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Mike Anson,[1] David Bholat,[2] Miao Kang,[3] Kilian Rieder[4] and Ryland Thomas[5]

Abstract

It is well known that quantitative credit restrictions, rather than Bagehot-style 'free lending' constituted the standard response to financial crises in the early days of central banking. But why did central banks in the past frequently restrict the supply of loans during financial crises? In this paper, we draw on a large novel, loan-level data set to study the Bank of England's policy response to the crisis of 1847. We find that credit rationing due to residual imperfect information in the sense of Stiglitz and Weiss (1981) cannot be a convincing explanation for quantitative credit restrictions during the crisis of 1847. We provide preliminary evidence which could suggest that discriminatory credit rationing on the basis of loan applicants' type and identity characterized the Bank of England's (BoE's) response to the crisis of 1847. Our results also show that collateral characteristics played an important role in the BoE's loan decisions, even after controlling for the identity of loan applicants. This finding confirms the hypothesis in Capie (2002) and Flandreau and Ugolini (2011, 2013, 2014) that the characteristics of bills of exchange submitted to the discount window mattered. Since our results suggest that the Bank also took decisions on the basis of the identity of loan applicants, our preliminary findings would seem to challenge Capie's 'frosted glass' metaphor, but more work is required to confirm these conjectures.

Key words: Credit rationing, lender of last resort, collateral management.

JEL classification: E44, E52, E58, G21, N12, N2.

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1 Introduction

It is well-known that quantitative credit restrictions[1], rather than Bagehot-style "free lending"[2], constituted the standard response to financial crises in the early days of central banking. Studying the Banque de France's (BdF) and the Bank of England's (BoE) response to financial panics during the 19$^{\text{th}}$ century, Bignon et al. (2012) provide evidence that the two central banks only gradually assumed their role as a lender of last resort (LLR) and routinely rationed credit up to and including the crisis of 1847. In Austria-Hungary, the Oesterreichische Nationalbank (OeNB) rationed credit until as late as the crisis of 1873 (Jobst and Rieder, 2016; Rieder, 2017). The historical experience of the banking crises during Great Depression of the 1930s in the United States represents another infamous example (Friedman and Schwartz, 1963), suggesting that these practices endured until well into the 20$^{\text{th}}$ century.

Why did central banks in the past frequently restrict the supply of loans during financial crises? Economic history research shows that central bank credit rationing can have particularly dire macroeconomic consequences. If credit is rationed during a financial crisis when demand for central bank liquidity is high, illiquidity-induced mass failures of financial intermediaries can ensue and trigger severe real economic crises (Calomiris and Mason, 2003; Richardson and Troost, 2009). Hence, a detailed understanding of the reasons behind central banks' decisions to restrict credit may help reveal some of the underlying culprits for the deep recessions associated with financial crises in the past.

In microeconomic theory, the term "credit rationing" has a very precise meaning. In their seminal contribution, Stiglitz and Weiss (1981) define credit rationing as a situation in which, among a group of identical loan applicants, only some receive a loan while others do not. In addition, the unsuccessful applicants would not be granted a loan even if they were willing to remunerate the loan at a higher interest rate or pledge more collateral. Throughout this paper, we thus refer to "pure" credit rationing when we use the term in the sense of Stiglitz and Weiss (1981). As we discuss in more detail below (c.f. Section 2), pure credit rationing results from information asymmetries between lenders and borrowers, in particular adverse selection dynamics, which generate credit frictions in financial markets. Microeconomic theory suggests that credit rationing can be an equilibrium outcome in credit markets characterized by residual imperfect information.[3] In addition to "pure" credit rationing, credit restric-

[1] The literature on credit restrictions commonly differentiates between price and non-price credit rationing. In this paper, we focus exclusively on non-price, i.e. quantitative, credit restrictions.

[2] Walter Bagehot (1873) advocated that central banks acting as LLR should lend freely, at high interest rates, and in return for good collateral. These principles are generally derived from various passages in Bagehot's major work, *Lombard Street*.

[3] Residual imperfect information is defined as a lack of information which persists even after borrower screening.

tions can also be the result of active lender discrimination against specific borrower or collateral types. We term this form of restrictions "discriminatory" credit rationing or "supply-side" credit rationing. Third, a particularly large number of rejections of loan applications relative to total applications may also be demand-side driven. As we will discuss below (c.f. Section 2), the standard test for the presence of central bank credit rationing in the existing literature might identify episodes as phases of credit rationing, although the apparent restrictions simply reflect a (sharp) deterioration of the quality of borrowers or collateral asking for credit. It is possible to imagine that the central bank uses a certain set of rules or minimum standards when deciding whether to accept or reject loan applications. If these minimum standards are less binding (because they are only rarely violated) in normal times but become much more so during financial crises (for example because distressed institutions run out of top quality collateral), then perceived credit restrictions might only reflect "rules-based" rejections.

In this paper, we draw on a large novel, hand-collected loan-level data set to study the Bank of England's policy response to the crisis of 1847. We chose this particular episode for two reasons. First, the consensus view, shared by contemporary sources such as *The Economist* and the secondary literature (Bignon et al., 2012), cites the BoE's policy response to the crisis of 1847 as an example *par excellence* of central bank credit restrictions. Second, although the BoE is said to have rationed credit in many earlier crises (Wood, 1939), the crisis of 1847 constitutes the first episode for which the Bank documented not only all successful loan applications, but also those demands for credit it had decided to reject.

Our work makes three distinct contributions to the extant literature. First, to the best of our knowledge, this study is the first to systematically exploit historical loan-level data from the BoE's archive using econometric techniques. Hence, we introduce our sources and explain how we compiled our data set in some detail below. Second, the availability of information at the loan/loan application level enables us to discriminate between the three different explanations for central bank credit rationing mentioned above. Previous studies merely identify episodes of credit rationing, whereas we are in a position to investigate the drivers of credit restrictions using microdata. Third, our paper directly contributes to a long-standing debate in financial history. Forrest Capie (2002, p.310) suggested that the BoE's discount window[4] was "made of frosted glass and raised just a few inches". According to Capie, the central bank did not care about the identity of the discount loan applicant: it simply lent against good collateral. Flandreau and Ugolini (2011, 2013, 2014) challenged this conventional account. In their view, the identity of all counterparties was crucial for obtaining central bank credit via

[4]As we explain below, the discount window was one of the two main lending arms of central banks in the past (the second standing facility consisted of advances, i.e. secured loans, frequently also called Lombard loans).

the discount window. In other words, the BoE would at least "raise an eyebrow" to check the identity of the discounter and record the names of all the counterparties involved in the loan application.[5] Thanks to our loan-level data set, we are able to test these conflicting hypotheses econometrically.

Although this study constitutes only a first step, we are able to draw two main conclusions on the basis of our analysis so far. First, we find that pure credit rationing alone cannot be a convincing explanation for quantitative credit restrictions during the crisis of 1847. While more work is needed to differentiate between supply- and demand-side driven rationing, we provide preliminary evidence which could suggest that discriminatory credit rationing on the basis of loan applicants' type and identity characterized the BoE's response to the crisis of 1847. Second, our results show that "collateral"[6] characteristics played an important role in the BoE's loan decisions, even after one controls for the identity of loan applicants. This finding confirms the hypothesis in Capie (2002) and Flandreau and Ugolini (2011, 2013, 2014) that the features of "collateral" submitted to the discount window mattered. Moreover, our results suggest that the Bank also took decisions on the basis of the identity of loan applicants. These preliminary findings would seem to challenge Capie's "frosted glass" metaphor, but more work is required to confirm these conjectures.

The remainder of this paper is organized as follows. Section 2 discusses the theoretical and empirical literature on credit rationing. It also delves into the Capie-Flandreau/Ugolini debate in more detail. Moreover, Section 2 explains how we use our data to discriminate between the different hypotheses on the drivers of credit rationing and discount window policies we would like to test. Section 3 provides the historical background for this study: we briefly survey the Bank of England's lending policies, discuss the London money market and explain the context of the 1847 crisis. In Section 4, we describe our data sources and data mining/sampling techniques. Section 5 marshals descriptive evidence and conducts simple tests based on our data. Section 6 contains the econometric analyses of our paper. Section 7 concludes.

[5] As we explain below, discount loans consisted of outright purchases of bills of exchange. Bills bore the name of several parties involved in the underlying (real) transaction, c.f. Santarosa (2015).

[6] Strictly speaking, it is incorrect to refer to "collateral" in 19$^{\text{th}}$ century discount window operations. As we explain below, the BoE bought bills of exchange outright rather than lending against them. We refer to "collateral" characteristics here whenever we refer to the characteristics of the bills purchased by the central bank. Hence, collateral characteristics really represent a proxy for the credit standing of the underlying guarantors (acceptor and other endorsers).

2 What drives credit rationing?

In a textbook model of supply and demand for credit in a competitive, frictionless environment, the price, i.e. the interest rate, would adjust to clear the market for loanable funds. In their seminal article, Stiglitz and Weiss (1981) show that credit markets do not always have to clear: credit rationing can be an equilibrium outcome in financial markets. The intuition for their finding is as follows. Loan interest rates influence both the demand for loans and the willingness to supply loans. For a given set of collateral requirements, higher interest rates attract riskier borrowers because these borrowers estimate their probability of having to repay loans to be low in the first place. The lender's expected return to loans is hump-shaped[7], i.e. decreases once the interest rate exceeds a certain level, because the negative impact on earnings deriving from a higher probability of default associated with riskier borrowers more than outweighs the positive effect of higher interest rates. Consequently, there might be a "threshold" interest rate above which credit is rationed. Fearing the negative effects of adverse selection, the lender rations credit even though some loan applicants would be willing to pay higher rates to obtain loans.[8] When the lender experiences an excess demand for credit, the interest rate is not drawn upon as a tool to clear the market: among a group of identical loan applicants, only some receive a loan while others do not. In other words, there is an arbitrary element involved in credit rationing à la Stiglitz and Weiss (1981): some borrowers are discriminated against despite having the same characteristics as successful applicants. A selection of applicants takes place, where the selection process itself cannot be meaningfully justified from an economic or financial point of view. It occurs via filtering processes, for example on a "first come, first served" basis, and might indirectly entail "picking winners".

