, does increase bank account ownership.

#### *Robustness of the First Stage*

One concern that could bias these results is if the mandate reduced participation in the Child Benefit program. The direct and transactional costs associated with obtaining an eligible account could raise the costs of participation enough to reduce take-up. Comparing participation rates, I find that the Child Benefit had extremely high self-reported take-up in both periods: roughly 97 percent of families with children. These estimates provide no evidence of a change in take-up and replicate the government's own estimates (House of Commons, 2006).

Another potential threat to the first stage arises from the series of reforms to the Family Credit. The point estimate on the maximum credit variable is small and insignificant which could arise from insufficient variation, rather than indicate no effect on the propensity to own an account. I test directly for these reforms with the approach of Gregg et al. (2006) who exploit the larger benefits the reform directed to families with younger children (under age 11). If these reforms explain changes in account ownership, ownership should increase more for families with

children under age 11 than for those with children age 11 to 15.<sup>17</sup> I re-estimate the first-stage for families with children under age 16, assigning families with children under the age of 11 to the treatment group and households with children age 11 to 15 to the control group. If the Family Credit reforms increased bank account ownership, the difference-in-differences estimator should be positive and significant.

The results for this robustness check in Table 6 provide no evidence that those families receiving larger benefits from these reforms increased account ownership more than those who received smaller benefits. The point estimates are positive but not statistically significant. Thus, the series of reforms to the Family Credit do not explain my first stage results.

The first stage estimates provide evidence that the electronic transfer mandate affected the banking behavior of families with children. This increase provides an exogenous change to identify the impact of bank account ownership on use of credit and consumption.

#### *Second Stage Results: Use of Credit*

Bank accounts could provide lower cost access to credit, allowing families to forgo more costly credit sources such as pawn brokers and rent-to-own outlets, in favor of credit cards and bank loans. I investigate this possibility with reduced form and two-sample IV estimates of the measures of access to mainstream credit sources: credit card ownership, receipt of a loan in the last year, total number of loans held, and weekly loan payment. The standard errors in the IV estimates are bootstrapped to account for the error in the first stage prediction (Murphy and Topel, 1985).

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<sup>17</sup> Gregg et al. consider both the reforms to the Family Credit and increases in the Child Benefit and Income Support (cash welfare). In their main specification, they adopt a triple-difference estimator which compares the behavior of lower income families with young children to higher income families with older children across time. I do not difference the data by income for two reasons. First, high-income families already had virtually universal levels of account ownership in the pre-period. Therefore, these families cannot respond to the mandate by increasing their ownership of accounts. Secondly, income is an outcome of many these reforms as they provided incentives for increased labor supply on both the intensive and extensive margin.

I begin with examining how the instrument affects credit card ownership and loan use by performing difference-in-differences estimates in the first row of Table 7. These reduced-form results, intended to determine if there is a relationship between the mandate and the outcome of interest, will provide preliminary evidence for or against a relationship between bank account ownership and access to credit. In the first row of Table 7, the largest effect of the mandate is for the credit card ownership outcome – ownership increased 8 points after the mandate. The mandate is also associated with a small increase in the average weekly loan payment (£2.55). The point estimates on the probability of receiving a loan in the last year and the total number of loans are both positive (0.02 percentage points and 0.04 loans, respectively) but not statistically different from zero.

With linear specifications in both stages of the model, I perform two-sample IV estimates in the second row of Table 7. All IV estimates are positive but only the point estimate for the credit card outcome is statistically significant. The estimate indicates that by transitioning into bank account ownership, a previously unbanked family increases the probability that they will own a credit card by 17 percentage points.

In Panel B of Table 7, I check if this result is sensitive to the linear specification by estimating the model with a bivariate probit approach. The marginal effects from this approach nearly replicate the linear estimates and provide robust evidence that nearly twenty percent of new bank account owners also became owners of a credit card. Although the two sample approach prevents the calculation of the number of banked families with children that owned a credit card in the before the mandate, ownership of credit cards by all families with children was 51 percent. Therefore, the 17 point increase suggests an economically large effect of account ownership on ownership of a credit card.

