

Bank Credit and Preferences for Redistribution: Experimental Evidence from the UK (Pre-Analysis Plan)

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1 Project Description

Over the past decades, economic inequality increased in many advanced democracies and, at the same time, welfare policies became less generous. This empirical observation stands in stark contrast to existing theories that assume that demands for redistribution increase as the group of beneficiaries of those policies grows (e.g. Meltzer and Richard, 1981). A buoyant scholarship has studied why individuals do not elect governments that provide higher levels of fiscal redistribution as economic inequality increases, for instance arguing that redistributive preferences are not solely driven by individuals' self interest but by altruistic motives too (Rueda, 2018) or that voters misperceive their relative economic position and thus form preferences that do not correspond with their economic situation (Cruces, Pérez Truglia and Tetaz, 2013).

In the aftermath of the global financial crisis, some scholars have argued that the massive expansion of credit markets that predated the Great Recession did not only fuel an economic bubble, but also allowed individuals to smooth consumption and to “keep up with the Joneses” despite growing disparity between those with top-incomes and those in the low and middle classes (Rajan, 2010). And while empirical research on the political and economic drivers of credit supply since the Great Recession has accumulated knowledge about the incentives that policymakers face to increase credit flows to society (Ansell and Ahlquist, 2017; Herrera, Ordoñez and Trebesch, 2014), the empirical evidence on the *demand-side effects* of credit expansions is sparse. Hence, we know little about how citizens react to better, more affordable access to credit, and most of the existing research implicitly assumes that myopic voters simply accept credit as a substitute for traditional welfare policies.

In a previous paper, we develop a theory about the conditions under which voters might be willing to trade off welfare policies for better access to credit (Markgraf, Rosas and Lavezolo, 2018). We followed Barr (2001) in arguing that the welfare state serves a redistributive function that shifts income flows across individuals (*Robin Hood function*) as well as an insurance function that allows individuals to protect against economic risk and uncertainty (*Piggy Bank function*); we then submitted that individuals support the welfare state out of different motives: low-income individuals in jobs with high labor market risks are more likely

to benefit from permanent redistributive welfare measures while high-income, low-risk individuals support the welfare state in order to have a publicly-funded insurance net that they might use sparingly to smooth consumption. Credit, we argue, is only an incomplete substitute for the welfare state in the sense that loans need to be repaid and thus cannot replace permanent redistributive measures. Following this theory, we submitted that high-income, low-risk individuals would be more likely to see access to bank credit as an advantageous substitute for the welfare state, whereas other individuals would be less likely to perceive credit as a close substitute for welfare.

We tested these implications using observational survey data from the European Social Survey in 23 countries over eight years (2002-10) and found overall support for our hypotheses. Yet, the study relied on survey data and used survey items that are not perfectly suited to test the theoretical claim: our main independent variable was a survey item that elicits respondents' self-assessments of their ability to access credit and our outcome variable was an item that simply asks respondents whether they agree that the government should reduce economic inequality. There are obvious concerns when analyzing this type of data: first, we cannot know for sure which sources of credit respondents have in mind as they consider this survey item, though we have taken every possible precaution we could imagine to ensure that the item is a reasonable proxy for bank credit. Second, the outcome variable does not allow us to disentangle the redistributive or insurance motivations of respondents.

Research on the relationship between credit access and support for the welfare state would therefore benefit from gaining a better understanding of the sources of borrowing available to individuals. While in our previous paper we purported to control for potential confounders, we harbor no illusions that we arrived at valid causal inferences regarding the conditions under which individuals willingly trade welfare for credit. It is certainly reasonable to be concerned about endogeneity. For one, our previous study lacked a complete understanding of the mechanisms that lead individuals to report different levels of access to credit, which means that uncontrolled covariates may confound the credit access effect we attempted to measure. Moreover, the very willingness to report high access to credit may depend on the structure of the welfare state: individuals with poor access to credit may see this as more than sufficient if they live in an environment with generous unemployment insurance. The experimental conjoint analysis that we propose therefore allows cleaner identification strategies to verify the existence of a causal credit access effect on preferences for redistribution.