It has been shown that credit rationing is likely to be less prominent in more realistic financial market settings where lenders can vary both interest rates and collateral requirements simultaneously (Bester, 1985). However, some financial institutions find it difficult to adjust risk controls as flexibly as standard commercial banks. Central banks are a case in point. First, typically central banks do not adjust collateral requirements "on the fly". Statutory limits often determine the collateral types and haircuts acceptable for central bank credit which apply to all central bank counterparties equally. These rules are difficult to change and they are rarely adapted to the individual borrower. Second, central banks do not usually adjust interest rates to the characteristics of different borrowers.

[7] More recently, Arnold and Riley (2009) and Su and Zhang (2017) have challenged the theoretical possibility of rationing equilibria in the setting described by Stiglitz and Weiss (1981).

[8] The same argument can be made fixing the interest rate while changing the collateral requirement, c.f. Stiglitz and Weiss (1981).

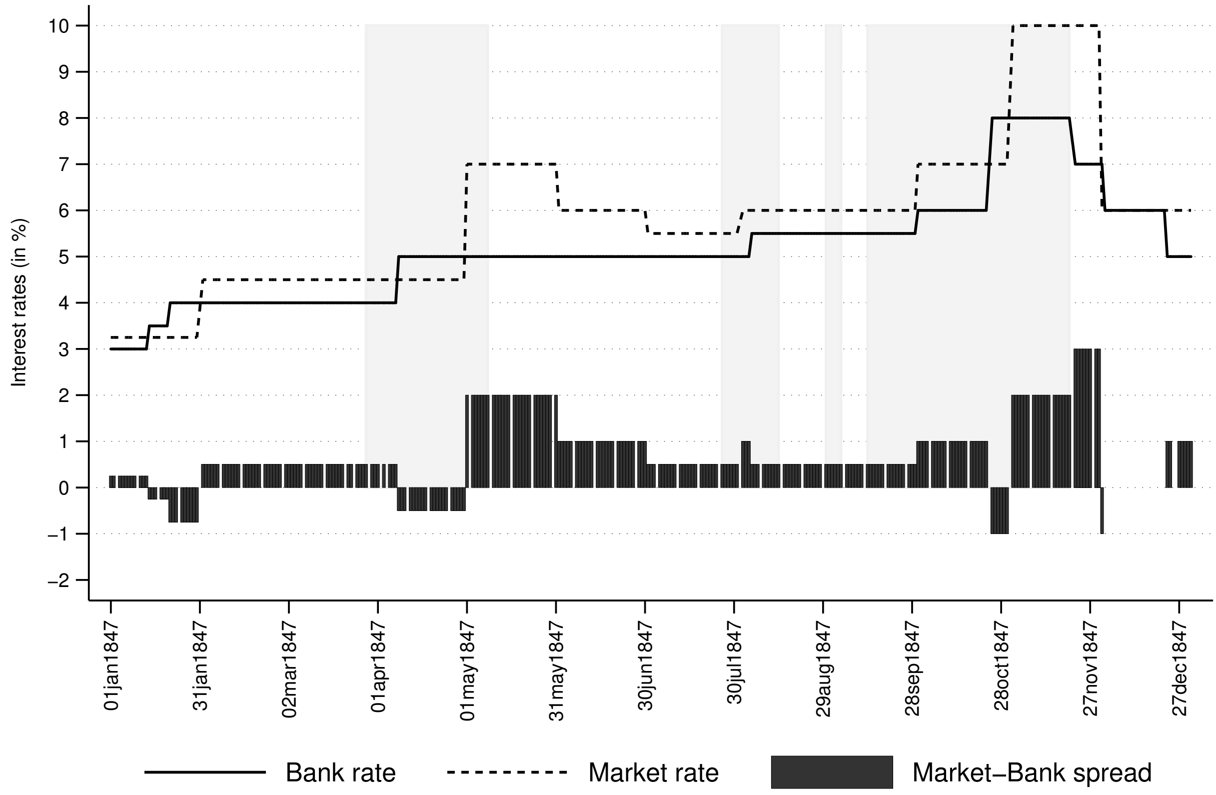
Although it may change over time, the Bank rate or main refinancing rate is typically the same for all counterparties at a specific point in time. Ceteris paribus, this lack of flexibility suggests that central banks may be particularly prone to succumb to rationing practices, in particular during financial crises, when demand for credit is high and residual information asymmetries become amplified by the hectic circumstances and large volumes of loan applications.

Contemporary observers and the secondary literature suggest that central banks indeed rationed credit frequently during historical crises in the 19th century, before they adopted a Bagehot-type doctrine of "free lending" and became fully-fledged LLRs (Bignon et al., 2012). Empirical tests of central bank credit rationing rely on comparisons of aggregate interest rate data. Under the null hypothesis of free lending, the central bank's "interest rate (for any given quality) ought to always be above, or equal to, the market rate (for the same quality)" (Bignon et al., 2012, p.590). The market rate can only be higher than the central bank rate in the presence of credit rationing because, under free lending, an initially higher market rate would decrease immediately due to funding liquidity arbitrage. Bignon et al. (2012) find that credit rationing was common during financial crises in France and Britain at least until the beginning of the 1850s, while evidence from Austria-Hungary and the United States suggest that credit restrictions characterized central bank policy responses to much later crises too (Jobst and Rieder, 2016; Rieder, 2017; Friedman and Schwartz, 1963). Figure 1 shows the test in graphical form for the British crisis year of 1847: a positive market-Bank spread reflects episodes identified as moments of central bank credit rationing.[9]

Can historical episodes identified as moments of central bank credit rationing be fully explained by the presence of informational frictions? The gradual adoption of LLR policies makes this explanation a plausible one. As more sophisticated screening and monitoring techniques became available over time, residual imperfect information should have constituted a less and less binding constraint for central bank lending during crises: Bagehot-type free lending in turn became possible. Yet, the empirical test proposed by Bignon et al. (2012) only identifies occurrences of credit rationing, not their underlying drivers. Instances of credit rationing identified on the basis of interest rate spreads are compatible with at least three different explanations. First, credit rationing could be driven by informational frictions as suggested by Stiglitz and Weiss (1981). We term this type of credit restrictions "pure" credit rationing. In this case, the positive spread between market and Bank rates results from the fact that some borrowers cannot obtain credit despite the fact that their loan application is qualitatively equivalent to the successful applicants.

[9] The shaded areas in gray in Figure 1 represent crisis windows we identified using aggregate loan data, c.f. Section 4 below.

Figure 1: Credit rationing during the crisis of 1847: market rate for prime bills, Bank rate and the market-Bank spread



Source: Bank of England Archives, The Economist

Second, credit rationing as identified by the "rates test" could also derive from active discrimination by the central bank against certain types of applicants or certain types of collateral. To see why this second explanation is equally compatible with the empirical test based on interest rate comparisons, one needs to understand the details behind the interest rates drawn upon for the test.[10] For Britain, Bignon et al. (2012) compare the market rate sourced from *The Economist* to the Bank rate for bills with the same remaining maturity. While this approach obviously controls for maturity, it remains unclear what exactly it tells us about the other characteristics of the collateral for which it is quoted and whether it provides any information on the type/identity of loan applicants at all. Although the market rate is sometimes mentioned as referring to "prime" or "first class" bills, it is unknown whether this definition is consistent over time or what exactly it entails. Hence, the relative quality of loan applications largely remains a black box when we compare market and Bank rates. If discrimination happens on the basis of characteristics we do not observe by simply comparing interest rates, then

[10]In the following, we focus on a discussion of market rates to explain why episodes of credit rationing identified by the interest rate test could be compatible with explanations other than pure credit rationing. However, one might also challenge the Bank rate component of the test: in the specific historical case of the BoE's response to the crisis of 1847, one can show that although there was a single Bank rate, the BoE effectively charged a range of different rates. Once a weighted average Bank rate is computed, central bank credit rationing appears less extreme than in the traditional accounts (although it persists), c.f. Figure 6 in the Appendix. We explore these different rates, including their rationale and drivers, in another ongoing research project.

the absence of funding arbitrage might not be caused by informational frictions but by the fact that some of the collateral or borrower types in the market are judged to be less credit-worthy during periods of financial distress (from the point of view of the central bank). In this scenario, the market-Bank spread during crisis episodes does not reflect pure rationing affecting qualitatively identical borrowers/collateral, but could be the result of discriminatory practices the central bank "switches on" when in crisis mode. Therefore, an outside observer of interest rates would be mistaken in her believe to be comparing like with like.

Third, a particularly large number of rejections of loan applications relative to total applications may also be demand-side driven. The standard test for the presence of central bank credit rationing in the existing literature might identify episodes as phases of credit rationing, although the apparent restrictions simply reflect a (sharp) deterioration of the quality of borrowers or collateral asking for credit. Under this hypothesis, the market-Bank spread is not a sign of informational frictions causing a break-down of funding arbitrage. Rather, it merely reflects lower quality borrowers/collateral swamping the market. This phenomenon could remain hidden to the outside observer of interest rates if the latter are compared only on the basis of a few characteristics that do not capture the fall in quality during crisis windows. It is also possible to imagine that the central bank uses a certain set of rules or minimum standards when deciding whether to accept or reject loan applications. If these minimum standards are less binding (because they are only rarely violated) in normal times but become much more so during financial crises (for example because distressed institutions run out of top notch collateral), then perceived credit restrictions might only reflect "rules-based" rejections.

