

Do Bank Accounts Help Families to Save?  
Evidence from the United Kingdom

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Abstract

I use an electronic transfer mandate in the UK Child Benefit program, a transfer received by virtually all families with children, to estimate the effect of bank account ownership on the savings behavior of less educated families with children. With the mandate increasing account ownership by as much as 17 percent for less educated families with children, it provides an exogenous increase in account ownership to examine the causal effect of bank account ownership on the accumulation of financial assets. When a less educated family becomes an owner of a bank account, I find that bank account ownership increases the probability that a family owns at least £10 of financial assets. I also find evidence that ownership increases financial assets in the upper tail of the distribution. These results, however, are not robust to the definition of less educated families. Overall, my findings suggest that there is substantial heterogeneity in the savings response to owning a bank account and that it is unlikely that requiring lower income families to own a bank account will substantially increase their level of savings.

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## I. Introduction

Before technological advancements, governments throughout the world distributed public transfers via coupon book or check. Now, however, governments can pay benefits electronically into a recipient's bank account or onto a stored value card, eliminating coupons and checks. Despite a trend towards electronic administration, we do not have a clear understanding of how it may affect the unbanked, that is, families that own neither a transaction nor a savings account. In particular, for unbanked families, a requirement that public transfers be paid electronically is an inducement to own an account and may alter other aspects of their financial behavior, as well.

The related normative question is whether governments *should* encourage bank account ownership. Some in government suggest that account ownership is below the socially optimal level and have implemented policies to encourage account ownership, in part, because bank accounts are assumed to help the poor to save. There is, however, little evidence to support this claim. If account ownership does aid in asset accumulation, bank accounts may improve the economic well-being of their owners by providing resources to smooth income shocks, invest in human and physical capital, and finance retirement. Knowing if bank accounts affect savings 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 UK 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 compare Child Benefit recipients across time to estimate the magnitude of the mandate's effects on ownership of accounts eligible to receive an electronic transfer of Child Benefits. Employing a quasi-experimental design, which compares the behavioral response of less educated families with children to less educated families without children, I find that the mandate substantially boosted bank account ownership, which I define as either ownership of

a transaction account and/or savings account. Families with children increased their ownership of bank accounts 4 to 11 percentage points (5 to 17 percent) as a result of the mandate.

This increase in bank account ownership provides a new approach to investigate the causal relationship between bank accounts and savings. Previous work examining this relationship relies almost exclusively on studying voluntary participants in asset building programs and suffers from sample selection bias. The implementation of electronic transfer for recipients of the nearly-universal Child Benefit ensures that unobserved preferences for savings are uncorrelated with program participation. With an instrumental variable approach, I measure the effect of owning a bank account on the level of financial assets. After three years in which the mandate was fully implemented, I find account ownership increases the probability of owning at least £10 of financial assets. I also find evidence that bank account ownership increases financial assets above the 77<sup>th</sup> percentile for this sample which suggests substantial heterogeneity in the response to bank account ownership. However, these results are not robust to the definition of less educated families. I conclude that there is no robust evidence that owning a bank account has a causal effect on the accumulation of financial assets.

The paper proceeds as follows: Section II provides institutional details on the UK 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 UK Child Benefit*

The Child Benefit is a tax-free, universal benefit for families with children.<sup>1</sup> Benefits depend only on the number of eligible children in a family. Eligibility is defined as either a child

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<sup>1</sup> The government estimates take-up of the Child Benefit at 97 percent (House of Commons, 2006). My calculations of self-reported take-up replicate these estimates.

under the age of 16 or under the age of 19 and enrolled full-time in non-advanced education or an approved training program.<sup>2</sup> Families receive benefits monthly, although single parent and lower income families can choose to receive weekly payments.

Benefit levels are determined each fiscal year (April to March) with the release of the government's budget. 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 income source. Using the Family Resources Survey (FRS), a nationally representative survey of households, I compare Child Benefit receipts to family income in the 1999 to 2003 period. Child Benefits average six percent of income for median income families. For those at the lowest income decile, Child Benefits average 19 percent of income. With the importance of Child Benefits to the family 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>3</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 families that lack access to financial services, particularly banking services, but also affordable credit, savings opportunities, insurance, and financial advice.

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<sup>2</sup> Technically, a child remains eligible until August 31<sup>st</sup> after their 16<sup>th</sup> birthday if s/he does not continue into an education or training program.

<sup>3</sup> All government cash transfers were affected, including public pensions, means-tested benefits, unemployment benefits, veteran's benefits, disability benefits, and benefits for those with children. I focus on the Child Benefit because it has no income limitations and nearly full take-up.

The government phased in the mandate for Child Benefits between April 2003 and March 2005, ending the widely used order book (coupon book).<sup>4</sup> With an order book, recipients exchanged benefit coupons for cash at the Post Office. 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 into their 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.<sup>5</sup> Without providing information on their designated account to the Benefits Agency, claimants would not receive their benefits.<sup>6</sup> In effect, the mandate used the public benefit system to transition recipients into the banking system.

#### *Available Accounts*

Each of the accounts (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, 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

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<sup>4</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 (“New Benefits System ‘Poorly Promoted’”, BBC News, March 13, 2003). Recipients could also adapt to the new regime before they received a notification letter.

<sup>5</sup> The government allows payment into any bank, building society, or National Savings & Investments (NS&I) account that accepts Direct Payment (<http://www.hmrc.gov.uk/childbenefit/introduction.htm>).

<sup>6</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.

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; bill payment through direct debit; and, no fees for routine banking activities. Some banks offer Basic Bank Accounts that earn interest. The distinguishing feature of these accounts 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. Only available at the Post Office and intended for benefit recipients unable or unwilling to open a bank account, POCAs function much like the order books previously available. POCAs only allow benefit receipt over-the-counter at a Post Office and the balance in a POCA does not earn interest. Therefore, POCAs are not transaction accounts because POCAs do not provide the ability to pay bills or allow deposits. POCAs may, however, assist in acquiring familiarity with modern financial transactions and, eventually, transition owners into a bank account. For example, POCA owners can choose to withdraw only a portion of a benefit payment, rather than the entire sum, as was required with coupons and checks.

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>7</sup> I tabulate how families received their Child Benefits using FRS data in Table 1. Panel A displays the payment receipt methods in the pre-mandate period between 1999 and 2003. In this period, 56 percent of all recipients received their benefit via 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.

Table 1 also compares families at different education levels, with these levels defined either by age at school leaving of all adults in the family or attainment of recognized qualifications of all adults in the family. Panel A shows that 70 percent of families with adults who left full-time education at or before the age of 16 (less educated) versus 31 percent of families with

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<sup>7</sup> Conventional bank account owners may open a Basic Bank Account because they want lower cost banking services and do not value the additional services that conventional bank accounts provide. Conventional bank account owners may open a POCA because they want to receive benefits at a Post Office or, as anecdotal reports suggest, they want to separate Child Benefit income from other sources of income (BMRB Social Research, 2006).

adults who left full-time education at or after the age of 19 (more educated) chose the coupon payment method. Even starker contrasts exist when I define these levels by attainment of recognized qualifications. More than 83 percent of families with adults that lack any recognized educational or vocational qualification (less educated) chose the coupon payment method whereas only 27 percent of families with adults possessing at least degree-level qualifications (equivalent to a college degree) chose the coupon payment method.

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, nearly all families (98 percent) report electronic receipt after the mandate's implementation.<sup>8</sup> Differences in payment method by education level also nearly disappear after the mandate.

### III. Conceptual Background

In 2003, at least 10 percent of families in the US and the UK were unbanked (Aizcorbe et al., 2003; Treasury Committee House of Commons, 2006). Tabulations of UK families from the FRS in the pre-mandate period suggest that the choice to forgo bank account ownership is correlated with financial hardship. Unbanked families are much less likely to work and, conditional on working, earn substantially less. The unbanked also have far lower levels of mean and median financial assets than banked families. The unbanked are more likely to be single, nonwhite, and lower 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 US and the UK (Barr, 2004; Carbo et al., 2007; Dunham, 2001; GAO, 2002; Hogarth and O'Donnell 1997; Hogarth, et al., 2004; Klawitter and Fletschner, 2006; Treasury Committee, 2006; Vermilyea and Wilcox, 2002).