The conjoint experiment that this document describes will provide causal evidence on the link between better credit access and support for the welfare state. It disentangles how different forms of bank credit — namely, access to *credit card loans* and access to *mortgage loans* — affect spending and taxation preferences of individuals in the United Kingdom. We distinguish between demand for the welfare state's redistribution function (social security transfers for low-income families) and for its insurance function (unemployment benefits), and thus are able to link different forms of bank credit with preferences over the different functions of the welfare state in order to causally test our theory about the trade-off between bank credit and support for the welfare state.

Prior to fielding this survey, we piloted two preliminary versions among a handful of colleagues and friends. The purpose of these pilot tests was to ascertain that the survey design was clear to respondents. As a result of these pilot tests we changed marginally some of the text surrounding our conjoint experiments.

2 Research Design

Conjoint analyses help us gain insight into individuals’ preferences across different dimensions and are particularly well suited to answer our research question, namely, whether and under what conditions individuals see bank credit as a substitute for different welfare policies. Conjoint analyses thus overcome the limitation of traditional survey experiments that vary one dimension at a time. Our conjoint experiment asks respondents to sequentially compare six pairs of countries. Respondents are asked to choose between two countries, a “forced-choice” design that enables us to evaluate the role of each attribute trait in the assessment of one profile relative to another. Moreover, respondents are asked to rate each country on a one to seven scale, which allows us to assess levels of support or opposition to each profile separately.

The fictitious countries that respondents get to assess vary along five attributes: “basic social security for low-income families”, “unemployment support”, “average income tax” “credit card loans (average interest rate)”, and “housing loans (average interest rate)”. These attributes along with their respective traits appear in Table 1. The traits for each attribute are clearly ordinal, and they are randomly assigned with equal probability. Though the countries that respondents see are fictitious, the traits that they assess are conceivable within current policy debates in the United Kingdom. In fact, in most cases we have chosen the “middle” level to correspond to current practice; the one exception is credit card loans, where we set the “highest” level to correspond to current practice.¹

Following Hainmueller, Hopkins and Yamamoto (2014), the order in which attributes appear in the survey is randomized across respondents to avoid primacy bias, but held constant within respondents to avoid confusion. In other words, the attribute order that respondents see for their first paired comparison remains unaltered during the other five paired comparisons they get to see. Attribute traits for all dimensions are fully randomized — hence, no combinations of traits are ruled out (no “blocking”). With unblocked traits, we also guarantee that predictors will be orthogonal, which is an assumption that we need to satisfy in order to estimate average marginal interaction effects.

3 Hypotheses

Note that country characteristics include two attributes about the ease of obtaining credit: *credit card loans*, which conveys the affordability of short-term credit that might help smooth consumption paths, and *access to housing loans* that provide the possibility of acquiring assets with long-lasting value. Trivially, we expect respondents to prefer cheaper to more expensive credit; we also expect them to prefer higher to lower welfare. We will report these marginal effects in our study. However, our main testable implications follow from theory developed in Markgraf, Rosas and Lavezzolo (2018). The basic starting point is the general proposition that welfare safety nets provide services for which there would be lower demand if financial markets were fully developed. An implication of this proposition is that

¹Appendix A lists sources of information for current levels of all these attributes in the United Kingdom. In our experiment, we simplified these policies to make them more understandable, but always keeping in mind to keep alternative levels within conceivable ranges.

Table 1: Five different attributes with corresponding traits employed in the conjoint analysis

| Attribute | Traits |
|--|---|
| Credit card loans (average interest rate) | 1: 5% 2: 15% 3: 25% |
| Housing loans (average interest rate) | 1: 2% 2: 6% 3: 10% |
| Unemployment support | 1: None 2: Up to 12 months 3: Up to 24 months |
| Basic social security for low-income families | 1: None 2: 33% of average income 3: 66% of average income |
| Average income tax | 1: 15% 2: 25% 3: 35% |

individuals would willingly trade-off less welfare (and, presumably, a lower tax bill) if they only had more affordable access to credit. This is the general implication that we test in our conjoint experiment.