The second panel of Table 7 also provides results from alternative specifications for the loan outcomes: bivariate probit for the receipt of a loan in the last year (Column 2), a poisson approach in the second stage for the number of loans (Column 3), and a tobit specification in the second stage for weekly loan payments (Column 4). The marginal effect for the receipt of a loan in the last year using a bivariate probit estimation approach is positive and statistically significant. The estimate suggests that when a previously unbanked family transitions into account ownership, the probability that they will receive a loan from a bank increases by 33 points. This is a very large effect as only 15 percent of families with children in the pre-period received a loan in the last year. However, with the lack of statistical significance in the linear approach, this finding is not robust to the specification.

Unlike the receipt of a loan in the last year, the results for other credit access outcomes using alternative specifications are similar to the linear specification. The marginal effect from a poisson approach for the number of loans currently held is positive but the bootstrapped standard errors are too large to conclude statistical significance. Additionally, the marginal effect of owning a bank account on the level of weekly loan payments in a tobit specification is positive but not statistically significant.

While these results suggest that some changes in borrowing from banks may have occurred, the evidence that new bank account owners access credit markets by owning a credit card is the most robust. The decision to use a credit card rather than obtaining a bank loan may be explained by lower transaction costs associated with credit cards than loans. Due to the pervasiveness of the credit card in society, credit cards may be more familiar to consumers who want to increase their consumption. Additionally, credit cards may present an easier application process and less

stringent approval criteria than a bank loan. An alternative explanation for this increase in credit card ownership is a decision by banks to market and sell credit cards to their new customers.

*Second Stage Estimates: Consumption*

The increase in credit card ownership could be related to families realizing greater consumption possibilities, particularly the need to finance large purchases such as durable goods. To examine the effect of bank account ownership on consumption, I estimate the same two-sample IV model as in the estimates for use of credit. The consumption outcomes include ownership of at least one vehicle, purchase of a vehicle in the past year, number of household appliances (freezer, washing machine, drier, dishwasher, microwave, computer, and central heating unit) in the family's residence, and level of weekly expenditures. Although the durable good outcomes are not exhaustive of the durable goods available for purchase, they constitute the most relevant durable good information available in all sample years of the EFS.

I report the reduced form and two-sample IV estimates for consumption in Table 8. Of these consumption outcomes, only the reduced form estimate for the number of household appliances is positive and statistically significant. The estimates for vehicle purchase and weekly expenditures are positive but insignificant, while the point estimate for vehicle ownership is small, negative, and statistically insignificant. Save for the reduced form estimate for household appliances, there is little indication that consumption changed as families transitioned into account ownership.

The second row of Table 8 provides the IV results for these consumption outcomes. The IV estimates for vehicle outcomes and weekly expenditures are statistically insignificant and, for vehicle ownership, the point estimate continues to be negative. These suggest that there was no

significant change in vehicle ownership, vehicle purchase, or expenditure levels arising from the transition of unbanked families into account ownership.

In contrast to the results for vehicle ownership and weekly expenditures, the result for household appliances is large, positive, and statistically significant. The IV estimate shows an increase of three household appliances when previously unbanked families became owners of a bank account. This result suggests an increase in durable good consumption when families enter the formal financial sector.

I test the robustness of the results for the number of household appliances with a poisson specification in the second stage. The marginal effects from this estimation, shown in Panel B of Table 8, are again quite large and show an increase of more than two household appliances when unbanked families transition into account ownership. With families with children owning, on average, 5.3 household appliances in the period before the mandate, the magnitude is again considerable. The results for changes in household appliances are large and robust to functional form. There is an economically important effect on durable good consumption when families transition into account ownership.

I use a bivariate probit approach for the vehicle ownership outcomes to provide an alternative estimation approach. The marginal effects from these estimates are considerably different from the linear specification. The marginal effect for vehicle ownership is positive, although still not statistically significant, and the marginal effect for vehicle purchase is large and quite significant. The positive effect for vehicle purchase in a bivariate probit specification suggests that previously unbanked families may purchase vehicles as they transition into account ownership but the effect is not robust to the specification.