The UK government justified the electronic transfer policy and its interest in Basic Bank Accounts 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

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<sup>8</sup> Those that do not report receiving an electronic transfer may arise from misreporting or receiving an extreme hardship waiver. Additionally, some recipients could still be using a coupon book because the last coupon books issued contained coupons valid until July 2005.

arises from asymmetric information between banks and consumers. Banks may expect that applicants with low current income, poor credit history, or previous problems managing a bank 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 families 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 prevent banks from offering these services (Barr, 2004). Some consumers may not apply for an account because they assume they will not qualify for one or they may not know about the availability of appropriate accounts. Minimum balance requirements and banking fees, such as those incurred for over withdrawal, 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. The unbanked may also lack the financial education to fully understand the benefits of account ownership. Finally, a behavioral perspective posits even more reasons for families to forgo account ownership (Bertrand et al., 2006).

Bank account ownership may be less than socially optimal because accounts could produce positive externalities. Ownership allows electronic payment of public transfers, which reduces administrative costs and benefit fraud (Barr, 2004; HM Treasury, 2004).<sup>9</sup> Ownership may further policy goals if the costs incurred by the unbanked in converting benefit and wage payments into cash diminish the value of incentives embodied in public transfers and take-home pay (Barr 2004; HM Treasury, 2004). Moreover, if bank accounts have a causal effect on asset accumulation, account ownership could reduce dependence on the social safety net by providing resources to cope with unforeseen hardships and fund retirement.

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<sup>9</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).

To capture these perceived externalities and meet its equity objectives, the UK enacted two related policies. First, they stimulated demand for bank accounts by implementing an electronic transfer requirement for all public benefits. They also increased the supply of low-cost accounts 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. In sum, these two policies should affect both the supply of and demand for bank accounts in the UK.

In general, policies to increase bank account ownership are not adequately studied to determine their effectiveness, either in transitioning families into account ownership or determining the effect ownership has on other economic behavior. To my knowledge, only Washington (2006) considers such a policy using a large, cross-sectional sample. Washington examines US state 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 delay does not indicate that other factors better explain the behavioral response, her work suggests that demand responds slowly, as consumers adapt to new account offerings. In the context of the UK, her work indicates that the creation of Basic Bank Accounts should not have large effects on the banking behavior of unbanked consumers in the short term.

While the mandate should have large effects on transitioning unbanked families with children 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 encourage asset accumulation. The government tried to tackle the upward trend in the portion of the population without financial assets – nearly 10 percent of the population in 1999 – with several other policies (Banks and Tanner, 1999). For example, the government encouraged asset accumulation through the Child Trust Fund, as well as expanded tax-advantaged savings programs.<sup>10</sup> Still, particularly among those with lower incomes, many families have very little savings and face high costs in accessing credit (HM Treasury, 2004).

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<sup>10</sup> Launched in 2005, the CTF is a publicly funded asset for children born after August 31, 2002. The CTF provides an initial public endowment of £250 or £500, depending on family income. This endowment, any additional public or private contributions, and interest grow tax-free. The CTF is neither owned nor accessible by

Despite little empirical work on the subject, fairly consistent correlations exist between bank account ownership and savings behavior. Although there are conjectures that bank accounts directly affect savings behavior, account ownership may actually only provide access to the financial system or represent unobservable preferences for savings or financial literacy (Carney and Gale, 2001). The only research studying the link between account ownership and savings behavior to overcome the potential endogeneity of bank account ownership is by Aportela (1999). Aportela examines a natural experiment in Mexico where bank branches opened in the post offices of several towns and offered new low-cost savings options. Comparing towns affected by the expansion to towns that were not, he concludes that these new options increased the average household saving rate by as much as 8 percent. However, he cannot rule out that new savings in the formal financial system was not a substitute for informal savings instruments, such as the purchase of durable goods.

Other research examining the link between bank accounts and savings largely consists of evaluating volunteers in small scale pilot programs, and, even amongst these likely motivated “savers”, no consensus emerges about the effect of account ownership on savings (Beverly, et al., 2004; Barr, 2004; Grinstein-Weiss, et al., 2006; Linnenbrink, et al., 2006; Schreiner, et al., 2002). In sum, there is no clear evidence of a causal relationship between owning a bank account and level of financial assets. This may result from the absence of a relationship, a lack of independent variation, or the focus on the formal financial system to the exclusion of informal savings instruments.

#### IV. Methodology

In principle, the decision to own a bank account is likely related to unobservable preferences for savings. Therefore, studying the savings decision without correcting for the potential endogeneity of the ownership decision may lead to estimates that are biased by these unobservable preferences. My estimation strategy involves using the exogenous increase in account ownership created by the electronic transfer mandate in the Child Benefit program to estimate the causal effect of account ownership on savings. This strategy disentangles

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the parent. Withdrawals occur only when the child turns 18 at which time the balance is available for any purpose the child chooses.

unobservable preferences for savings from unobservable preferences for program participation.

I adopt an instrumental variable approach by estimating two-stage least squares (2SLS) and IV quantile regressions. In the first stage, I estimate a difference-in-differences specification 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 in the Child Benefit program. Although all families had access to the newly available Basic Bank Accounts, the exposure of families with children to the mandate in the Child Benefit program results in isolating the effect of the mandate on account ownership.<sup>11</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 family income for lower income families. Contrasting the response of less educated families with children to those without children allows comparisons of groups that face similar labor market and financial environments.

In addition to assumptions about the comparability of families with children and without children, identification of the first stage also requires that no contemporaneous shock differentially affected the account ownership choices of 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). This series of 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>11</sup> Families without children could be exposed to the mandate if they received other public benefit programs. I do not control for participation in 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 30 percent of my sample of childless families with no qualifications receive at least one the three largest benefit programs (Disability Living Allowances, Incapacity Benefit, and Income Support) also subject to the mandate. The figure is 15 percent for the sample of childless families with minimum schooling.

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 UK require direct deposit of wages (Collard, 2007). Additionally, supplementary credit income could 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>12</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 estimation uses the increase in account ownership caused by the mandate to identify the effect of bank account ownership on financial asset accumulation. The mandate meets the two tests of a suitable instrument: the mandate was orthogonal to a family's preferences for savings and it increased bank account ownership for less educated families with children. In using variation arising from the mandate, I identify the effect of bank account ownership on the savings 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:

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

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

where *KIDS* is a dichotomous variable 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

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<sup>12</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 the labor force participation of lower income childless workers by 2 to 3 percentage points.

for identification, I exclude the difference-in-differences variable, *KIDS\*POST*, from the second stage equation.

## V. Data

I use the FRS, a cross-sectional, nationally representative survey of the UK population conducted each fiscal year (April through March).<sup>13</sup> The FRS provides detailed demographic, income, account, and financial asset information. I choose surveys from the pre-mandate (1999-2003) and post-mandate (2005-2008) period, excluding observations from the phase-in period of the mandate (2003-2005).<sup>14</sup> I limit my sample by dropping families where the oldest adult is at least 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 there until the 2002-2003 survey year. I adjust all income, benefit, and asset values to constant 2005 pounds using the Retail Price Index (RPI).

I focus my analysis on less educated families. I begin by creating a sample of less educated families defined by attainment of recognized qualifications. This definition, “No Qualifications”, designates families where all adults in the family lack any educational or vocational credentials. To ensure that my results are not driven by my sample selection, I also create a second sample of less educated families based on age at school leaving. The second definition, “Minimum Schooling”, designates families where all adults report leaving full-time education at (or before) the current minimum schooling age 16.<sup>15</sup> The “No Qualifications” definition is a smaller sample of families that includes only those with the lowest level of job market skills.