More specifically, however, we do not expect all sources of credit to be equally effective in securing this trade-off. To the contrary, we expect individuals to understand that easier access to credit card loans might provide (some) protection against temporary income shocks such as brief episodes of unemployment, but we doubt that individuals would see consumer loans as permanent substitutes of welfare transfers such as social security for low-income individuals. In contrast, individuals may see more affordable access to housing loans as enabling independence from both unemployment support and more permanent income support. In general, we expect that easier access to either type of credit opportunity would correlate with lower tolerance for taxation. Consistent with these arguments, we estimate six specific interaction effects between sources of credit and sources of welfare.

1. Credit card loans and unemployment support: We expect a positive interaction between credit card loans and unemployment support, all else constant. In other words, the difference in support for the most and least generous levels of unemployment support should be smallest when credit card loans are most affordable. (The interaction is positive because of the way in which the questions are asked; increasing interest rates for credit card loans correspond to *less affordable* credit).
2. Housing loans and unemployment support: We expect a positive interaction between housing loans and unemployment support, all else constant. In other words, the difference in support for the most and least generous levels of unemployment support should

be smallest when housing loans are cheapest.

3. Credit card loans and basic social security for low-income families: we do not expect to find a statistically significant interaction effect for credit card loans and basic social security for low-income families, as this type of credit cannot be a permanent substitute for low income. Finding a positive interaction effect, would go counter to our theoretical expectations.
4. Housing loans and basic social security for low-income families: We expect a positive interaction between housing loans and low-income social security, all else constant. In other words, the difference in support for the most and least generous levels of basic social security for low-income families should be smallest when housing loans are cheapest.
5. Credit card loans and average income tax: We expect a negative interaction between credit card loans and average income tax, all else constant. In other words, the difference in support for the highest and the lowest taxation levels is largest when credit card loans are most affordable.
6. Housing loans and average income tax: We expect a negative interaction between housing loans and average income tax, all else constant. In other words, the difference in support for the highest and the lowest taxation levels is largest when housing loans are cheapest.

We estimate these interaction effects in six different models, inspecting one two-way interaction each time.² In all of these models, $Y_i(\mathbf{t}) \in \{0, 1\}$ is the preference that respondent i expresses for a country with characteristics \mathbf{t} , where \mathbf{t} includes *credit card loans*, *access to housing loans*, *unemployment support*, *social security for low-income families*, and *average income tax*. Corresponding to the first hypothesis, for example, we estimate the following linear probability model based on the ANOVA parameterization of Egami and Imai (2019) (and abusing notation for the coefficients for “controls”):

$$\begin{aligned}
 Y_i(\mathbf{t}) = & \mu + \sum_{j=1}^2 \beta_{1j} \cdot \text{credit card}_{ij} + \sum_{l=1}^2 \beta_{2l} \cdot \text{unemployment}_{il} \\
 & + \sum_{j=1}^2 \sum_{l=1}^2 \gamma_{jl} \cdot \text{credit card}_{ij} \times \text{unemployment}_{il} + \Lambda \cdot \text{Controls}_i + \varepsilon_i
 \end{aligned}$$

Notice that there are three levels for each of the two ordered factors of interest, in this case *credit card* and *unemployment*, which means that there are nine possible interactions when we combine *credit card* and *unemployment* levels. To expect a “positive interaction” in this context — where, remember, higher interest rates on credit card loans correspond to less

²Because of the full factorial design of the conjoint experiment, which generates orthogonal predictors, we could in principle estimate all interactions simultaneously in the same model without fear of biasing any estimates. In these circumstances, however, we would quickly run out of degrees of freedom.

affordable credit — means that we should see the largest difference in support for the most and least generous levels of unemployment support when we condition on high levels of *credit card loans*, i.e., on the most expensive loan rates for credit card debt. For each hypothesis, we will report in tables and plots these conditional differences in means for the corresponding “welfare” variable (i.e., unemployment support, social security, average taxation rate) when conditioning on levels of the “credit” variable (i.e., credit card loans, housing loans).