Taken as a whole, the consumption results indicate if a family has high consumption needs, providing access to more affordable credit creates an opportunity to meet those needs. The response in household appliances, but not vehicles, is consistent with the previous results for credit cards given that household appliances are often purchased with a credit card while vehicles are usually purchased with a loan. Still, the magnitude of the effect is striking, particularly because both mean and median total income for families with children only increased by approximately £10 a week in the period after the mandate. Consumption increases without commiserate increases in income implies that consumer debt is rising. Unfortunately, changes in the level of indebtedness cannot be measured because neither the FRS nor the EFS collects information on non-mortgage consumer debt.

### *Mechanism*

One possible reason that ownership of credit cards and household appliances increased after the change in bank account ownership levels is the presence of liquidity constraints. Prior to the mandate, unbanked families desired higher levels of consumption but did not have adequate borrowing opportunities to finance this consumption. Although neither the FRS nor the EFS provides direct information on the extent to which families are constrained, the administration of the Child Benefit does provide a method for identifying those families facing greater liquidity constraints.

The default payment period for the Child Benefit is a monthly payment. However, families headed by a single parent and families that receive the low-income Child Tax Credit, can choose to receive weekly payments. Families opting for a weekly payment are an easily identifiable group who are likely to be constrained because their choice of the weekly payment suggests they may not be able to smooth consumption over the course of the month.

Table 9 compares the mean credit and consumption outcomes for Child Benefit recipients by payment period and time. In addition, because the distribution of expenditures is slightly right skewed, I also show median levels of weekly expenditures in brackets. The third column of Table 9 provides the difference for weekly recipients before and after the mandate. This column demonstrates that for nearly every outcome (except for vehicle purchase in the last year and weekly expenditures), weekly recipients increased their use of credit and consumption in the period after the mandate. Similar calculations for monthly recipients in Column 6 demonstrate the contrast in their behavior over time. Monthly recipients show a drop in every measure of use of credit and consumption (except for household appliances) in the period after the mandate. These differences suggest that weekly and monthly recipients displayed different responses to the mandate on their choices about credit and consumption.

In Column 7 of Table 9, I perform raw difference-in-differences estimates to demonstrate the relative differences in the responses of these two groups. I consider weekly recipients as my treatment group because they self-identify as liquidity constrained. Monthly recipients are the control group. The estimates show that weekly recipients experienced positive and significant increases in their ownership of credit cards, number of loans, weekly loan payments, ownership of vehicles, number of household appliances, and median level of weekly expenditures, relative to their monthly recipient counterparts.

These simple differences show that increases in the use of credit and level of consumption are concentrated among the likely liquidity constrained weekly Child Benefit recipients. These families may have previously been unable to gain access to affordable credit in order to finance their desired consumption levels but, by transitioning these families into the formal financial sector, the mandate allowed them to borrow at lower rates and consume more.

## VI: Conclusion

In this paper, I use a new approach to identify the effect of bank account ownership on use of credit and consumption. I first show that an electronic transfer mandate in the U.K. had large effects on account ownership. Using this exogenous change in bank account ownership, I find bank account ownership increases ownership of credit cards and the number household appliances. I find no robust effect, however, on receipt of loans from a bank, vehicle ownership, or level of weekly expenditures.

My results suggest that owning a bank account has a significant effect on the use of credit. These results may shed light on why some families may use alternative credit options that are more costly than mainstream credit sources: they do not have a bank account by which to gain access to these sources. While further work is needed to test this explanation – particularly the extent to which mainstream credit sources are substitutes for alternative credit sources – my results suggest that, if these two credit sources are substitutable, the U.K. government may have succeeded in its goal to increase access to affordable credit by crowding out alternative credit services. In effect, by encouraging the unbanked to transition into account ownership, policymakers in California and in the U.K. may be providing a method for families to access credit at lower costs. To the extent that policymakers want to encourage bank account ownership to reduce the use of alternative financial providers, forcing the unbanked into a bank account may be a desirable policy.