In the introduction, we argued that understanding the drivers of credit restrictions may help reveal some of the underlying culprits for the deep recessions associated with some financial crises in the past. A detailed understanding of the causes for credit rationing might also provide policy-relevant insights regarding the pre-requisites for successful LLR.[11] For example, should the fear of adverse selection turn out to constitute the main driver of central bank credit rationing during financial crises, policies and governance reforms targeted at reducing residual imperfect information may be crucial to minimize the negative effects of credit restrictions. If, on the contrary, credit restrictions are the result of discriminatory practices which effectively violate Bagehot's rules[12], a publicly known lending framework and central bank accountability could help mitigate the incidence of credit rationing.

[11] For a review and discussions of the foundations for and prerequisites of LLR, c.f. Calomiris et al. (2016).

[12] Bagehot (1873, p.97) defined "good collateral" as financial paper which "in ordinary times is reckoned a good security" that is "commonly pledged and easily convertible". A central bank which, in the context of a financial crisis, refuses to lend against collateral it considers to be acceptable/eligible in normal times would thus violate Bagehot's rule.

In order to differentiate between the different possible drivers of credit rationing, we need to go beyond the interest rate tests discussed above. We argue that micro-data at the loan-level is required to provide a fresh perspective on this question. While we discuss the nature and sources of our data in detail in Section 4 below, we now proceed to explain how we propose to discriminate between the different causes of credit restrictions using these disaggregated data. To simplify notations, we assign the following acronyms to the three hypotheses: PR stands for "pure" credit rationing à la Stiglitz-Weiss, DR is used as an abbreviation for discriminatory rationing and RBR is short for rules-based credit restrictions.

Under the PR hypothesis, the data should exhibit three major characteristics. First, credit rationing à la Stiglitz-Weiss implies that, among a group of qualitatively identical loan applications, only some receive a loan while others do not: in other words, there is an important random element to PR. Hence, during crisis windows, when rationing is acute, rejected loan applications should not be easily distinguishable from accepted applications for credit. Alternatively, a weaker version of this argument would see rejected and accepted applications during crisis episodes to be less different than in normal times when the problem of residual imperfect information is less daunting. A simple test of this prediction would be to investigate whether the applicant and collateral characteristics of rejected and accepted applications are on average statistically different from each other in times of crisis. Second, and related to the first point, under the PR hypothesis, we would expect the quality characteristics of loan applications (collateral characteristics and/or applicant identity) to be weak predictors of rejections in times of acute financial distress – or at least be weaker predictors than in normal times. Finally, the PR hypothesis suggests that the chronological or size rank of a loan application, rather than its inherent quality, may predict the central bank's decision to reject or accept demands for credit. If fears of adverse selection reduce the central bank's willingness to raise interest rates, there may be a particular threshold of credit the central bank is willing to grant on any given day before starting to reject "excess" applications. For example, one might expect that demands submitted at a later time of the day or demands which stand out as particularly large in comparison to the other applications on a given day might have a higher probability of being rejected.

The RBR hypothesis stands in stark contrast to PR. Under this hypothesis, rejections of credit applications follow clear rules, both in crises and normal times. For example, the central bank might always require collateral to fulfill certain minimum ratings to be eligible for a loan. Hence, the quality characteristics of rejected and accepted loan applications should be statistically different in the context of RBR. As a corollary, at least some collateral characteristics and/or features of applicant identity are expected to be important predictors of credit rejections. Moreover, given a constant set of rules according to which the CB grants or rejects demands for credit, regression coefficients should be similar in normal and crisis times. The RBR hypothesis also requires that the share of low quality applications is on average measurably higher in crises than during times of calm: according to RBR, the higher amount of low quality applications drives the high share of rejections in times of distress. In contrast to PR, simple ranks of applications for credit should not matter: as long as an application fulfills certain substantive quality criteria, it should not be essential when the demand for credit is placed on a given business day or how large it is relative to other loan requests.

In many respects, the DR hypothesis is difficult to distinguish from RBR. Under the DR hypothesis, there is room for rules that might make it possible to distinguish rejected from accepted loan applications on the basis of their characteristics during times of distress. Also, some collateral characteristics and/or some features of applicant identity should be good predictors for rejections in times of crisis, potentially reflecting discrimination against specific applicants or collateral types. Furthermore, DR could be practiced despite or in addition to the pouring in of lower quality applications in crisis times. Crucially, however, under DR at least some regression coefficients should be radically different in crisis times and normal times. Since discriminatory practices come to the fore in times of distress, additional or different rules for rejections should apply and thus be reflected in changes of the predictive power of variables and/or the size/sign of their coefficients. Consequently, out-of-sample predictions of credit rejections during crisis windows, on the basis of coefficients obtained from regressions explaining loan decisions in normal times, should be inaccurate. More precisely, one would expect to significantly underpredict the intensity of rejections in crisis times using these out-of-sample coefficients. In contrast, RBR would require that out-of-sample predictions are fairly accurate given that the same set of rules is applied in both crises and normal episodes. We summarize the possible tests and predictions according to the different hypotheses in Table 1. If we do not have a clear prediction for the outcome of a specific test, the entry for the corresponding hypothesis reads "N/A". In the present version of this paper, we mostly marshal evidence that allows us to discriminate between PR on the one hand and DR and RBR on the other hand.

An interesting aspect of the proposed tests in Table 1 is that they also speak directly to a long-standing debate in financial history. The traditional narrative links the transformation of the BoE into the first modern central bank, including the adoption of lender of last resort policies, to the development of an anonymous dealing with its day-to-day counterparties on the London money market. Capie (2002, p.310) famously suggested that the Bank's discount window was "made of frosted glass

Table 1: Testing for credit rationing using microdata

Test	**PR**	**DR**	**RBR**
Rejected applications ≠ accepted applications	No	Yes	Yes
Share of low quality applications	N/A	N/A	higher in crisis
Regression coefficients similar in crisis & normal times	N/A	No (at least some are different)	Yes
Out of sample predictions	N/A	Bad (underpredicting rejections)	Good (accurate predictions)
Collateral characteristics matter	No	Yes	Yes
Applicant identity matters	No	Yes	Yes
Intra-day ranks matter	Yes	N/A	No

and raised just a few inches". According to Capie, "the central banker does not know, nor does he care who is on the other side of the window; [h]e simply discounts good quality paper or lends on the basis of good collateral" (Capie, 2002, p.311). Flandreau and Ugolini (2011, 2013, 2014) challenge this conventional account. In their view, the identity of all counterparties was crucial for lending via the discount window. The authors argue that the very nature of bills of exchange, i.e. the "collateral" in the transactions we consider[13], meant that all the names written on the bill submitted – the discounter, the acceptor, and the original drawer – were important as they were all jointly responsible for its payment (Santarosa, 2015). In other words, the BoE would at least "raise an eyebrow" to check the identity of the discounter and record the identity of all the counterparties on the submitted bills.

In simple terms, the debate between Capie (2002) and Flandreau and Ugolini (2011, 2013, 2014) boils down to whether the identity of the loan applicant (the so called "discounter") mattered for the BoE's decision to accept or reject a given demand for credit. While the two sides agree that the quality of "collateral" was crucial for successful applications, they disagree on the importance of applicant characteristics. As will become clearer below, discriminating between these two hypotheses is not trivial, not least because discount loan applications were not made with individual bills but usually contained packets of bills which were all submitted simultaneously. Testing requires microdata on the loan-level and a research design that allows for *ceteris paribus* analysis. We are not simply interested in testing whether discounter identity or collateral characteristics mattered. Rather, to discriminate between the "frosted glass" and the "raised eyebrow" hypotheses, it is necessary to check whether the discounter identity matters once we have controlled for collateral characteristics. In the present version of this paper, we merely start presenting some suggestive evidence in this regard.

[13]Please see below for a more detailed explanation of discount window operations and the nature of bills of exchange.

3 Historical background

Before we discuss our data (sources) and explain our empirical strategy in more detail, the present section provides some brief background information on our case study. We focus on the institution (the Bank of England), its environment (the London money market) and the episode of interest (the crisis of 1847) to set the groundwork for the remainder of this paper.[14]

3.1 The Bank Charter Act and financial stability

The Bank Charter Act of 1844 had sought to remedy the errors of crises past by trying to prevent the over-issue of banknotes that many had felt was the major cause of previous crises in 1825 and 1837 and the bursts of inflation experienced during the French Wars. The Act gave the Bank of England an effective monopoly in the issue of new bank notes and those additional notes had to be backed one for one with gold. This was felt to be sufficient to control the money stock and prevent inflation but the Act also built in a safety valve to allow it to respond to emergency demands for liquidity. The 1844 Act split the Bank into two departments, an accounting convention that exists to this day (Bank of England, 2017).