I categorize families as bank account owners if any adult owns a transaction and/or savings account. Transaction accounts include Basic Bank Accounts and other conventional bank

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<sup>13</sup> The survey period begins in April of each year and continues through March. Therefore, the 1999-2000 survey begins in April 1999 and continues through March 2000.

<sup>14</sup> In addition to not observing when families were subject to the mandate, I also drop the phase-in period of the mandate (2003-2005) because the FRS samples in these years do not differentiate between ownership of a Basic Bank Account and a POCA. Therefore, I cannot compute my first-stage equation for these years.

<sup>15</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.

accounts that are meant for daily use. Savings accounts do not include savings bonds, stocks, or accounts that require a maturation period. POCAs are not considered a bank account.

I measure savings with the valuation of a family's total financial assets, created by the Department for Work and Pensions (DPW). For families that initially estimate their savings and investments at more than £1,500, the total financial assets equals the total actual value of their assets.<sup>16</sup> For those that initially estimate their savings and investments at less than £1,500 or refuse to provide an estimate, DPW assigns a value based on the interest earned in the family's accounts. Because asset levels are so low amongst my less educated samples, in addition to creating a dichotomous variable *Estimate1500* that represents whether or not a family initially estimates their savings and investments at more than £1,500, I create two indicator variables: *Asset10* and *Asset100* which represent families with at least £10 and at least £100 (equivalent to \$19USD and \$192USD) of financial assets, respectively. I also use the continuous measure of financial assets with the variable *Assets*. Finally, I create *LogAssets*, which is the natural logarithm of *Assets*. To ensure that the natural logarithm exists and it is non-negative, I assign families with financial assets of less than £1, a value of £1 before taking the natural logarithm.<sup>17</sup>

I present pre-period observable characteristics for my samples in Table 2. The sample with "No Qualifications" 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 (43 percent versus 53 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 single than families without children (61 percent versus 85 percent), either because families with children are married (82

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<sup>16</sup> Only families that estimate the value of their financial assets at between £1,500 and £20,000 have detailed data collected on their assets. The top censoring at £20,000 is less of a concern among the less educated population I focus because these groups are considerably less likely than the general population to own assets of this value.

<sup>17</sup> This decision rule is binding 74% of families with no qualifications and 51% of families with minimum schooling.

percent of non-single parents) or cohabitating.<sup>18</sup> Higher cohabitation and marriage rates explain the larger earned income of working families with children, but the difference is economically small – only £20 more per week. Owing to demographic trends in fertility, parents are 7 percentage points more likely to be nonwhite, as well as either younger (under 25) or older (age 50 to 55) than those without children. Finally, families with children have much lower rates of homeownership than families without children (28 percent versus 47 percent) but this is primarily driven by the very low rates (6 percent) for parents under age 25.

Columns 3 and 4 of Table 2 reports similar characteristics of the sample with “Minimum Schooling”. Similar to differences in observables between types of families without qualifications, families with and without children with minimum levels of schooling are statistically different in observable ways that are explained by the presence of children. The differences are, however, smaller.

I show the pre-period outcomes for my sample in Table 3. The low rates of bank account ownership – 62 percent and 84 percent of families without qualifications and minimum schooling, respectively – in the first row suggest there is a potential for a behavioral response to the mandate. Transaction account ownership, shown in the second row, is 6 to 8 percentage points lower, indicating that some families in the pre-period chose to own only a savings account. Comparing across types of families, account ownership rates for those without children are only a few points higher than those with children.

Pre-period financial asset values are also given in Columns 1 through 4 of Table 3. As shown in Table 3, these families have low levels of financial assets. Few families have detailed asset information collected because they do not estimate their savings and investments are worth more than £1,500. Among families without qualifications (Columns 1 and 2), those with children have less financial assets than those with children: only 20 percent of families with children have at least £10 of financial assets, while 31 percent of families without children

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<sup>18</sup> Adults in the UK do not have to be married to be treated as a family under the benefit system. The FRS provides 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 the Child Benefit, as well as other public benefits, I use this benefit unit definition to construct my sample families.

have at least £10 of financial assets. The figures are 15 percent and 25 percent for at least £100 of financial assets amongst families with and without children, respectively. Finally, those without children have £559 higher mean assets and 0.9 logarithmic points more *LogAssets*, although the median level is zero for both types of families which indicates that the distribution is right skewed.

Families with minimum levels of schooling have more financial assets, but similar differences between families with and without children appear in Columns 3 and 4 of Table 3. Less than half of families with children have at least £10 in financial assets and only slightly more than half (52 percent) of families without children has at least £10 in financial assets. Families without children have higher mean assets and *LogAssets* than those with children but the difference is smaller than for families without qualifications. Still, families without children are the only subsample to have a non-zero level of median assets.

#### IV. Results

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

I first estimate how the mandate affected ownership of 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 of 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 Credit, WFTC, or WTC that a family could receive based on demographic characteristics; categorical variables for an employed adult in the family, single adult families, nonwhite families, 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 standard errors are clustered on children and time (Bertrand et al., 2004).

I begin with raw difference-in-differences, reported in Table 4, for families without any qualifications. Ownership of bank accounts increased by 12 percentage points for families with children, compared to families without children (Panel A). Moreover, this increase appears to be driven by families choosing Basic Bank Accounts: ownership of transaction accounts also increased by 12 percentage points (Panel B). With pre-period bank account ownership rates of 62 percent, these raw differences indicate an increase in bank account ownership of 19 percent.

Raw difference-in-difference estimates for families with minimum level of schooling in Panels C and D of Table 4, show a similar pattern of changes in account ownership. Raw differences for this sample indicate an increase in bank account ownership of 6 percent.

I control for observable characteristics with a linear probability model.<sup>19</sup> These results, shown in Table 5, differ only slightly from the raw estimates, indicating that observable characteristics have limited explanatory power in the decision to own a bank account. For the “No Qualifications” sample, the probability of owning a bank account increases by 11 percentage points (17 percent) for families with children compared to those without children. As in the raw estimates, there is an 11 point (20 percent) relative increase in the propensity of owning a transaction account.

To check the robustness of this result, I consider families with “Minimum Schooling”. These estimates, also reported in Table 5, are roughly a third of the magnitude of those for families without any qualifications and suggest that account ownership increased 5 to 6 percent for families with children. A smaller response amongst this sample is expected because their higher pre-period rates of account ownership provide less opportunity for a behavioral response.

The large increases in the propensity to own bank accounts suggest that the mandate had large effects on banking behavior, particularly for families without any qualifications. Because the

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<sup>19</sup> In unreported results, I also estimated Equation 1 with a probit approach. The mean marginal effects from a probit approach are nearly identical.

difference-in-differences estimator compares relative responses, the 11 percentage point increase in transaction accounts compared to families without children (who could also own a new Basic Bank Account), indicates that Basic Bank Accounts and other transaction accounts were a more popular choice amongst Child Benefit recipients than families without children. This is evocative of Washington's 2006 conclusions: expanding the supply of low-cost bank accounts, such as the Basic Bank Account, is not enough to substantially boost account ownership. However, these results indicate that mandating the ownership of an account capable of receiving an electronic transfer while also offering low-cost banking options does increase bank account ownership.

#### *Robustness of the First Stage*

A potential source of bias in the first stage arises from the series of reforms to the Family Credit. The small and insignificant point estimate on the maximum credit variable may result 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 directed to families with children under the age of 11, relative to families with children age 11 to 15. If these reforms explain increases in account ownership, ownership should increase more for families with children under age 11 than for those with children age 11 to 15.<sup>20</sup> I re-estimate equation 1 for families with children under age 16, assigning families with children under the age of 11 to the treatment group and families with children age 11 to 15 to the control group. If the Family Credit reforms increased bank account ownership, the difference-in-difference 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. All point estimates are insignificant. Thus, families with

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<sup>20</sup> Gregg et al. consider not only the reforms to the Family Credit but also 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 young children. 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 clearly an outcome measure of many these reforms as they provided incentives for increased labor supply on both the intensive and extensive margin. Selecting the sample on income could bias my results by changing the composition of my treatment and control groups.

younger children do not appear to respond differently to the mandate than families with older children. The Family Credit reforms do not explain the increase in bank account ownership for families with children.