Measuring large responses for consumption provides evidence against the notion that the poor would save more if only they had a bank account.<sup>18</sup> These responses could represent self-

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<sup>18</sup> Work in the U.S. suggests that low-income families do not save because of asset tests in means-tested cash-welfare programs (Hubbard et al., 1995; Ziliak, 2003; Hurst and Ziliak, 2006). As a result, low-income families save by accumulating durable goods because these often do not limit eligibility for means-tested programs. The U.K. maintains asset tests in their cash welfare program, Income Support, but the asset limits are so high (£16,000) that

control problems that lead families with children favoring current period consumption over future consumption. Indeed, evidence from food consumption suggests lower income families do have self control problems that prevent smoothing over the course of a month (Hastings and Washington, 2008; Shapiro, 2005). If self-control problems lead to “excessive consumption”, the increase in credit card ownership for previously unbanked families could result in high levels of consumer debt. In such a case, bank account ownership may not improve long-term economic well-being and may only fuel the very same cycle of debt that policymakers try to end when they attempt to regulate or ban the operations of alternative financial providers. This is particularly true in light of findings by Stango and Zinman (forthcoming) that these families underestimate the cost of short-term debt.

The result for increases in credit card ownership is also troubling in light of the potential for adverse selection in the credit card market (Ausubel, 1999). Credit cards are far from a uniform product. Those consumers that accept an offer from a credit card company may be more likely to default than consumers that choose to not accept the offer. The EFS does not provide information on the type of credit cards families hold, nor does it provide any information on the terms associated with these cards. Still, a higher likelihood of default, potentially coupled with self-control problems, suggest that gaining a credit card as a result of owning a bank account may potentially produce severe financial distress in the long run.

Undoubtedly, more work needs to be done on the types of credit used by lower income families, both banked and unbanked, before the welfare implications of policies that encourage bank account ownership can be fully evaluated. What is clear, however, is that policymakers need to more fully consider how transitioning the unbanked into the banking system may affect

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few potential applicants are affected. Thus, means-tested programs in the U.K. would not explain the decision to save through durable good purchase rather than financial assets. I explore the savings decision in a related paper.

the overall economic well-being of these families. In particular, policymakers should carefully how policies may affect access to and use of different credit sources, as well as the effects credit access has on consumption needs and consumer debt levels.

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Table 1. Payment Methods of Households Receiving Child Benefit, By Time Period

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Panel A. Pre-Mandate Period (April 1999 - March 2003)

	Coupon Book	Bank	Check	Other
All Families	55.5	43.9	0.3	0.3
Without A Bank Account	96.1	2.2	1.0	0.8
With A Bank Account	51.5	48.0	0.2	0.3
<u>Education Groups By Age at School Leaving</u>				
Less Educated	70.1	29.5	0.4	0.4
More Educated	30.5	69.1	0.2	0.2

Panel B. Post-Mandate Period (April 2005 - March 2008)

	Coupon Book	Bank or POCA	Check	Other
All Families	1.1	97.6	0.7	0.7
Without A Bank Account	10.0	77.3	9.4	3.3
With A Bank Account	0.8	98.2	0.4	0.6
<u>Education Groups By Age at School Leaving</u>				
Less Educated	1.6	96.5	1.2	0.8
More Educated	0.6	98.4	0.3	0.6

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Note: Authors' calculations using FRS data 1999-2003 and 2005-2008. Other methods of receiving the Child Benefit include the Benefit Payment Card project and payments made to registered charities on behalf of disabled persons. Families are considered owning a bank account if at least one adult in the household reports owning a Basic Bank Account, Current Account, NSB Investment Account, NSB Ordinary Account, or Savings or Investment Account. See text for definitions of less and more educated groups.