The Issue Department of the Bank was to look after the note issue and the issuance of new notes was tied to the amount of gold it held in reserve. There was a fixed limit of £14mn on the number of notes that could be backed by securities known as the fiduciary issue. The idea was that if gold flowed out of the country due to an overheating economy and a balance of payments deficit, the stock of notes in circulation would automatically decline as people cashed in their notes for gold to pay for imports. It was also envisaged that the Bank would respond to this by raising interest rates and reducing its lending to protect its reserves and attract more gold from abroad. The combination of higher interest rates and a falling money supply would lead to lower activity and prices which would improve the balance of payments and provide an automatic stabilization mechanism. The rest of the Bank – the Banking Department – was envisaged by the Act to operate like any other private bank. It could lend to the private sector on normal commercial terms (largely via discounting bills of exchange or advances secured on high quality collateral) and provide deposit accounts for its customers. Importantly, the Banking Department was to back up its liabilities with its own reserve holdings of Bank of England notes in a similar way to other commercial banks. The freedom offered to the Banking Department

[14] Some of the subsections below draw heavily on material published in a companion working paper (Anson et al., 2017).

allowed it to act as safety valve in the event of a crisis. Even though the total note stock would be pinned down by the gold reserves in Issue Department, the part of the note stock held in reserve in the Banking Department could be used to provide liquidity in a crisis. This arrangement allowed the Bank to discount bills to meet an increased demand for notes or sterilize the impact of a fall in notes in circulation resulting from an external drain of gold. The Banking Department could also lend more and create deposits for its customers like any other bank. The deposit liabilities of the Bank were not subject to any constraint or any formal link to the gold reserves in Issue Department.[15]

These stipulations, although well-intentioned, had several (largely) unforeseen consequences each of which were to play a part in the build up to the 1847 crisis and how it unfolded. First, the Bank directors took the freedom offered by the split of departments to use the Banking Department balance sheet to compete more aggressively with the growing commercial banking system and discount market. The Bank at this stage was still privately owned and needed to make a return for its shareholders. In the two years following the passing of the Act, the Banking Department's holdings of private securities expanded markedly following a cut in Bank Rate (the rate at which it would lend to the market) from 4% to 2.5%. Second, although there was a safety valve built into the Act allowing the Bank to increase its lending and respond to demands for emergency liquidity, the discretion the Discount Office could exercise in practice had severe limits, which were most clearly manifest during the height of financial crises. In a crisis, there would be an increased demand for Bank of England notes by the financial system which could be obtained by discounting short-dated bills with the Discount Office or borrowing on a secured basis ('advances'). Banking Department would typically pay loans out of its note reserve. As a crisis was deepening, and the demand for discounts continued to be high, the Banking Department's note reserve could effectively dry up. This possibility created the prospect that the Discount Office might no longer discount bills. Anticipation of this threshold effect could then cause additional panic in money markets – dynamics which would indeed happen in 1847 and in future crises. In response, the Government at this point often provided the Bank with an indemnity allowing it to breach the 1844 Act. The suspension of the Act would allow the Issue Department to create additional notes which would be forwarded to the Banking Department in exchange for some of its bills and securities. The notes could then be used by the Discount Office to discount additional bills. During the 1847 and 1866 financial crises, the mere existence of the indemnity stopped the panic and this transaction never took place. It did, however, get carried out in 1857.

[15] For a more detailed discussion, c.f. Anson et al. (2017).

3.2 The London money market around 1850

In the 19$^{\text{th}}$ century, the key money market instrument/cash equivalent was the bill of exchange. A bill of exchange was a written instruction ordering one party to pay another. While many readers today may have no practical experience with a bill of exchange, most will be familiar with cheques, which are legally a special kind of bill of exchange (Elliott et al., 2013). A cheque is 'drawn' (written) by a person on their bank to pay a third party. Similarly, a bill of exchange is 'drawn' by one party (called a 'drawer') on another (called a 'drawee') instructing them to pay either the drawer or a third party (called a 'payee').[16] Unlike a cheque, a bill of exchange is not necessarily 'drawn on' a bank. It can be a payment instruction to anybody. In fact, in the 19$^{\text{th}}$ century, a bill of exchange was most often literally a bill following the sale of goods and services. For example, a manufacturer might supply goods to a merchant on credit perhaps because the merchant was unable to pay cash for the goods until after they had been sold to consumers. In this example, the bill acted like an invoice tangibly documenting the trade credit that had been extended in the transaction between the two parties. The manufacturer (the drawer) would send a bill to the merchant (the drawee). If the merchant 'accepted' that they owed a debt to the manufacturer, they would sign their name on the bill. Legally, they were now referred to as the 'acceptor' of the bill instead of the drawee.

Rather than holding the bill to maturity, the manufacturer might cash in the debt before maturity in either one of two ways. One way was for the bill of exchange to be used directly as currency when paying for goods and services, or discharging debts. Indeed, in some areas of Britain during the 19$^{\text{th}}$ century, bills of exchange circulated as extensively as other types of currency such as Bank of England notes and Royal Mint coins (Ashton and Sayers, 1953). When a person or institution holding a bill transferred it to another, they had to sign their name on the back of the bill just as the acceptor had done. If the original acceptor did not pay in full or in part, all endorsers (including the original drawer of the bill) were liable to pay whoever currently held it. There was thus a 'bandwagon' effect at play as bills of exchange circulated. The more frequent a given bill circulated, the more endorsers it had. Since there were then more guarantors, the bill of exchange became an increasingly safer asset, more closely approximating cash (Santarosa, 2015). The negotiable nature of bills of exchange, underpinned by multiple endorsements, resulted in their emerging as the key cash equivalent in the first half of the nineteenth century.

[16] Like cheques, bills of exchange are negotiable instruments. This means two things. First, it means they can be transferred from one party to another without explicit consent from the drawee/acceptor, i.e. the debtor. Second, it means that any subsequent holders of the bills (transferees) are "capable of obtaining a perfect title to the instrument in spite of any defects in the title of the prior parties" (Holden, 1955, p.314)

The other way to encash the bill before maturity was for the holder of the bill to sell it to a financial firm. In the 18th and early 19th century, holders of bills of exchange often arranged sales of their bills through bill brokers. Bill brokers initially acted as financial intermediaries between buyers and sellers of bills of exchange. Like banking, bill brokering developed in Britain during the late 17th century spurred by real economic growth and the need for new financial channels to finance it (Pressnell, 1956). Indeed, bill brokering was an important supplement to the limitations of banks as they were structured at that time. In this period, the vast majority of banks were single shops (unit banks) without branches. Therefore, bill brokers acted as conduits for the buying and selling of bills of exchange across different geographical areas. Thus, banks holding bills of exchange who wanted cash before maturity would send their bills to bill brokers, who then arranged for their discount by banks in other parts of the country with surpluses of cash looking for investment in cash equivalents (Banks, 1999). For this service, bill brokers earned income from commission.

However, by the 1830s, many bill brokers had transitioned from intermediaries of bills of exchange to become investors in them. This shift in business model occurred in response to the 1825 financial crisis. According to most historians, many banks, particularly in the City, felt unnerved by the fact that, during the 1825 crisis, the Bank of England was perceived as having belatedly responded to their demands for liquidity via rediscounting of bills of exchange (Fletcher, 1976). The demand from London banks for a cash equivalent stimulated the introduction of new facilities by bill brokers. Rather than stockpile zero yielding Bank notes, London banks began to deposit their money 'at call' (on demand) with bill brokers, many of whom, spotting a market opportunity, started to offer demand deposits. As a result, many bill brokers evolved into so-called discount houses which financed their own portfolio of bills with funds borrowed from banks. The viability of discount house demand deposits to function as a cash equivalent increased when the Bank announced rediscounting facilities for London discount houses in 1833 (Fletcher, 1976), providing assurance to banks that discount houses would be able to honour their commitments to pay Bank notes on demand.

By the 1840s, a dense money market network had emerged structured through bills of exchange. At the core of this network were three key institutions: (1) 'clearing' banks[17], (2) discount houses and (3) the Bank of England. These institutions were thus interlinked through their balance sheets. Banks funded themselves mostly by notes and deposits. While they used some of these funds to buy bills directly, by the 1840s, a growing percentage of their assets were call loans to discount houses. The discount houses used banks' deposits to fund their portfolios of bills of exchange. The discount

[17]The qualifier 'clearing' in front of banks indicates that the institutions we are referring to are mostly London-based institutions who 'cleared' or settled claims on behalf of correspondent banks located elsewhere in the country.

houses in turn might rediscount these bills for cash from the Bank of England. The importance of these rediscounting operations became evident during the crisis of 1847.

3.3 The Bank's Discount office and its method of lending to the financial system

The bulk of the Bank of England's lending operations during the 19th century were exercised through the Discount Office at the Bank's London headquarters on Threadneedle Street. Though with a large responsibility, the Discount Office was physically small. The Office was headed by the Principal of the Discount Office and, on average, had seven staff throughout the period between 1840 and 1880. In terms of headcount, this made the Discount Office a relatively small part of the Bank. The Office was open six days a week, or roughly 305 trading days per year, but was open only from 11 am to 2 pm every day (Ogden, 1999). It is not known whether these hours were extended during financial crises.

It is worth bearing in mind that the Bank of England's branches outside of London would have also been discounting bills, besides the Discount Office at Head Office in London.[18] Unfortunately, data on the activities of branches no longer exist. However, we do have a sense of the aggregate value of these transactions from the annual reporting of the Bank's branches' activity to Court.[19] During the 1847 and 1857 crises, roughly 40% of the Bank's business discounting bills by value was done through its branches. In 1866 it was 50%.