The models for the other five hypotheses are similar to the example we present here, only the interaction effects that we estimate vary from model to model. As is typical in conjoint experiments, we will estimate standard errors allowing for clustering at the “respondent” level. We allow for the possibility that respondents fail to find much difference between adjacent levels of some ordered factors. If we do so, rather than fussing with piecemeal elimination of irrelevant levels within factors in an undisciplined manner that would promote data mining, we commit to using Egami and Imai’s Grouping and Selection using Heredity in ANOVA regularization method (Egami and Imai, 2019), which allows us to collapse “unnecessary” levels of ordered factors in a consistent manner that is independent of our preferences.

3.1 Non-experimental items

Our theory does not lead us to expect other interactive hypotheses, so we additionally commit not to test for other interactions between the five attributes of the conjoint experiment. Because these personal characteristics are not randomly assigned, but are self-reported by respondents, we do not make strong claims about causality here (for starters, we have no way of anticipating what the sample distribution of these personal characteristics will be). As we explain below, our expectations about heterogeneity in effects are such that we will estimate the six above-mentioned interactive models in sub-samples of the data based on a number of respondent attributes; because our interest in these heterogeneous effects is largely for descriptive purposes, and to avoid printing a huge amount of models, we will succinctly report only those instances where we see obvious heterogeneity in effects.

We ask respondents a number of questions about their personal experiences with the welfare state and financial products, their political leaning, and their socio-economic backgrounds.³

- *Political leaning.* We ask respondents to place themselves on an eleven-point left-right scale (the default scale position is 5, right in the middle of the distribution, but needs to be confirmed by the respondent).
- *Personal experience with the welfare state.* We ask respondents about their current employment situation.⁴ Furthermore, we ask respondents whether they — currently or in the past — depend on either welfare transfers or unemployment benefits: respondents can select “Yes, I receive/have received unemployment support.”, “Yes, I

³All questions are asked in the survey after the experimental conjoint analysis, except for the questions determining the quotas, namely, education, gender, age, and region. See Section 3.2 for more information about the survey flow.

⁴Respondents can choose between six mutually exclusive answer options: “I have temporary full-time employment”, “I have permanent full-time employment”, “I have part-time employment”, “I am self-employed”, “I am currently unemployed”, and “None of the above”.

receive/have received welfare transfers (e.g. food stamps, subsidized housing).”, and “No, I receive/have received neither unemployment support nor welfare transfers.”; multiple answers are possible. Moreover, we collect information about respondents’ actual and perceived labor-market risk by asking them about their sector of employment (respondents can place themselves in one out of 20 sectors)⁵ and by asking them about their perceived risk of becoming unemployed in the next 12 months, as well as their perceived ability to find a similar or better job (on a scale from 1–5).⁶

- *Personal experience with financial products.* We ask respondents whether they have ever taken out a loan — we distinguish between credit card loans, student loans, mortgage loans, and other loans. Moreover, respondents are asked how easy it was for them to obtain the loan on a scale from 1 (very easy) to 5 (very difficult). This provides an ordered scale of the difficulties that respondents have encountered in obtaining bank credit. We also ask respondents whether they ever faced problems with repayment of their loan(s). Respondents can choose between “I had to declare bankruptcy”, “I had to renegotiate the payment schedule”, “I had to take out another loan to repay a previous one”, “I had to pause repayment for a while”, and no repayment problems. Multiple answers are possible. This provides an ordered scale of repayment issues. Finally, respondents are asked about their personal experience during the global financial crisis and how they and their entourage were affected by the crisis. They can choose between “I/Someone in my family/A person I know lost at least some savings during the crisis” or “None of the above”. Multiple answers are possible.
- *Socio-economic background.* In order to obtain a relatively representative sample we ask four quota questions before inviting respondents to proceed with the survey. We ask respondents about their age, gender, location (first three digits of postcode), and education.⁷ Additionally, we later ask respondents about their overall household income, including income from all sources; we present respondents with ten income brackets that reflect the income deciles in the UK taken from the European Social Survey and ask them to place themselves in one of the income brackets.⁸ Finally, we ask respondents whether any member of their household owns the house they are living in; respondents can choose between “No, we do not own our house”, “Yes, we are currently paying our mortgage”, or “Yes, our house is fully paid for”.