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Table 2. Pre-Period Observable Characteristics for Families With and Without Children, by

	Family Resources Survey		Expenditure and Food Survey	
	With Children (1)	Without Children (2)	With Children (3)	Without Children (4)
Working Families	0.725 (0.446)	0.758 (0.428)	0.738 (0.440)	0.737 (0.441)
Net Earned Income, if Working	3.886 (5.676)	3.219 (4.332)	3.795 (2.187)	2.903 (1.936)
Weekly Hours of Main Earner, if Single Adult	40.517 (14.857)	42.332 (11.853)	40.360 (15.150)	40.849 (12.742)
Nonwhite	0.373 (0.484)	0.723 (0.447)	0.351 (0.477)	0.738 (0.440)
Homeowner	0.071 (0.257)	0.052 (0.222)	0.067 (0.251)	0.057 (0.231)
Number of Children	0.545 (0.498)	0.630 (0.482)	0.540 (0.498)	0.616 (0.486)
Age 0 to 4	1.846 (0.931)	-	1.823 (0.924)	-
Age 5 to 10	0.445 (0.657)		0.356 (0.579)	
Age 11 to 15	0.641 (0.766)		0.665 (0.778)	
Age 16 to 18	0.596 (0.742)		0.584 (0.726)	
Age Distribution of Oldest Adult	0.164 (0.395)		0.144 (0.373)	
Under 25	0.061	0.252	0.059	0.312
Age 25 to 30	0.097	0.100	0.088	0.094
Age 30 to 35	0.182	0.099	0.172	0.093
Age 35 to 40	0.243	0.093	0.258	0.079
Age 40 to 45	0.216	0.102	0.226	0.100
Age 45 to 50	0.132	0.139	0.127	0.127
Age 50 to 55	0.069	0.214	0.070	0.195
Observations	14,977	19,064	3,880	5,347

Note: Author's calculations using FRS and EFS data from 1999-2003. All statistics are weighted. Net earned income is adjusted to constant 2005 pound and reported in hundreds

Table 3. Pre-Period Outcomes for Families With and Without Children, by Survey

	Family Resources Survey		Expenditure and Food Survey	
	With Children (1)	Without Children (2)	With Children (3)	Without Children (4)
<u>Account Ownership</u>				
Own Transaction Account	0.788 (0.409)	0.804 (0.397)	-	-
Own Bank Account	0.843 (0.363)	0.859 (0.345)	-	-
<u>Credit Market Access</u>				
Credit Card Ownership	-	-	0.505 (0.500)	0.579 (0.494)
Received Bank Loan in Last Year	-	-	0.154 (0.361)	0.086 (0.280)
Number of Bank Loans, for Loan Holders	-	-	1.262 (0.551)	1.227 (0.509)
Weekly Loan Payment, for Loan Holders	-	-	44.642 (33.012)	45.127 (30.264)
<u>Consumption</u>				
Own at Least One Vehicle	-	-	0.727 (0.445)	0.736 (0.441)
Vehicle Purchase in Previous 12 months	-	-	0.252 (0.434)	0.253 (0.435)
Household Appliances	-	-	5.261 (1.145)	4.913 (1.346)
Weekly Adult Expenditures	-	-	232.454 (160.689)	147.003 (135.967)
Observations	14,977	19,064	3,880	5,347

Note: Author's calculations using FRS data from 1999-2003 and EFS data from 1999-2003. All statistics are weighted.

Table 4. Raw Difference-in-Differences Estimates of Impact on Family Bank Account Ownership

	Pre-Mandate (April 1999 - March 2003)	Post-Mandate (April 2005 - March 2008)	Time Difference for Groups
<u>Bank Accounts</u>			
Households with Children	0.843 (0.003)	0.943 (0.003)	0.100 (0.004)
Households without Children	0.859 (0.002)	0.909 (0.002)	0.050 (0.004)
Group Difference at Point in Time	-0.016 (0.004)	0.034 (0.004)	
	Difference-in-Differences		0.049 (0.006)
<u>Transaction Accounts</u>			
Households with Children	0.788 (0.003)	0.926 (0.003)	0.138 (0.005)
Households without Children	0.804 (0.003)	0.885 (0.003)	0.081 (0.005)
Group Difference at Point in Time	-0.016 (0.004)	0.041 (0.004)	
	Difference-in-Differences		0.057 (0.007)

Source: Author's calculations using FRS data from 1999-2003 and 2005-2008. All reported means are weighted.