The Discount Office typically lent in two ways: discounts and advances. Discounts involved the Discount Office purchasing bills of exchange at discount on their face value. The amount of this discount was determined by the discount rate, also known as Bank rate. A Bank rate of 6% thus meant that the BoE would purchase bills at 94% of their face value. Advances were collateralized loans by the Bank, akin to modern day repos. The discount houses would temporarily sell debt securities to the Bank with an agreement to buy them back at a future date prior to maturity. In 1847, the debt securities used as collateral included government bonds and railway stocks, as well as other bills with a maturity longer than the Bank would be comfortable to purchase outright. In general, in the mid-19th century, discounts were much more important than advances. For example, discounts represented 65% and 62% of transactions in 1857 and 1866, respectively.[20] Although, as we

[18] The Bank was permitted by law to establish branches outside of London from 1826.

[19] Reports were made to the Special Discount Committee, BoE C35 and the annual data can also be found in Bank of England Archives, Discount Office Analyses and Summaries, BoE C30/3.

[20] The ratio of discounts to advances would change later in the 19th century when advances became more important, c.f. Ugolini (2016).

will discuss in Section 4, the Discount Office retained immaculate records of the bills it discounted (and rejected), very little documentation remains that sheds light on what determined the Office's decisions to discount bills and make advances. This lack of documentation gave rise to the "Frosted Glass" versus "Raised Eyebrow" debate we have already discussed in the previous section.

3.4 The crisis of 1847

The crisis of 1847 occurred in two phases (Dornbusch and Frenkel, 1984; Campbell, 2014). The first phase, which peaked in April 1847, did not lead to a full blown crisis but was a harbinger of things to come. The economy was in a weak position at the start of the year and the Bank's aggressive discount policy mentioned earlier had helped to fuel a boom in railway shares between 1845 and 1846 that was now beginning to unwind. Bad harvests had also put upward pressure on corn prices and imports which in turn put pressure on gold reserves. The Bank, however, was slow to react to these developments and insulated the impact by continuing to lend at rates lower than those in the market. By April, the Bank's reserve of notes had fallen dangerously low and both the Bank and the public woke up to the implications. The Bank reacted strongly by raising Bank Rate, cutting back its lending to the market and selling government bonds. The sudden change in policy led to a temporary panic, but was partially cured when higher rates led to inflows of gold.

A sting in the tail came later in the year. The bad harvests of 1845-6 were followed by a better harvest in 1847 and the price of corn fell sharply. Many companies had speculated on prices remaining high and began to suffer heavy losses. As a result there was a string of commercial failures with a knock-on effect to exposed lenders – several discount houses and provincial banks were forced to shut their doors (King, 1936). The renewed financial distress led to an increasing scramble for safety and rates in the discount market shot up to unprecedented levels. The Bank did its best to accommodate (albeit at high interest rates), but its reserve of notes started plummeting and, as discussed earlier, the Bank Charter Act implied no new Bank notes could be issued. Initially, the approaching reserve limit served to reinforce the panic. Leading firms in the City sent a deputation to the Government to ask that the Act be suspended. The Prime Minister and the Chancellor reacted by writing a letter to the Bank's Governor on 25 October 1847 allowing them to increase their lending and indemnifying the Bank from any legal breach of the 1844 Act. The suspension of the Act almost immediately ended the crisis to such an extent that the Bank did not need to actually breach the legal limit on the note issue.[21]

[21] Suspension of the Bank Charter Act became a regular feature of the authorities' response to future crises. Indeed,

Of more concern to this paper is the Bank's approach to lending during the heat of the crisis and the extent to which it rationed credit in the weeks leading up to the indemnity. Our disaggregated loan-level data allow us to peer beneath the aggregate interest rate data and look more closely at what the Bank was doing at a micro level.

4 Data

In the present section, we briefly describe our primary sources and data sets. Our paper exploits the wealth of detailed loan-level information the Bank of England's Discount Office began to record meticulously from the mid-1840s onwards. We focus exclusively on discount loans for two reasons. First, as we discussed above, discounts made up the bulk of loans granted by the BoE. Second, in contrast to discount loan applications, the Bank did not start recording the details of advance transactions in their daily books until 1857.

To understand how the Bank kept its books, it is necessary to explain how exactly the daily discount window interactions between the BoE and the market looked like. In the large majority of cases in 1847 (but also in other years), applications for discount loans took the form of so called 'packets' of bills, rather than individual bills. Loan applicants, i.e. discounters, submitted a packet of bills to the Discount Office. The BoE staff subsequently screened the individual bills contained in the packet and decided how many bills, if any, the Bank would accept for discount. The actual size of the loan granted by the BoE would then depend on the amount written on accepted bills minus the discount. Usually, the Bank discounted all accepted bills from a given packet at a single rate of interest.[22] Hence, strictly speaking, loan-level data in the context of the Bank's 19$^{\text{th}}$ century lending practices refers to the characteristics of a given packet submitted to the discount window.

These discount window interactions are reflected one-to-one in the Bank's ledgers which survived in the BoE's historical archives. Every single business day, the Discount Office clerks first recorded summary information on each and every submitted packet in the so called daily transactional ledgers. For each day in a given year, these ledgers contain information on the name of the discounter who applied for a loan, whether the discounter held deposit accounts with the Bank (so called Drawing Office or "D.O." customers), the loan amount applied for (before the subtraction of the discount), the

the mere anticipation of suspension was probably enough to dampen the impact of other incipient liquidity crises – for example in the crises of 1878 and 1890 there was little pressure on the Bank's note reserve.

[22]On occasion, a packet would be discounted at two rates. In 1847, around 13 percent of packets had dual rates. A glance through the ledger books after 1847 reveals that the Bank gradually decreased the number of packets which were given dual discount rates. The practice stopped in 1856.

number of bills in the submitted packet and the discount rate eventually charged for the accepted bills inside the packet. The daily transactional ledgers also document how many of the bills submitted with the packet were rejected (if any) and the ledgers even give the rejected Sterling amount (i.e. the sum of the amounts written on rejected bills). Thus, based on the information in the daily transactional ledger, it is straightforward to compute the actual loan amount received by a given discounter.

The daily transactional ledgers, however, only constituted the first step in the sophisticated loan documentation system maintained by the Bank. In a second step, the Discount Office staff recorded the accepted bills from a given packet in the so called discounter ledgers. The discounter ledgers had a separate section for each and every loan applicant who appeared at the BoE's discount window. The individual sections were organized according to the dates of loan applications put forward by the discounter. To illustrate, consider the following example. If Rothschild & Sons, a particularly prominent discounter in the London market around 1850, had submitted a packet of 30 bills on 30 March 1847, these bills would first appear as a single entry (i.e. in packet form) in the daily transactional ledger. Assuming the Discount Office decided to accept all 30 bills, these bills would then be transcribed one for one into the discounter ledger where they could be found under the section corresponding to "Rothschild & Son" and the entry for 30 March 1847. In other words, the discounter ledgers "unpack" the aggregate information from the daily transactional ledgers. The discounter ledgers record the entire set of information written on each of the accepted bills. It states the location whence the bill was originally drawn, the drawer's name, acceptor's name, the date when the bill would become due for payment and the amount on the bill.

In addition, from 25 August 1847 onwards, the Bank also maintained a ledger where staff recorded the details of rejected bills in a way almost identical to the discounter ledgers. For each day in a given year, the rejected bills ledger contains information on the discounter's, the drawer's and the acceptor's name of a rejected bill, alongside the bill's maturity date and its amount. The only difference between the discounter ledgers and the rejected bills ledger is that the latter does not state the location whence the bill was originally drawn but mentions the acceptor's location instead.[23] Hence, for example, had the Bank rejected three out of the 30 bills submitted by Rothschild & Son, the details of these three bills would have to be looked for in the rejected bills ledger, whereas the accepted 27 bills would still be recorded in the discounters ledger. We can only speculate on the reasons why the BoE started to record rejected bills on 25 August 1847 (and not before) and why the Bank documented bills it had decided to refuse in such a systematic and precise manner. Regarding the motivation to start a

[23]This slight difference between the discounter ledgers and the rejected bills ledger is enticing and we discuss it in more detail below.

rejected bills ledger on 25 August 1847, this decision is likely be related to the Bank's response to the crisis of 1847. The Bank had rejected large amounts of bills in the first eight months of 1847. Perhaps, one reason to start documenting rejected bills could have been the wish of the Discount Office to compile a set of undesirable bill characteristics to accelerate or streamline rejection decisions. Nevertheless, the recording of rejected bills appears to be a particularly costly exercise to undertake for securities the Bank was not going to purchase. Thus, another possibility is that the Bank used this information to keep track of more general developments in the financial system, e.g. to get a sense of the overall indebtedness of highly levered acceptors.

We started our data collection by transcribing all transactions (i.e. submitted packets) recorded in the 1847 daily transactional ledger books. This first step was necessary in order to get at the population of loan applications during our year of interest. In 1847, a total of 9,206 packets containing 97,637 bills were submitted to the Discount Office. In other words, assuming 305 business days and 3 hours of daily business, the Discount Office examined more then 10 packets or, equivalently, more than 100 bills per hour. To minimize transcription errors, we validated the discounter names using the Discount Office's "Index A – Z Discounter Ledgers 1847 - 1850" from the Bank's archives and we compared the daily total we recorded in the daily transactional ledgers to aggregate statistics for each day (which are also available in the ledgers). In a second step and based on our data for the entire population of loan applications in 1847, we collected several random samples. First, we drew a random sample of 1,000 packets from the underlying population, imposing the restriction that 50% of these packets were submitted in what we call "crisis weeks", with the other 500 packets being presented to the discount window in "normal" weeks. Throughout this paper, we define "crisis weeks" as those weeks where the level of notes and discounts recorded and/or the note reserve in Banking Department are more than two standard deviations from the mean. According to our definition, weeks of particularly strong financial distress thus cover the following dates in 1847: 28 March – 8 May, 26 July – 14 August, 30 August – 4 September and 13 September – 20 November. These crisis weeks are shaded in gray in Figure 1 above. As can be seen, although our crisis windows mostly overlap with the weeks in which credit rationing is strongest according to Figure 1, they are not identical. The reason we chose to focus on crisis weeks according to our definition rather than merely picking episodes of particularly strong credit rationing as identified by the rates test is threefold. First, according to the test, the BoE appears to have rationed credit almost during the whole year, leaving practically no control weeks. Second, and perhaps more subjectively, due to the skewed distribution of the market-Bank spread, choosing a threshold above which we consider credit rationing to be "severe" appeared more prone to arbitrary selection to us than our method of filtering for crisis weeks (i.e. assuming a normal distribution and applying the 95% rule). Third, as we discussed in Section 2 above, there are good reasons to distrust the accuracy of the rates test due to the lack of information regarding the transactions "hidden" behind aggregate market (and Bank) rates. We do not unpack the 1,000 packets from our first sample. Rather, as we explain below, we use them to obtain a first approximation of how important the applicant (discounter) identity was for loan decisions.