The first stage estimates show large, significant, and robust increases in account ownership arising from the mandate. This increase provides an exogenous change to identify the impact of bank account ownership on the accumulation of financial assets.

*Second Stage Estimates: Effects at the Mean*

Bank accounts are typically treated as a place to save money and build financial assets. To examine the effect of bank account ownership on savings, I estimate the effect of owning a bank account on each of four different financial asset measures: *Estimate1500*, *Asset10*, *Asset100*, and *LogAssets*. I begin with OLS and reduced form estimates before proceeding to 2SLS estimates.

Panel A of Table 7 provides parameter estimates for the sample of families with no educational qualifications. To provide a benchmark against which to measure the IV estimates, the first column provides OLS estimates of equation 2 which do not correct for the potential endogeneity of bank account ownership on savings. The effects are large, statistically significant, and economically large. Owning a bank account increases the probability of estimating savings and investments at more than £1,500 by 7 percentage points (110 percent), the probability of accumulating £10 of financial assets by 24 points (82 percent), and the probability of accumulating £100 of financial assets by 17 points (114 percent). Using a continuous measure of financial assets produces similar results: owning a bank account increases financial assets by 144 percent. Results for families with minimum levels of schooling (Panel B) suggest similarly large effects of bank account ownership on asset accumulation. These estimates are biased upwards if families that own a bank account have higher preferences for savings than families that do not own a bank account.

The second column of each panel of Table 7 provides reduced-form estimates. These results are shown because Angrist and Krueger (2001) argue that the lack of a reduced-form relationship implies a lack of relationship in the IV approach. Therefore, an insignificant

reduced-form estimate would provide evidence against an effect of account ownership on savings. Except for the *Asset10* measure in the second column of Panel A, the reduced-form point estimates on the dichotomous outcomes are not statistically different from zero. Moreover, the effects tend to be small in magnitude and, in fact, negative for families with minimum schooling. These reduced form estimates provide little evidence that there is a direct relationship between the implementation of the mandate and changes in financial asset levels for those with children. The mandate is only associated with a three percentage point increase in the probability that families without any qualifications own at least £10 of financial assets.

With these initial results, I address the potential endogeneity of bank account ownership with the 2SLS approach of Equation 2 in the final column of Table 7. Taken as a whole, the IV estimates for each outcome show that, after removing the endogeneity of bank account ownership, owning a bank account is not significantly related to an increase in these financial asset measures.<sup>21</sup>

Focusing first on the results for families without any qualifications in Panel A, only the probability of owning at least £10 of financial assets is statistically significant. The estimate suggests that owning a bank account increases the probability of having £10 of financial assets by 30 percentage points (152 percent). This is a very large effect for these families and provides evidence that owning a bank account may assist in accumulating an extremely modest level of financial assets. For other financial asset measures, the point estimates are positive but insignificant. Interestingly, the 2SLS point estimates are all larger than the OLS estimates which suggests that the OLS estimates are biased downwards. The confidence interval for each 2SLS estimate, however, is quite large.

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<sup>21</sup> Results from bivariate probit estimation with 999 bootstrap replications for the standard errors are larger than the results with the endogenous dependent variable and very significant. Bivariate probit results suggest that that account ownership increased the propensity of owning financial assets of £10 and £100 by 26 percentage points and 38 percentage points, respectively, for low educated families with “No Qualifications.” Estimates for low educated families with “Minimum Schooling” are nearly identical – bank account ownership increased the propensity of owning at least £10 and £100 of financial assets by 28 and 37 percentage points, respectively. With reduced form estimates suggesting no effect on behavior and the suggestions of Angrist (2001), I focus on the linear estimates.

I check the robustness of this result with the second sample of less educated families in Panel B of Table 7. In contrast to the results in Panel A, the 2SLS estimates in Column 3 of Panel B confirm the intuition that the OLS results in Column 1 are upward biased. As in the reduced form estimates, the point estimates for families with minimum levels of schooling are negative and no estimate approaches statistical significance. For all outcomes in this sample, there is no evidence that owning a bank account has any effect on a family's financial assets.

The results presented thus far are focused on estimates through the sample means. While the lowest educated sample shows a positive effect of owning a bank account on £10 of savings, there is no other evidence to support the hypothesis that owning a bank account increases financial assets. The mean, however, may not capture all the interesting or important effects of account ownership on financial asset accumulation. The next set of results considers effects in other portions of the distribution.

#### *Second Stage Estimates: Effects Throughout the Distribution*

As in other studies of savings behavior, there is likely substantial heterogeneity in the savings response among families that transition into bank account ownership.<sup>22</sup> I examine this heterogeneity with reduced-form quantile and IV quantile estimates using the *Asset* outcome (Chernozhukov and Hansen, 2006 and 2008). The quantile estimates do not include additional covariates due to difficulties in achieving convergence.<sup>23</sup>

I begin with reduced-form quantile estimates for families with no qualifications. I plot the difference-in-difference estimator (solid line) and the 90 percent confidence interval (dashed lines) at the 5<sup>th</sup> through the 95<sup>th</sup> quantile in Figure 1a. Although the mandate had no effect on much of the financial asset distribution, it is associated with an increase in assets above the 70<sup>th</sup> quantile. Moreover, the estimate is economically large: the mandate is associated with an increase of up to £2,700 in financial assets. This increase provides initial evidence that the increase in account ownership will also increase savings for some families.

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<sup>22</sup> Although the IV approach ensures that preferences for saving are uncorrelated with preferences for participation in the Child Benefit program, it is almost certainly true that previously unbanked families that were induced to own a bank account because of the mandate are not homogeneous in their preferences for savings, their ability to save, or their response to account ownership.

<sup>23</sup> This is potentially an important limitation of these results. My IV estimates suggest that observable characteristics, particularly income and homeownership, are important for explaining a family's savings decision.

Figure 1b provides the IV quantile estimates for families without any qualifications. As in the reduced form quantile estimates, the IV quantile estimates show that bank account ownership has no effect on the lower tail of the distribution of financial assets. However, bank account ownership leads to an increase in financial assets above the 77<sup>th</sup> percentile. The effect of owning a bank account on financial assets is substantial at the upper end of the distribution – the point estimate at the 95<sup>th</sup> percentile is nearly £6,100. The contrast between this estimate and the zero effect for most of the distribution reflects the substantial heterogeneity in the response to owning a bank account.

I check the robustness of this result by performing reduced-form quantile and IV quantile estimates for the sample of families with minimum schooling in Figure 2a and 2b. For this sample, the reduced-form quantile estimates in Figure 2a show that the mandate is associated with an increase in assets above the median. Thus, a large portion of families experienced at least some increase in financial assets after the mandate. Moreover, the upper quantiles experienced an economically large increase: nearly £2,000 at the 95<sup>th</sup> percentile.

The IV quantile estimates are plotted in Figure 2b for families with minimum schooling. These IV quantile estimates are small and very imprecise. While the point estimates in the middle and upper quantiles are positive, the standard errors are so large that these estimates are not significantly different from zero. Overall, despite some measurable changes in the reduced-form estimates, IV quantile estimates provide no evidence that owning a bank account affects the accumulation of financial assets in any portion of the financial asset distribution.