Table 5: First Stage Difference-in-Differences Estimates for Family Bank Account Ownership, With Covariates

	<u>Bank Account</u>		<u>Transaction Account</u>	
	LPM (1)	Probit (2)	LPM (3)	Probit (4)
Kids	-0.006 (0.008)	-0.001 (0.006)	-0.006 (0.011)	-0.002 (0.008)
Post Period	0.017* (0.010)	0.020** (0.008)	0.025*** (0.008)	0.028*** (0.005)
Kids*Post Period	0.039*** (0.008)	0.053*** (0.004)	0.045*** (0.010)	0.066*** (0.006)
Observations	53,811	53,811	53,811	53,811

Note: Author's calculations using FRS data from 1999-2003 and 2005-2008. All regressions are weighted and standard errors clustered by time and the number of children. Other covariates include a cubic in weekly earned income; the maximum Family Credit, WFTC, or WTC credit based on demographic characteristics; dichotomous variables for single adult family, employed family, nonwhite family, and homeowner; fixed effects for the five year age grouping of oldest adult; fixed effects for government region; and, a linear time trend. Earned income and maximum benefit values are both adjusted to constant 2005 pounds. Earned income is reported in hundreds of pounds per week. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Table 6. First Stage Robustness Check, Difference-in-Differences Estimates for Account Ownership by Age of Children in Family

	Bank Account (1)	Transaction Account (2)
Young Children	0.007** (0.004)	0.007 (0.004)
Post Period	0.046*** (0.014)	0.053*** (0.013)
Young Children*Post Period	0.013 (0.011)	0.018 (0.013)
Observations	21,903	21,903

Note: Author's calculations using FRS data from 1999-2003 and 2005-2008. Estimates are created with a probit regression. Reported coefficients are the mean of the marginal effects. All regressions are weighted and standard errors clustered by the number of children and time. Treatment families are defined as families with all children under the age of 11. Control families are families with all children age 11 to 15. Other covariates include a cubic in weekly earned income; the maximum Family Credit, WFTC, or WTC credit based on demographic characteristics; dichotomous variables for single adult household, employed household, nonwhite household, and homeowner; fixed effects for the five year age grouping of oldest adult; fixed effects for government region; and, a linear time trend. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Table 7. Two Sample IV Estimates for the Effect of Bank Account Ownership on Credit Card and Bank Loan Outcomes

	Credit Card (1)	Loan in Last Year (2)	Number of Loans (3)	Weekly Loan Payment (4)
Panel A. Linear Estimates				
Reduced Form	0.063*** (0.028)	0.022 (0.020)	0.039 (0.037)	2.550* (1.586)
Second Stage Estimate	0.171*** (0.071)	0.401 (0.388)	0.493 (0.671)	40.682 (30.390)
First Stage F Statistic	26.420	26.420	26.420	26.420
Panel B. Marginal Effects from Alternative Specifications				
Bivariate Probit	0.187*** (0.097)	0.328*** (0.110)	-	-
Second Stage Poisson	-	-	0.768 (0.679)	-
Second Stage Tobit	-	-	-	28.607 (22.747)
Observations	13,805	13,805	13,805	13,805

Note: Author's calculations using FRS data from 1999-2003 and 2005-2008 and EFS data from 1999-2003, 2005-2007. All regressions are weighted and standard errors clustered by number of children and time. The standard errors for these estimates are calculated using 999 bootstrap replications. The results in Panel A generated with a linear regression in both stages, except for the first stage of the credit card estimates which are performed with a probit first stage to ensure that the resulting coefficient is bounded between [0,1]. For estimates in Panel B, the first stage of the Poisson and Tobit regressions use a linear approach. The marginal effect of the tobit specification is the effect for families with a strictly positive amount of weekly loan payment. Other covariates include a cubic in weekly earned income; the maximum Family Credit, WFTC, or WTC credit based on demographic characteristics; dichotomous variables for single adult household, employed household, nonwhite household, and homeowner; fixed effects for the five year age grouping of oldest adult; fixed effects for government region; and, a linear time trend. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Table 8. Two Sample IV Estimates for the Effect of Bank Account Ownership on Consumption