Our second random sample contains 200 packets (100 packets from crisis weeks and 100 packets from normal weeks). We sampled these 200 packets from the entire population by imposing three restrictions: 1) the submission took place between 25 August and 31 December 1847, 2) at least one of the bills in the packet was rejected, and 3) the packet did not contain more than 10 bills. On the one hand, the date restrictions are necessary for unpacking both the accepted and rejected bills in the packets (as mentioned above, the rejected bills ledger only starts on 25 August 1847). On the other hand, the restriction on the number of bills per packet is one of convenience: it limits the amount of bills we need to unpack for this second random sample. Undoubtedly, this restriction leads to an unrepresentative sample because it automatically excludes larger packets. For example, one might hypothesize that larger packets tend to be submitted by specific types of discounters (e.g. banks or bill brokers). Hence, if we had sampled the 200 packets with the purpose of analyzing the role of loan applicant identity in the BoE's credit decisions, this restriction would be highly problematic. However, as we explain below, the purpose of this second sample is precisely the contrary: we use it to fix discounter identity by comparing bills from the same discounter, submitted on the same day. This goal also explains why we only drew packets which had at least one bills rejected. This sampling design allows us to compare like with like (in terms of the discounter's identity and date fixed effects) and focus on the role of bill characteristics in the BoE's loan decisions.

The results displayed and discussed in the present version of this paper are based on these first two samples only. Our third random sample is still work in progress. For our third sample, we again drew 200 packets from the entire packet population, but this time we only imposed two rules: the date restriction (necessary for unpacking) and the crisis week (50% – 50%) restriction. We collect this third sample with the purpose of conducting a representative "horse race" between discounter identity and bill characteristics.

Before we conclude our data section, it should be noted that we draw on several other internal BoE documents and external sources to make sense and use of the recorded data on discounter, drawer and acceptor names. We mostly use dummy variables to indicate whether a given name stands for a specific quality or type of counterparty. First, we matched the names on the packets and bills with some professions that emerge as important from our discussion of the London money market in Section 3. We checked discounter, drawer and acceptor names to see whether any of these parties was a banker or bill broker. We obtained the relevant information from the *Bankers' Almanac* and the *London Post Office Directory* for 1846. Second, the *London Post Office Directory* was also helpful for coding a dummy indicating whether an acceptor was based in London or elsewhere. This information is directly available in the ledger data for rejected bills only. Since we found many of the rejected bills' acceptors to be located outside London, we suspected that the acceptor's address played a role in the BoE's loan decisions and checked the names on accepted bills against the *London Post Office Directory* for 1846. Third, we examined our list of names against a list of companies that failed in 1847. The list of failed companies was compiled from various issues of *The Economist*, which published this information periodically. Fourth, we used the Bank's so called internal rating books which contain a (separate) list of discounters and acceptors. The rating books provide information on when the counterparty listed was first "introduced"[24] to the Bank, which Bank director had introduced the name, the trade/profession of the counterparty and the credit limit the Bank had assigned to it. Interestingly, the majority of discounters and acceptors that appear in our data sets were *not* in the rating books. Furthermore, the two lists were not mutually exclusive: some names appear in both books. Altogether, only 315 discounters and 103 acceptors are listed in the discounter and acceptor rating books for 1847 respectively. We conclude that a counterparty did not necessarily have to be part of these two "clubs" to be allowed to discount or to be considered as a decent acceptor. Although we could not find any background information on the purpose and use of these rating books by the Bank, we still assume that the Bank collected and maintained these lists of names for a particular reason, for example, because it considered them to be particularly good names. Hence, we coded dummy variables for discounters, drawers and acceptors indicating whether any of the names appear in the rating books. Sixth, for discounters in our data set we also documented whether they had a deposit account running in their name. Finally, based on our data from the packet population of 1847, we compiled lists of top discounters and top acceptors as counterparties with total loans (discounted or accepted, respectively) by value more than two standard deviations from the mean.

[24]An "introduction" meant that one of the directors of the Bank had to present the counterparty to the Governing Board before it was accepted as a regular discounter at the discount window.

5 Descriptive evidence

5.1 Patterns in loan decisions during the crisis year of 1847

We begin the exploration of our loan-level data set by looking at descriptive, aggregate patterns in the BoE's decisions to accept or to refuse a loan application. Figure 2 illustrates a point we made in Section 4 above: only a small fraction of all packets submitted to the discount window in 1847 got entirely rejected (7%). Although roughly one third of the packets see some of their bills rejected, the bulk of rejections is partial (23%). Figure 2 already conveys an important message: discounter identity alone cannot be the whole explanation for bill rejections. Had it been the sole driver of loan decisions, packets should have always either been entirely rejected or accepted in full. It is important to emphasize, however, what *cannot* be concluded on the basis of Figure 2. First, the conclusion that discounter identity cannot be the whole story behind rejections does not necessarily mean that discounter identity does not matter at all. It is possible, and indeed plausible, to imagine that both the identity of the borrower and the quality of collateral played a role in the Bank's credit decisions. Second, Figure 2 does not necessarily mean that decisions to (partially) reject packets were made on the basis of bill characteristics. For example, pure credit rationing could be compatible with the data illustrated in Figure 2: the Bank might have always rejected bills or entire packets in moments of excess demand, proceeding more or less randomly in its decisions to refuse credit.

Figure 2: Packets submitted to the Bank of England's discount window in 1847 (N=9,206)

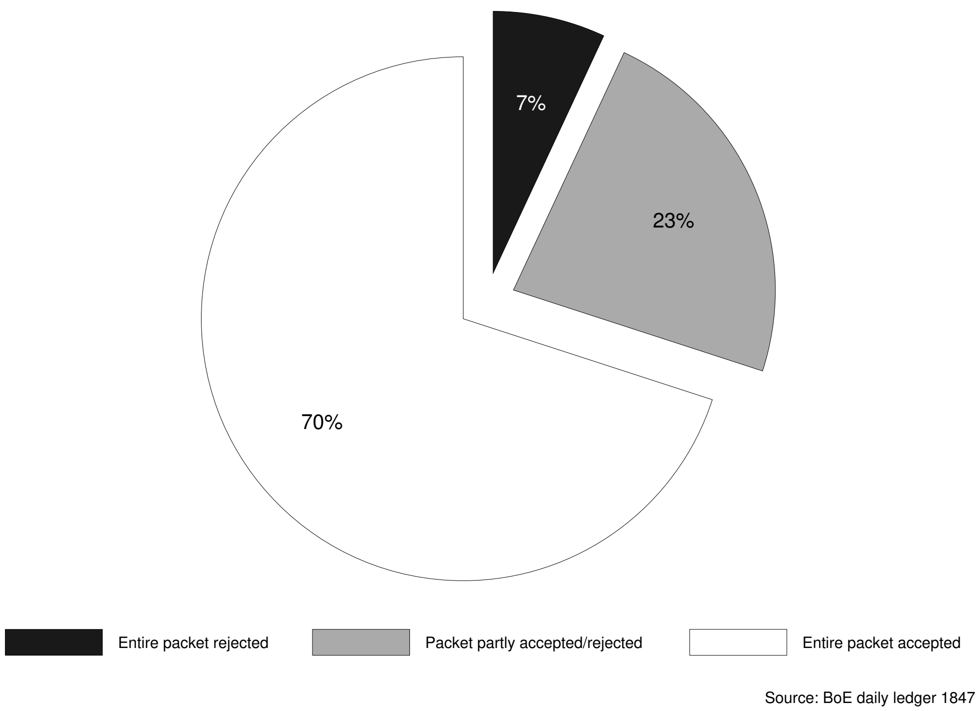


Figure 3: Packets submitted to the Bank of England's discount window in 1847 (N=9,206; 119 crisis days out of 310 days)