As in the linear estimates, these quantile estimates provide mixed evidence of the effect of bank accounts on financial assets: for the least well-off families with children, there appears to be some positive effects of account ownership on savings, whereas for the larger sample of lower educated families, there is no effect. Several sources of measurement problems could present problems with achieving robust results. First, any changes in account values may be difficult to measure in cross-sectional data because of the low levels of financial assets that

low-income families own, regardless if they own a bank account.<sup>24</sup> In addition, for much of my sample, the financial asset values are an assigned value based on the interest collected in their accounts. If either the interest earned is misreported or if accounts do not earn interest, it will be difficult to calculate a discernible change in financial assets.<sup>25</sup> In fact, some Basic Bank Accounts do not earn interest, which may make it impossible to measure changes in financial assets until families with Basic Bank Accounts accumulate enough savings to value the account at £1,500 or more.

Finally, due to difficulties in achieving convergence, the quantile estimates were generated without including additional covariates in the equation. It is likely that these omitted variables are important in explaining the decision to save. By not including these covariates, it may be difficult to find evidence linking bank account ownership to increases in financial assets.

## VI: Conclusion

In this paper, I use a new approach to identify the effect of bank account ownership on savings. I first show that an electronic transfer mandate in the UK had large effects on account ownership. Using this exogenous change in bank account ownership, I find that owning a bank account increases financial assets only for the least educated families I consider. For these families, account ownership increases the probability that a family will report owning at least £10 of financial asset. Ownership also affects saving for those with higher preferences for savings.

Three years after all recipients were subject to the mandate, I do not find robust evidence that owning a bank account increases the financial assets of less educated families. Because increasing financial assets was one of the mandate's goals, the mandate is of limited success. This conclusion however, has a number of caveats, including difficulties measuring the financial assets of lower income families, the data collection methods in the FRS, and the lack of included covariates.

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<sup>24</sup> These low levels of financial assets may arise because low incomes increase the opportunity cost of foregone consumption and low-education levels may lead to little financial education to understand the importance of saving.

<sup>25</sup> The FRS does not ask if the account is interest bearing. Therefore, it is difficult to discern if the bank account earns interest on deposits or if no monies are held in the account to generate interest. These problems suggest that the structure of the FRS questionnaire should be changed to collect detailed account values on all respondents so that savings levels of lower income families can be more accurately measured.

Taken as a whole, these findings provide little evidence for the notion that the poor would save more if only they had a bank account.<sup>26</sup> Families with children may favor current period consumption over future consumption. These families could also use informal savings mechanisms such as purchasing durable goods. In some contexts including the stock of durable goods is useful for measuring household savings (Browning and Lusardi, 1996). In such a context, Fitzpatrick's (2009) finding of an increase in household appliances when families in the UK transitioned into account ownership could be a measure of increased household savings. Interpreting this finding as informal savings is consistent with evidence from the US suggests low- and middle-income families save by choosing the lump sum Earned Income Tax Credit (EITC) payment (rather than the advanced EITC) and, when they receive their income tax refund, purchase a durable good (Edwards, 2004; Goodman-Bacon and McLanahan, 2008; Romich and Weisner, 2000; Smeeding, et al., 2000; Smeeding, 2002).

Despite the lack of evidence for savings, the UK's experience with the electronic transfer mandate is useful. The large increase in bank account ownership – up to a 17 percent increase – can inform US policymakers interested in transitioning the unbanked into the financial mainstream. EFT'99, the US law mandating electronic transfer for federal benefits, envisioned a mandate similar to the UK's mandate, complemented by the provision of low-cost bank accounts (Stegman, 1999). The Treasury's rulemaking process, however, made electronic transfer voluntary. As a result, recipients must opt-in to electronic transfer in large federal benefit programs, such as Social Security and the EITC. While the federal government requires electronic benefit transfer (EBT) in Food Stamps and Temporary Assistance to Needy Families (TANF), EBT largely consists of a debit card only capable of accessing benefits, much like the UK's POCA. Additionally, EBT is not coupled with the provision of a low-cost bank account for recipients and banked recipients cannot always receive an electronic transfer into their bank account (Barr, 2004).

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<sup>26</sup> Work in the US 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 UK maintains asset tests in their cash welfare program, Income Support, but the asset limits are so high (£16,000) that few potential applicants are affected. Thus, means-tested programs in the UK would not explain the decision to save through durable good purchase rather than financial assets.

If the US enacts a binding electronic transfer mandate, while also providing the option of a low-cost bank account appropriate for lower income populations, unbanked recipients will transition into bank account ownership. The effects on account ownership in the US could be smaller than the effects in the UK. Unlike the UK Child Benefit, most transfer programs in the US are means-tested and, therefore, recipients could be more sensitive to the transaction costs associated with compliance. Still, these means-tested programs could be a useful way to target such a policy at the unbanked. The motivation for such a policy, however, should not rest solely on the argument that it will help the poor to save because it is unlikely that owning a bank account will substantially change a family's financial assets.

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

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

|  | Coupon<br>Book | Bank | Check | Other |
|--|----------------|------|-------|-------|
| All Families   | 55.5           | 43.9 | 0.3   | 0.4   |
| Do Not Own A Bank Account                            | 96.0           | 2.2  | 1.0   | 0.8   |
| Own A Bank Account                                   | 51.5           | 48.0 | 0.2   | 0.3   |
| <u>Education Groups By Age at School Leaving</u>     |                |      |       |       |
| Less Educated  | 70.1           | 29.2 | 0.4   | 0.4   |
| More Educated  | 30.5           | 69.1 | 0.2   | 0.2   |
| <u>Education Groups by Recognized Qualifications</u> |                |      |       |       |
| Less Educated  | 83.4           | 15.3 | 0.8   | 0.6   |
| More Educated  | 27.1           | 72.4 | 0.1   | 0.3   |

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

|  | Coupon<br>Book | Bank<br>or POCA | Check | Other |
|--|----------------|-----------------|-------|-------|
| All Families   | 1.1            | 97.6            | 0.7   | 0.7   |
| Do Not Own A Bank Account                            | 10.0           | 77.3            | 9.4   | 3.3   |
| Own 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   |
| <u>Education Groups by Recognized Qualifications</u> |                |                 |       |       |
| Less Educated  | 2.8            | 94.4            | 1.8   | 1.0   |
| More Educated  | 0.5            | 98.6            | 0.1   | 0.8   |

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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 family 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. Observable Characteristics from the Pre-Mandate Period for Families With and Without Children, by Definition of Less educated Families

|   | <u>No Qualifications</u> |                         | <u>Minimum Schooling</u> |                         |
|---|--------------------------|-------------------------|--------------------------|-------------------------|
|   | With Children<br>(1)     | Without Children<br>(2) | With Children<br>(3)     | Without Children<br>(4) |
| Working Families                        | 0.429<br>(0.495)         | 0.532<br>(0.499)        | 0.725<br>(0.446)         | 0.758<br>(0.428)        |
| Net Earned Income, if Working           | 2.69<br>(3.880)          | 2.51<br>(3.950)         | 3.92<br>(5.690)          | 3.25<br>(4.350)         |
| Weekly Hours of Main Earner, if Working | 34.88<br>(15.650)        | 40.53<br>(12.080)       | 40.51<br>(14.860)        | 42.33<br>(11.580)       |
| Single Adult                            | 0.609<br>(0.488)         | 0.847<br>(0.360)        | 0.373<br>(0.484)         | 0.723<br>(0.447)        |
| Nonwhite                                | 0.171<br>(0.377)         | 0.100<br>(0.300)        | 0.071<br>(0.257)         | 0.052<br>(0.223)        |
| Homeowner                               | 0.279<br>(0.449)         | 0.466<br>(0.500)        | 0.545<br>(0.498)         | 0.63<br>(0.483)         |
| Number of Children                      | 1.98<br>(1.080)          | -                       | 1.85<br>(0.931)          | -                       |
| Age 0 to 4                              | 0.466<br>(0.689)         |                         | 0.445<br>(0.658)         |                         |
| Age 5 to 10                             | 0.699<br>(0.820)         |                         | 0.641<br>(0.766)         |                         |
| Age 11 to 15                            | 0.656<br>(0.780)         |                         | 0.596<br>(0.742)         |                         |
| Age 16 to 18                            | 0.161<br>(0.404)         |                         | 0.164<br>(0.395)         |                         |
| Age Distribution of Oldest Adult        |                          |                         |                          |                         |
| Under 25                                | 9.58                     | 25.38                   | 6.12                     | 25.25                   |
| Age 25 to 30                            | 11.50                    | 8.13                    | 9.70                     | 10.00                   |
| Age 30 to 35                            | 17.53                    | 8.43                    | 18.19                    | 9.92                    |
| Age 35 to 40                            | 22.18                    | 9.01                    | 24.34                    | 9.27                    |
| Age 40 to 45                            | 19.72                    | 11.00                   | 21.59                    | 10.22                   |
| Age 45 to 50                            | 12.15                    | 15.28                   | 13.16                    | 13.91                   |
| Age 50 to 55                            | 7.34                     | 22.77                   | 6.92                     | 21.43                   |
| Observations                            | 3,946                    | 5,565                   | 14,977                   | 19,064                  |

Note: Author's calculations using FRS data from 1999-2003. All statistics are weighted. Net earned income and child benefit values are both adjusted to constant 2005 pounds. Earned income is reported in hundreds of pounds per week.