	Own at Least One Vehicle (1)	Vehicle Purchase (2)	Number of Household Appliances (3)	Weekly Expenditures (4)
Panel A. Linear Estimates				
Reduced Form	-0.006 (0.024)	0.030 (0.025)	0.135*** (0.064)	8.861 (10.267)
Second Stage Estimate	-0.343 (0.427)	0.638 (0.508)	3.069*** (1.337)	55.820 (144.257)
First Stage F Statistic	26.420	26.420	26.420	26.420
Panel B. Marginal Effects from Alternative Specifications				
Bivariate Probit	0.138 (0.091)	0.353*** (0.085)		
Second Stage Poisson	-	-	2.314*** (1.337)	-
Observations	13,805	13,805	13,805	13,805

Note: Author's calculations using FRS data from 1999-2003 and 2005-2008 and EFS data from 1999-2003, 2005-2007. All regressions are weighted and standard errors clustered by number of children and time. The standard errors for these estimates are calculated using 999 bootstrap replications. Other covariates include a cubic in weekly earned income; the maximum Family Credit, WFTC, or WTC credit based on demographic characteristics; dichotomous variables for single adult household, employed household, nonwhite household, and homeowner; fixed effects for the five year age grouping of oldest adult; fixed effects for government region; and, a linear time trend. Reduced form estimates are regressions of the outcome on the instrument (i.e. the difference-in-differences estimator). All estimates are performed using OLS. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Table 9. Comparison of Pre- and Post-Mandate Mean Outcomes for Child Benefit Recipient Families, by Payment Period

Outcome	Weekly Recipients			Monthly Recipients			Raw Difference-in-Differences (7)
	Pre-Mandate (1)	Post-Mandate (2)	Difference (3)	Pre-Mandate (4)	Post-Mandate (5)	Difference (6)	
Credit Card	0.356 (0.479)	0.422 (0.494)	0.065 (0.020)	0.647 (0.478)	0.641 (0.479)	-0.006 (0.021)	0.071** (0.029)
Loan in Last Year	0.129 (0.362)	0.131 (0.338)	0.012 (0.013)	0.195 (0.405)	0.170 (0.376)	-0.021 (0.017)	0.032 (0.021)
Number of Loans	0.323 (0.627)	0.341 (0.636)	0.017 (0.026)	0.506 (0.703)	0.432 (0.650)	-0.074 (0.029)	0.091** (0.039)
Weekly Loan Payment	9.816 (23.750)	12.530 (29.182)	2.714 (1.170)	19.380 (31.177)	16.758 (29.478)	-2.622 (1.284)	5.336*** (1.738)
Own at Least One Vehicle	0.594 (0.491)	0.642 (0.480)	0.049 (0.019)	0.856 (0.351)	0.847 (0.360)	-0.009 (0.015)	0.057** (0.025)
Vehicle Purchase	0.208 (0.406)	0.206 (0.405)	-0.002 (0.016)	0.293 (0.455)	0.288 (0.453)	-0.005 (0.020)	0.003 (0.026)
Household Appliances	4.990 (1.172)	5.607 (0.998)	0.617 (0.043)	5.524 (1.051)	5.930 (0.952)	0.407 (0.043)	0.210*** (0.061)
Weekly Expenditures	197.612 (146.972) [162.308]	189.771 (145.775) [156.431]	-7.841 (6.057) [-1.579] (6.157)	266.925 (162.891) [238.350]	249.415 (171.519) [211.907]	-17.510 (7.471) [-27.390] (6,752)	9.670 (9.617) [25.811***] (9.628)
Observations	1,863	1,036		1,926	791		5,616

Note: Author's calculations using EFS data from 1999-2003, 2005-2008. All reported means are weighted and standard deviations are in parenthesis. Number of loans in the last year and weekly loan payments are unconditional means. Median values are in square brackets. Median regression is also performed for the raw difference-in-difference estimates for weekly expenditures and the reported standard errors are calculated with 999 bootstrap replications. For the raw difference-in-difference column, the asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.