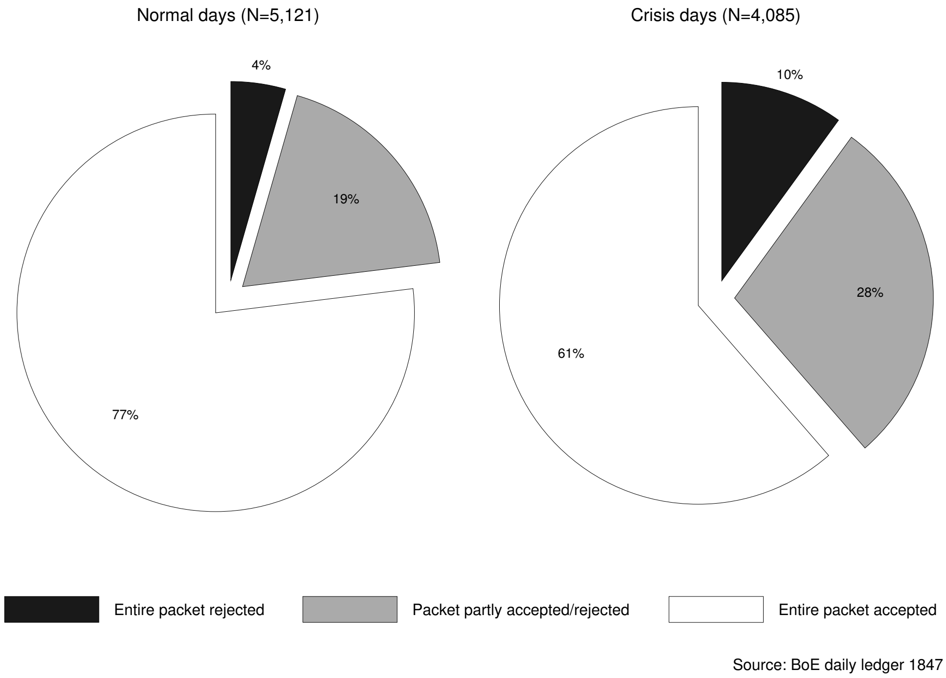


Figure 4: Number of Bills submitted to the Bank of England's discount window in 1847 (N=97,637; 119 crisis days out of 310 days)

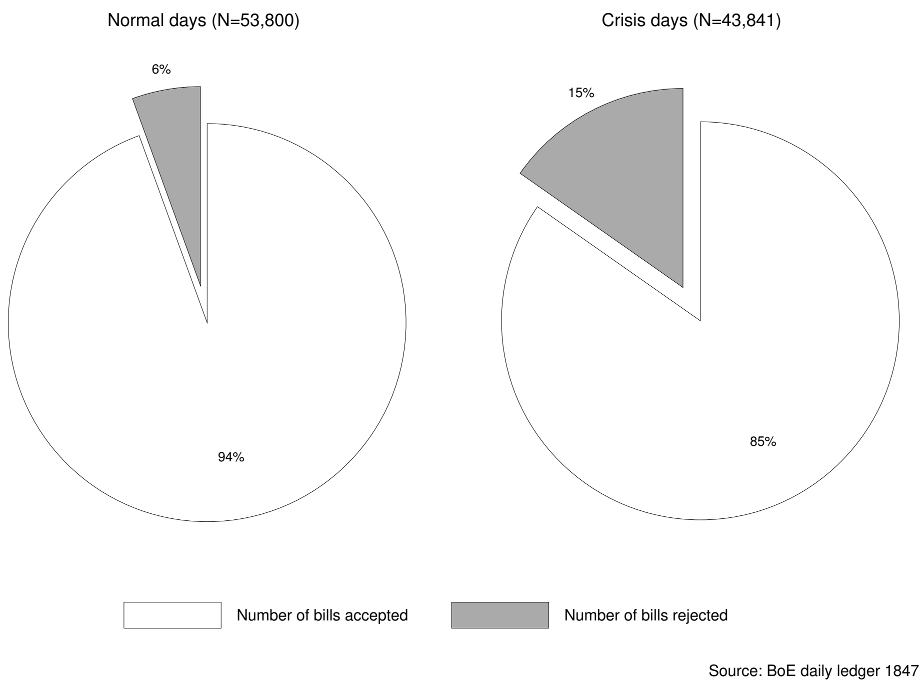
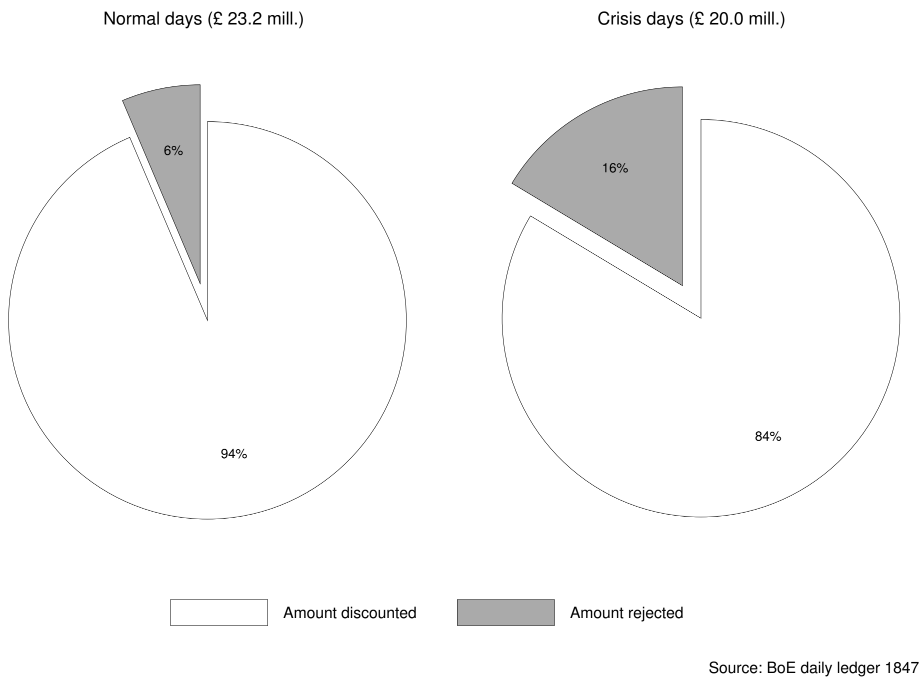


Table 2: T-tests: rejections of packets, bills and amount during crisis days vs. normal days in 1847

Period	**Total days/obs.**	**Total packet rejected**	**Part of packet rejected**	**Bills rejected**	**Amount rejected**
	Days	Count	Count	Count	Sum (in £)
Normal days	191	229	956	3,052	1,452,458
Crisis days	119	414	1,160	6,713	3,285,804
	Observations	Mean (share of total packets)	Mean (share of total packets)	Mean (rejected to total submitted)	Mean (rejected to total submitted)
Normal days	5,121	0.04	0.19	0.10	0.11
Crisis days	4,085	0.10	0.28	0.20	0.21
t-statistic		-10.65***	-11.09***	-15.74***	-15.63***

*** p<0.01, ** p<0.05, * p<0.1 (null of equal means)
Source: BoE daily ledger 1847

Figure 5: Monetary value on bills submitted to the Bank of England's discount window in 1847 (total of £ 43.1 mill.; 119 crisis days out of 310 days)



In this paper, we are interested in comparing the dynamics at the discount window in crisis weeks and normal weeks. Figure 3 draws on the same data used for Figure 2 but splits the population of loan decisions into crisis and normal times. Figures 4 and 5 follow the same approach, but look at the number of and amount on bills rejected. It illustrates clearly that both the share of packets that get entirely rejected and the share of packets which receive partial rejections are substantially higher in crisis weeks than during times of relative calm. Perhaps unsurprisingly, the Bank discounted more bills and a higher total amount on average crisis days than it does on average normal days (c.f. Figure 4 and 5). Yet, relative to normal times, the Bank rejects more packets (Figure 3), more bills (Figure 4) and higher amounts of the total amount requested (Figure 5) during crisis days. These findings are confirmed by more formal statistical mean equality tests in Table 2. Finally, Figure 4 and 5 suggest that the percentage share of bills rejected is a good proxy for the percentage share of monetary value rejected by the Bank. This observation implies that the individual bills all appear to have been more or less similar in terms of size. In the appendix, we provide an additional monthly break-down of packet and bill rejections (Figures 7 to 9). The relative amount of rejections varies quite a bit throughout the year but nicely follows the pattern of crisis weeks we have mentioned in Section 4.

Overall, this series of descriptive evidence leaves no doubt that crisis weeks (or days falling into crisis weeks) were special: on average, loan applications had a higher probability of being rejected in times of financial distress. In the next subsection, we begin to analyze why we observe these precise dynamics: what explains credit restrictions during crisis weeks?

5.2 Mean equality tests at the packet- and bill-level

We now implement some simple mean tests to shed more light on the drivers behind the BoE's decision to (partially) reject demands for credit. Most of the variables for which mean equality is tested in this subsection should be either self-explanatory or have been explained in Section 4. However, some variables for the bill-level tests have not been discussed yet and we will explain their meaning in the paragraphs below. As mentioned above, the results in this paper are based on our first two random samples only: the packet-level sample and the restrictive bill-level sample.

We start our tests at the level of packets. In normal weeks (c.f. Table 3), partially rejected packets were statistically different from those that were accepted by the BoE along the following criteria: entirely accepted packets contained fewer bills and were less likely to be submitted on busy days, when more bills were submitted to the discount window in general. Moreover, packets with rejections were less likely to have been submitted by a Drawing Office customer, but were more likely to be have been submitted by a banker or a company that failed in 1847 (all of these results are reflected in mean tests for the pooled sample, c.f. Table 15 in the appendix). In crisis weeks (c.f. Table 4), however, all of these statistical differences disappear, with the exception of the dummy reflecting discounters that failed in 1847: packets submitted by discounters that failed in 1847 were still more likely to be (partially) rejected in crisis weeks. Thus, this first set of tests suggests that, at the level of packets, there was only one rule for (partial) rejections that the BoE applied consistently both in crisis and non-crisis weeks in 1847. One could interpret this result in two ways: either the rules applied in crisis times were different from those applied in normal weeks and the packet-level variables do not capture these new rules very well (DR hypothesis), or the general lack of statistical differences in the characteristics of (partially) rejected packets and fully accepted packets in crisis times points to the PR hypothesis.