Table 3. Outcomes from the Pre-Mandate Period for Families With and Without Children, by Definition of Less educated Families

|                          | <u>No Qualifications</u> |                         | <u>Minimum Schooling</u> |                           |
|--------------------------|--------------------------|-------------------------|--------------------------|---------------------------|
|                          | With Children<br>(1)     | Without Children<br>(2) | With Children<br>(3)     | Without Children<br>(4)   |
| <u>Account Ownership</u> |                          |                         |                          |                           |
| Own Bank Account         | 0.623<br>(0.485)         | 0.664<br>(0.472)        | 0.843<br>(0.363)         | 0.859<br>(0.348)          |
| Own Transaction Account  | 0.541<br>(0.498)         | 0.580<br>(0.494)        | 0.788<br>(0.409)         | 0.804<br>(0.397)          |
| <u>Financial Assets</u>  |                          |                         |                          |                           |
| Estimate1500             | 0.065<br>(0.246)         | 0.140<br>(0.347)        | 0.212<br>(0.409)         | 0.265<br>(0.441)          |
| Assets                   | 1744.86<br>(34469.64)    | 2303.74<br>(17481.54)   | 4619.77<br>(77255.44)    | 5082.49<br>(48150.65)     |
| Log Assets               | [0]<br>1.23<br>(2.650)   | [0]<br>2.12<br>(3.370)  | [0]<br>3.23<br>(3.710)   | [24.17]<br>3.7<br>(3.890) |
| Asset10                  | 0.196<br>(0.399)         | 0.309<br>(0.462)        | 0.469<br>(0.499)         | 0.516<br>(0.500)          |
| Asset100                 | 0.145<br>(0.353)         | 0.254<br>(0.435)        | 0.388<br>(0.487)         | 0.439<br>(0.496)          |
| Observations             | 3,946                    | 5,565                   | 14,977                   | 19,064                    |

Note: Author's calculations using FRS data from 1999-2003 All statistics are weighted. Net earned income and child benefit values are both adjusted to constant 2005 pounds. Earned income is reported in hundreds of pounds per week. Median assets is reported in square brackets.

Table 4. Raw Difference-in-Difference Estimates of Impact of Mandate on Family Account Ownership, By Definition of Less Educated Families

|   | Pre-Mandate<br>(April 1999<br>- March 2003) | Post-Mandate<br>(April 2005<br>- March 2007) | Time Difference<br>for Groups |
|---|---|--|-------------------------------|
| <b>A. Bank Account Ownership for Families with No Educational Qualifications</b>        |   |  |                               |
| Families with Children  | 0.623<br>(0.007)                            | 0.864<br>(0.009)                             | 0.240<br>(0.012)              |
| Families without Children   | 0.664<br>(0.006)                            | 0.785<br>(0.007)                             | 0.121<br>(0.009)              |
| Group Difference at Point in Time   | -0.041<br>(0.010)                           | 0.079<br>(0.011)                             |                               |
|   |   | Difference-in-Difference                     | 0.119<br>(0.016)              |
| <b>B. Transaction Account Ownership for Families with No Educational Qualifications</b> |   |  |                               |
| Families with Children  | 0.541<br>(0.007)                            | 0.830<br>(0.010)                             | 0.289<br>(0.012)              |
| Families without Children   | 0.580<br>(0.006)                            | 0.746<br>(0.007)                             | 0.166<br>(0.010)              |
| Group Difference at Point in Time   | -0.039<br>(0.011)                           | 0.084<br>(0.012)                             |                               |
|   |   | Difference-in-Difference                     | 0.123<br>(0.017)              |
| <b>C. Bank Account Ownership for Families with Minimum Levels of Schooling</b>          |   |  |                               |
| Families with Children  | 0.843<br>(0.003)                            | 0.943<br>(0.003)                             | 0.100<br>(0.004)              |
| Families without Children   | 0.859<br>(0.002)                            | 0.909<br>(0.002)                             | 0.050<br>(0.004)              |
| Group Difference at Point in Time   | -0.016<br>(0.004)                           | 0.034<br>(0.004)                             |                               |
|   |   | Difference-in-Difference                     | 0.050<br>(0.006)              |
| <b>D. Transaction Account Ownership for Families with Minimum Levels of Schooling</b>   |   |  |                               |
| Families with Children  | 0.788<br>(0.003)                            | 0.926<br>(0.003)                             | 0.138<br>(0.005)              |
| Families without Children   | 0.804<br>(0.003)                            | 0.885<br>(0.003)                             | 0.081<br>(0.004)              |
| Group Difference at Point in Time   | -0.016<br>(0.004)                           | 0.041<br>(0.004)                             |                               |
|   |   | Difference-in-Difference                     | 0.057<br>(0.007)              |

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

Table 5. Linear Probability Estimates for Types of Account Ownership Types, By Definition of Less Educated Families

|   | <u>No Qualifications</u>         |  | <u>Minimum Schooling</u>         |  |
|---|----------------------------------|--|----------------------------------|--|
|   | Bank Account<br>Ownership<br>(1) | Transaction<br>Account<br>Ownership<br>(2) | Bank Account<br>Ownership<br>(3) | Transaction<br>Account<br>Ownership<br>(4) |
| Treatment Group: Families with Children     |                                  |  |                                  |  |
| Comparison Group: Families without Children |                                  |  |                                  |  |
| Kids  | -0.004<br>(0.014)                | 0.0001<br>(0.017)                          | -0.006<br>(0.008)                | -0.006<br>(0.011)                          |
| Post Period                                 | 0.096***<br>(0.008)              | 0.100***<br>(0.014)                        | 0.017*<br>(0.010)                | 0.025***<br>(0.008)                        |
| Kids*Post Period                            | 0.108***<br>(0.006)              | 0.110***<br>(0.001)                        | 0.039***<br>(0.008)              | 0.045***<br>(0.010)                        |
| Observations                                | 14,819                           | 14,819                                     | 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 the number of children and time. 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. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Table 6. Mean Marginal Effects for Types of Account Ownership for Families with Children, by Age of Children and Definition of Less Educated Families

|   | <u>No Qualifications</u>         |  | <u>Minimum Schooling</u>         |  |
|---|----------------------------------|--|----------------------------------|--|
|   | Bank Account<br>Ownership<br>(1) | Transaction<br>Account<br>Ownership<br>(2) | Bank Account<br>Ownership<br>(3) | Transaction<br>Account<br>Ownership<br>(4) |
| Treatment Group: Families with All Children Under the Age of 11 |                                  |  |                                  |  |
| Comparison Group: Families with All Children Aged 11 to 15      |                                  |  |                                  |  |
| Young Children  | 0.027***<br>(0.009)              | 0.028***<br>(0.011)                        | 0.015***<br>(0.003)              | 0.016***<br>(0.004)                        |
| Post Period   | 0.284***<br>(0.023)              | 0.300***<br>(0.031)                        | 0.090***<br>(0.009)              | 0.106***<br>(0.011)                        |
| Young Children*Post<br>Period                                   | -0.012<br>(0.020)                | -0.007<br>(0.015)                          | 0.011<br>(0.012)                 | 0.016<br>(0.014)                           |
| Observations  | 5,671                            | 5,671                                      | 21,903                           | 21,903                                     |