Our expectations are that individuals on the Left of the political spectrum will be somewhat less open to accept trade-offs between credit and welfare. We commit to reporting in-

⁵Information about the sector of employment allows us to determine the labor-market risk — e.g. sectoral unemployment rate (Rehm, Hacker and Schlesinger, 2012) — and the skill specificity of the respective career track (Iversen and Soskice, 2001).

⁶Questions about employment sector and perceived labor-market risk are only asked of respondents who report being employed.

⁷Respondents can choose between six answer options: “No qualifications”; “Skills for Life (including Basic Skills, Key Skills, Entry Level Certificates)”; “A-level/NVQ3 or equivalent”; “Bachelor’s degree”; “Master’s degree”; “Doctoral or professional degree (PhD, JD, MD)”.

⁸The income deciles are the following: 1st decile: under £11,475; 2nd decile: £11,475–15,392; 3rd decile: £15,392–19,088; 4th decile: £19,088–23,060; 5th decile: £23,060–27,473; 6th decile: £27,473–32,834; 7th decile: £32,834–39,169; 8th decile: £39,169–47,830; 9th decile: £47,830–63,277; 10th decile: >£63,277.

teraction effects on subsamples produced by comparing individuals with scores of 0–3 against those with scores of 7–10 on the Left-Right scale. Furthermore, we expect that respondents who currently benefit or in the past benefited from publicly-funded welfare policies will be less willing to trade off welfare policies for better credit access. Similarly, individuals in more precarious employment situations (part-time; temporary contract) and sectors (higher sectoral unemployment rates; higher skill specificity) will be less willing to accept the trade-off between better access to credit and institutional welfare policies.⁹ Besides the actual employment risk, we expect that respondents who perceive higher labor market risks (high unemployment risk; low re-employment chances) are somewhat more cautious in accepting credit as a private form of welfare.¹⁰ We commit to reporting the relationship between better access to credit and support for the dimensions of welfare policies for the different employment subsamples.

We also expect that respondents that report difficulty obtaining and/or paying loans, as well as those that report financial harm following the great recession, will be less open to accept a trade-off between credit and welfare. We commit to reporting the interactive effects identified above in subsamples comprising individuals that find it “very easy” or “easy” to obtain credit against those that find it “quite difficult” and “very difficult.” We will report separate estimates for those that report some kind of problem repaying a previous loan against those that report no problem. We will also report separate causal effects for those that report personal financial harm or financial harm to their entourage, and those that report no harm.

Finally, the socio-economic background should mediate the relationship between better credit access and demand for welfare transfers, unemployment benefits, and taxation. Respondents with higher income, better education, and who own a home are expected to be particularly attracted by the trade-off between bank credit and a generous welfare state.

3.2 Survey flow

Upon clicking on the link that opens up the survey, respondents first see a paragraph that informs them about the experiment and asks for their consent. Next, respondents answer our quota questions about year of birth (age), education, 3-digit postcode (location), and gender that determine whether respondents are invited to the remainder of the survey. After that, respondents see an information slide that provides a short description of the conjoint setup, explains how to use the “forced choice” buttons and how to scale both country profiles. Moreover, we inform respondents that we will ask them a number of questions after answering the six conjoint comparisons.