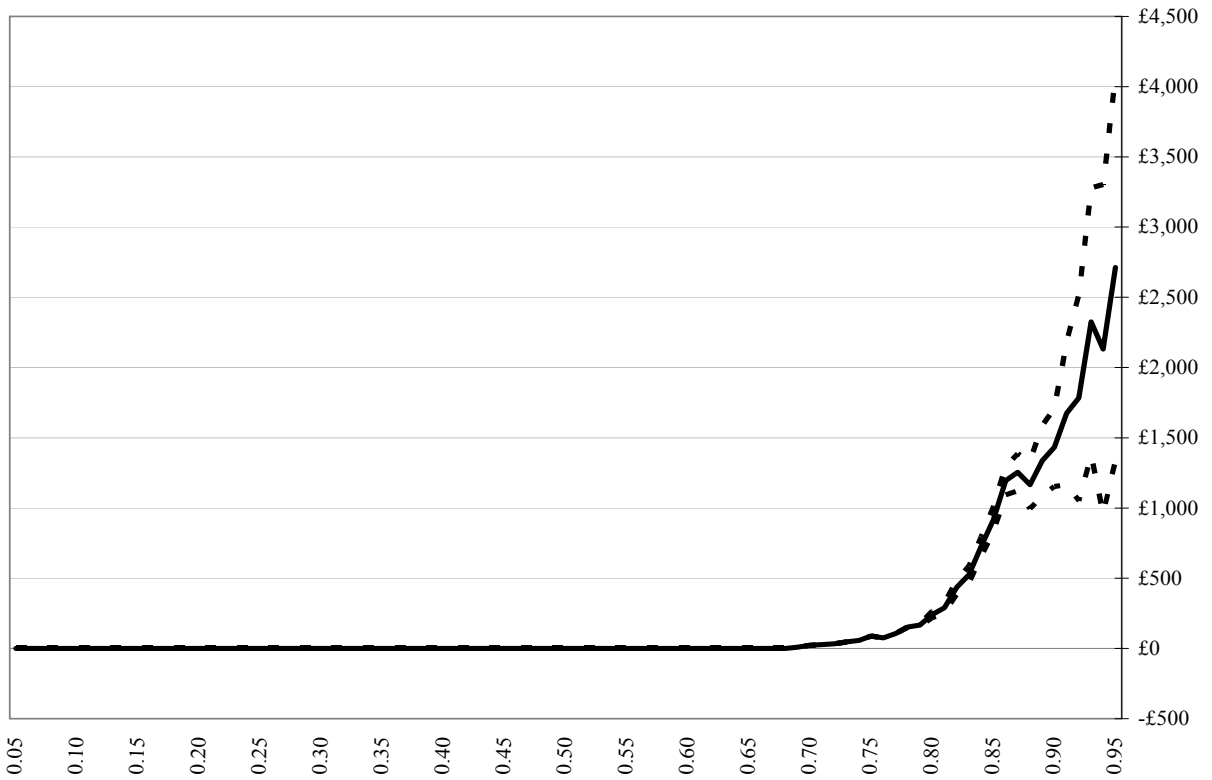
Note: Author's calculations using FRS data from 1999-2003 and 2005-2008. Estimates created with a probit specification and mean marginal effects are calculated using Jonah Gelbach's `margfx` stata routine. All regressions are weighted and standard errors clustered by the number of children and time. All regressions are weighted and standard errors clustered by the number of children and time. 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. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Table 7. Effect of Bank Account Ownership on Financial Assets, by Methodological Approach and Definition of Less Educated Family

|  | OLS                 | Reduced Form      | 2SLS              |
|--|---------------------|-------------------|-------------------|
|  | (1)                 | (2)               | (3)               |
| Panel A. Estimates for Families without Qualifications |                     |                   |                   |
| Estimate1500   | 0.067***<br>(0.010) | 0.018<br>(0.013)  | 0.178<br>(0.706)  |
| Asset10  | 0.239***<br>(0.020) | 0.032*<br>(0.019) | 0.297*<br>(0.174) |
| Asset100   | 0.166***<br>(0.019) | 0.025<br>(0.017)  | 0.233<br>(0.148)  |
| Log Assets   | 1.437***<br>(0.143) | 0.185<br>(0.126)  | 1.715<br>(1.138)  |
| Observations   | 14,819              | 14,819            | 14,819            |
| First Stage F Statistic                                |                     |                   | 43.296            |
| Panel B. Estimates for Families with Minimum Schooling |                     |                   |                   |
| Estimate1500   | 0.089***<br>(0.012) | -0.009<br>(0.013) | -0.243<br>(0.403) |
| Asset10  | 0.294***<br>(0.018) | -0.002<br>(0.014) | -0.042<br>(0.401) |
| Asset100   | 0.207***<br>(0.018) | -0.005<br>(0.018) | -0.120<br>(0.600) |
| Log Assets   | 1.805***<br>(0.137) | -0.038<br>(0.109) | -0.970<br>(3.255) |
| Observations   | 53,811              | 53,811            | 53,811            |
| First Stage F Statistic                                |                     |                   | 37.089            |

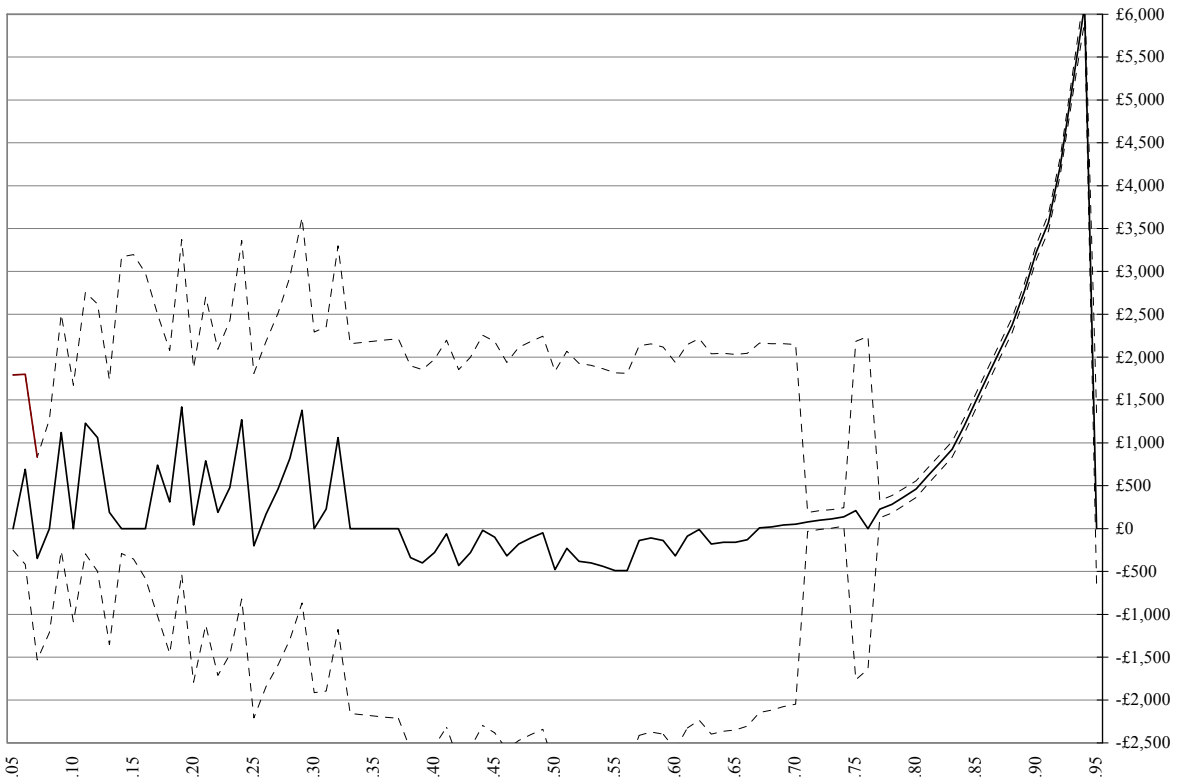
Note: Author's calculations using FRS data from 1999-2003 and 2005-2008. All regressions are weighted and standard errors clustered by number of children and time. Standard errors are bootstrapped with 999 replications drawn with replacement from each cluster. Other covariates include a quadratic in 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. Asterisks denote statistical significance as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Figure 1a: Reduced Form Quantile Estimates on Distribution of Financial Assets for Families With No Qualifications



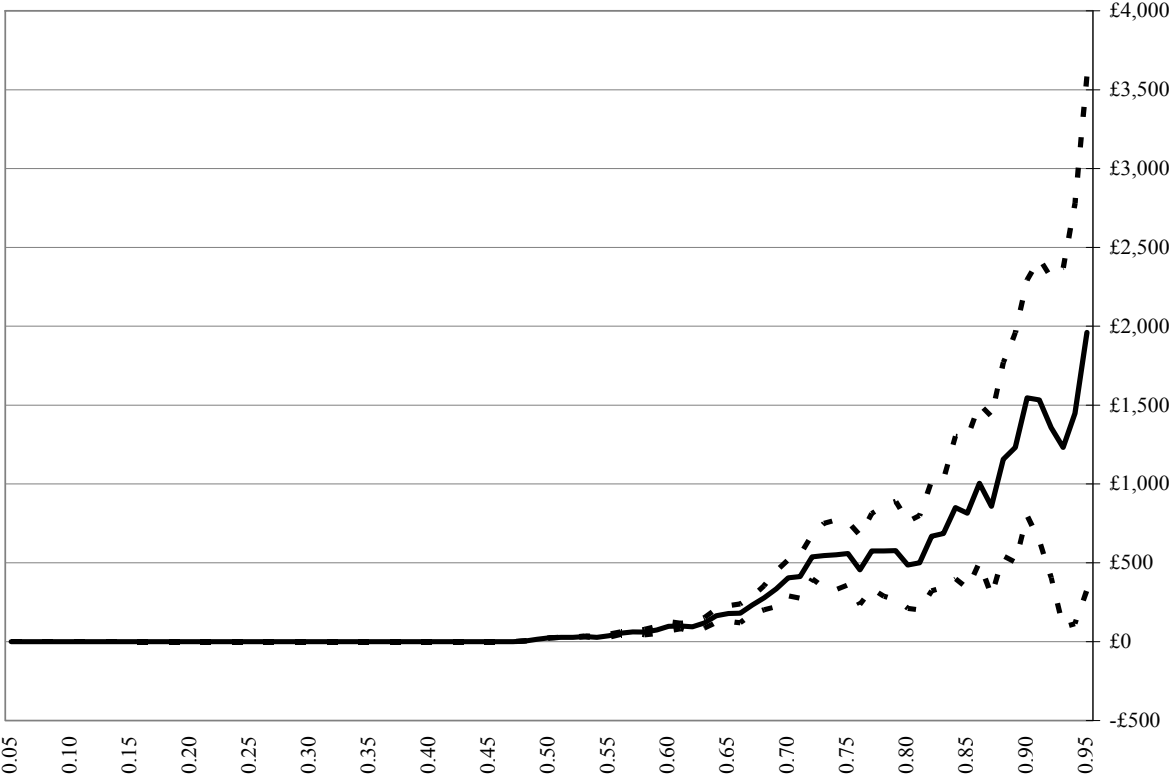
The solid line is the quantile estimate. The dashed line is the 90% confidence interval from a bootstrapped procedure with 999 replications. To ensure convergence, estimates do not include any covariates.

Figure 1b: IV Quantile Estimates on Distribution of Financial Assets for Families With No Qualifications



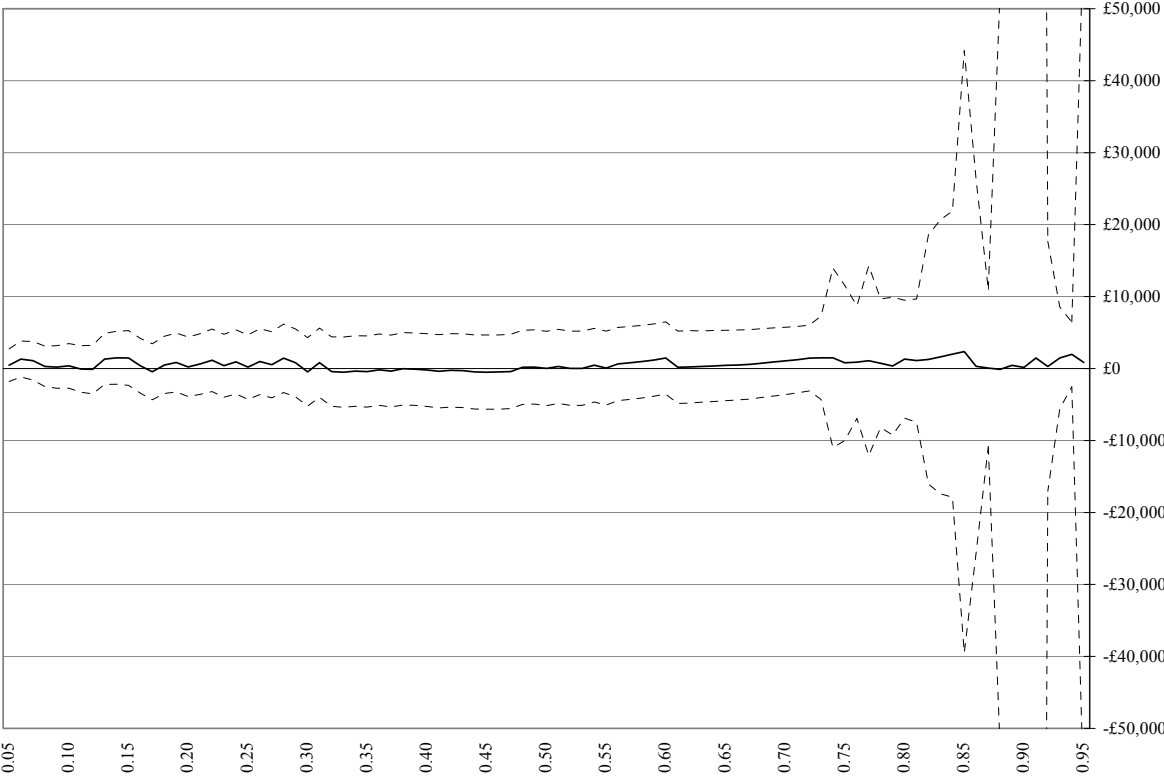
The solid line is the IV quantile estimate and the dashed line is the 90% confidence interval. To ensure convergence, these estimates do not include any covariates. Estimates are generated from Victor Chernozhukov and Christian Hansen's OX program generously provided by Christian Hansen.

Figure 2a: Reduced Form Quantile Estimates on Distribution of Financial Assets for Families With Minimum Schooling



The solid line is the quantile estimate. The dashed line is the 90% confidence interval from a bootstrapped procedure with 999 replications. To ensure convergence, these estimates do not include any covariates.

Figure 2b: IV Quantile Estimates on Distribution of Financial Assets for Families With Minimum Schooling



The solid line is the IV quantile estimate and the dashed line is the 90% confidence interval. To ensure convergence, these estimates do not include any covariates. Estimates are generated from Victor Chernozhukov and Christian Hansen's OX program generously provided by Christian Hansen.