In the conjoint part of the survey, respondents are then asked to choose between two countries with randomized characteristics; overall, respondents see six comparisons. They need to choose their preferred option as well as evaluate both countries on a 7-point scale (see Figure 1 for an example). After the conjoint, respondents answer 7–11 questions about their political attitudes, their experiences with the welfare state and labor market situation,

⁹This expectation applies the concepts of Iversen and Soskice (2001) and Rehm (2009) to the trade-off between bank credit and welfare state demands.

¹⁰For a similar argument, see Walter (2017).

| Living conditions | Country 1 | Country 2 |
|---|-----------------------|-----------------------|
| Unemployment support | Up to 12 months | None |
| Basic social security for low-income families | 33% of average income | 66% of average income |
| Credit card loans (average interest rate) | 25% | 25% |
| Average income tax | 25% | 15% |
| Housing loans (average interest rate) | 10% | 6% |

Country 1

Country 2

Please rate on a scale from 1 to 7 how likely it is you would move to each of the countries if you had to leave the UK.

| | Would definitely not move [1] | [2] | [3] | [4] | [5] | [6] | Would definitely move [7] |
|-----------|-------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|---------------------------|
| Country 1 | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> |
| Country 2 | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> |

Figure 1: Screenshot of conjoint task presented to respondents

their experience with financial products, and their economic situation. The overall survey takes about 7–10 minutes.

3.3 Sample

We aim for a sufficiently large sample of around 1,200 respondents. Orme (2010) provides a simple “rule-of-thumb” calculation to determine the required sample size of $\frac{nta}{c} \geq 500$, where n refers to the required number of respondents (sample size), t is the number of tasks (6 in our case), a is the number of choices for each comparison (2 in our case due to forced choice without “don’t know”), and c is equal to the largest number of levels for any one attribute; as we are especially interested in two-way interactions between our credit measures and the attributes proxying the welfare state, c is equal to the largest product of levels in any two attributes (9 in our case). Moreover, Orme (2010) advises a larger number than 500 representations of each main-effect level. With a more conservative value of 1,000, and solving the equation above for n , the required sample size is 750. The slightly larger sample size of 1,200 individual respondents ensures sufficiently large cell sizes for the estimations and allows us to explore the heterogeneity of the effects. The experiment will be slightly under-powered when it comes to assessing causal heterogeneity across self-described groups,

but (1) we have no way of anticipating how large these groups are, and therefore have no way of calculating sample size, and (2) these causal heterogeneity calculations are meant to be more indicative anyway, as we cannot experimentally assign respondents to pertain to different groups.

We recruit respondents from the panel pool of Bilendi. Our sample is representative of the UK population based on education, age, gender, and region. Based on the quotas from the European Social Survey (wave 8 in 2016), we aim to recruit not more than 20% of respondents from London and 80% from areas outside London. Moreover, we aim to have at least 50% of respondents without university education, an equal number of respondents from both genders, and around 25% of respondents below 35 years, 50% between 35–64 years, and 25% above 64 years.

A Sources of information for current attribute levels in the United Kingdom

For each attribute, we sought information that UK citizens would readily find in a typical website based on a cursory internet search. Each attribute entry in the following table links to one such website:

| Attribute | Basic policy |
|---|--|
| Unemployment support | Unemployment benefits are contribution-based for the first six months, but means-tested and income-based after that |
| Social security for low-income families | Social security comprises a variety of different subsidies, transfers, and compensation packages that are difficult to price. In consequence, we have offered options that are multiples of the average income. In this case, the high level corresponds to about £20,000, the middle level to about £10,000. [†] |
| Average income tax | The average tax rate is 20% (the average income is around £30,000) |
| Credit card loans | The average credit card interest rate is currently at around 23%. |
| Housing loans | A typical mortgage interest rate is somewhere between 2 and 4%, depending on factors like size of the down payment and loan maturity. |

[†]We decided to suggest multiples of average income as the formulation that was easiest to understand.

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