When we test for differences in the characteristics of all submitted packets between crisis weeks and non-crisis weeks (c.f. Table 5), we find that none of the characteristics that could be considered proxies for packet quality (for example, the share of loan applicants who were a top discounter or who failed in 1847) were statistically different, although clearly a higher share of all packets was (partially) rejected during crisis weeks (which reflects the figures presented in the preceding subsection) and the number of bills submitted as well as the amount of credit demanded were much higher in weeks of

Table 3: T-tests on packets - rejected vs accepted (in normal weeks)

Variable	Packets with rejections (Obs)	Packets with rejections (Mean)	All accepted (Obs)	All accepted (Mean)	Two-sided p-value
Total number of bills on day (ln)	122	3.43	378	3.35	0.02**
Total value of bills on day (ln)	122	11.79	378	11.71	0.14
Packet's rank on day (chronological)	122	0.48	378	0.51	0.34
Packet's rank on day (value)	122	0.55	378	0.51	0.27
Packet's total value	122	7.81	378	7.71	0.43
Packet's total bills	122	1.94	378	1.68	0.03**
Dummy - discounter with DO account	122	0.03	378	0.11	0.01**
Dummy - discounter in rating book	122	0.30	378	0.29	0.84
Dummy - discounter in acceptor book	122	0.05	378	0.10	0.11
Dummy - discounter is banker	122	0.02	378	0.01	0.06*
Dummy - discounter is bill broker	122	0.01	378	0.03	0.16
Dummy - discounter fails in 1847	122	0.11	378	0.06	0.04**
Dummy - discounter is top discounter	122	0.02	378	0.02	0.63

Table 4: T-tests on packets - rejected vs accepted (in crisis weeks)

Variable	Packets with rejections (Obs)	Packets with rejections (Mean)	All accepted (Obs)	All accepted (Mean)	Two-sided p-value
Total number of bills on day (ln)	177	3.58	323	3.56	0.46
Total value of bills on day (ln)	177	11.97	323	11.95	0.83
Packet's rank on day (chronological)	177	0.51	323	0.51	0.96
Packet's rank on day (value)	177	0.53	323	0.54	0.90
Packet's total value	177	7.79	323	7.79	0.99
Packet's total bills	177	1.85	323	1.69	0.11
Dummy - discounter with DO account	177	0.08	323	0.12	0.25
Dummy - discounter in rating book	177	0.33	323	0.30	0.45
Dummy - discounter in acceptor book	177	0.06	323	0.07	0.61
Dummy - discounter is banker	177	0.02	323	0.01	0.23
Dummy - discounter is bill broker	177	0.06	323	0.03	0.11
Dummy - discounter fails in 1847	177	0.10	323	0.04	0.02**
Dummy - discounter is top discounter	177	0.02	323	0.04	0.30

financial distress. Therefore, this second set of tests also seems to yield evidence against the RBR hypothesis because the actual quality characteristics of packets do not vary in normal versus crisis weeks. However, no clear statement on either the DR hypothesis or the PR hypothesis can be made.

The packet-level test results need to be interpreted with a grain of salt though, since the BoE took all its decisions at the bill-level, and not for packets. Hence, we implement the same sort of tests again for our bill-level sample to check whether the results we obtain are different once we consider the unit for which individual decisions were taken. Most variables at the bill-level are defined in analogy to the packet-level covariates (using indicator variables as explained in Section 4), but we add a handful of new ones. First, we we add three variables reflecting maturity: the days to maturity (counting from the day of submission), the spread from the mean remaining days to maturity of accepted bills (approximately 60 days), and a dummy for rest maturities greater than 95 days which the Bank is said

Table 5: T-tests on packets - normal times vs crisis times

Variable	Normal (Obs)	Normal (Mean)	Crisis (Obs)	Crisis (Mean)	Two-sided p-value
Rejection dummy (at least 1 bill rejected)	500	0.24	500	0.35	0.00***
Total number of bills on day (ln)	500	3.37	500	3.56	0.00***
Total value of bills on day (ln)	500	11.73	500	11.96	0.00***
Packet's total value	500	7.74	500	7.78	0.47
Packet's total bills	500	1.74	500	1.74	0.97
Dummy - discounter with DO account	500	0.09	500	0.11	0.34
Dummy - discounter in rating book	500	0.29	500	0.31	0.41
Dummy - discounter in acceptor book	500	0.08	500	0.07	0.41
Dummy - discounter is banker	500	0.01	500	0.01	0.56
Dummy - discounter is bill broker	500	0.03	500	0.04	0.28
Dummy - discounter fails in 1847	500	0.07	500	0.06	0.53
Dummy - discounter is top discounter	500	0.02	500	0.03	0.25

to have considered its absolute upper threshold. Second, we add an indicator variable for bills which bore the name "P.N." instead of a proper counterparty name (e.g. instead of the acceptor's name) in the discount or rejected bills ledger. Although there is no detailed description, we hypothesize that "P.N." was an abbreviation used to describe promissory notes, i.e. bills with only one other name apart from the discounter. Third, we coded combinations of counterparty names that seemed to show that the same person or firm figured as a discounter and acceptor or drawer and acceptor simultaneously. We found in particular that the discounter's and the acceptor's name were frequently the same. From the point of view of the Bank, promissory notes and bills showing twice the same counterparty should have been less attractive because they only bore two instead of the standard three signatures with joint liability (Santarosa, 2015). A fourth additional variable is an indicator variable for bills which had their acceptor's name recorded as "directors". We believe that the Discount Office referred to BoE directors in these cases and we created a dummy to check whether the Bank granted preferential treatment to its management.

Similar to the packet-level results, mean equality tests based on data from non-crisis weeks (c.f. Table 6) suggest that there were clear, systematic differences between accepted and rejected bills. These differences spanned a wide variety of characteristics ranging from maturity, amount, to names as well as types of signatories on the bills. In stark contrast to the the packet-level results, however, we find that most of these systematic differences also remain statistically significant when we concentrate on our subsample for crisis weeks (c.f. Table 7). At first sight, a comparison between Table 6 and Table 7 therefore suggests that the underlying drivers motivating the decision to accept/reject a bill were very similar during both crisis episodes and normal weeks. A priori, these two sets of tests at the bill-level yield results that are consistent with the RBR hypothesis. Nevertheless, the existence of rules could also be consistent with the DR hypothesis, as long as some rules change or are applied differently in crisis weeks. A detailed look at Table 6 and Table 7 shows that some minor differences exist: the BoE appears to have disliked larger loan applications during crisis weeks more than in normal times, while being less strict on maturity-related variables during episodes of distress.[25] Hence, although pure credit rationing seems to be unlikely, a clear-cut conclusion cannot be drawn between the RBR and the DR hypothesis. When comparing the quality of bills submitted in crisis weeks to those submitted in normal weeks (c.f. Table 8), we find some statistically significant differences but these differences do not allow for a clear conclusion that bills presented in weeks of financial distress were generally characterized by lower quality: in fact, the results partly suggest that bills submitted during

[25]This finding is interesting because it contradicts anecdotal evidence from *The Economist* cited by Bignon et al. (2012), suggesting that the BoE discriminated against longer maturity bills in 1847.

Table 6: T-tests on bills - rejected vs accepted (in normal weeks)

Variable	Rejected bills (Obs)	Rejected bills (Mean)	Accepted bills (Obs)	Accepted bills (Mean)	Two-sided p-value
Days to maturity (ln)	176	4.15	336	3.87	0.00***
Spread from mean maturity of 60 days	177	-9.08	337	3.17	0.00***
Dummy - maturity>95days	177	0.01	377	0.01	0.80
Amount on bill (ln)	177	5.47	377	5.30	0.06*
Dummy - promissory note	177	0.08	377	0.04	0.02**
Dummy - drawer or acceptor failed	177	0.01	377	0.00	0.64
Dummy - drawer or acceptor bill broker	177	0.01	377	0.01	0.97
Dummy - drawer or acceptor DO	177	0.01	337	0.01	0.69
Dummy - acceptor in London	177	0.49	337	1.00	0.00***
Dummy - discounter=acceptor	177	0.68	337	0.47	0.00***
Dummy - drawer=acceptor	177	0.01	337	0.03	0.14
Dummy - acceptor=directors	177	0.00	377	0.03	0.02**
Dummy - acceptor=bank	177	0.00	377	0.12	0.00***
Dummy - acceptor=top acceptor	177	0.00	377	0.06	0.00***
Dummy - acceptor=top discounter	177	0.00	337	0.02	0.07*
Dummy - acceptor in rating book	177	0.01	337	0.13	0.00***
Dummy - acceptor in acceptor book	177	0.00	377	0.08	0.00***
Dummy - drawer=bank	177	0.00	377	0.10	0.00***
Dummy - drawer in rating book	177	0.34	377	0.27	0.09*
Dummy - drawer in acceptor book	177	0.03	377	0.02	0.44

Table 7: T-tests on bills - rejected vs accepted (in crisis weeks)

Variable	Rejected bills (Obs)	Rejected bills (Mean)	Accepted bills (Obs)	Accepted bills (Mean)	Two-sided p-value
Days to maturity (ln)	191	4.00	354	3.87	0.03**
Spread from mean maturity of 60 days	192	-0.47	354	2.74	0.16
Dummy - maturity>95days	192	0.01	354	0.01	0.95
Amount on bill (ln)	192	5.71	354	5.47	0.01***
Dummy - promissory note	192	0.02	354	0.00	0.02**
Dummy - drawer or acceptor failed	192	0.02	354	0.01	0.24
Dummy - drawer or acceptor bill broker	192	0.00	354	0.00	0.46
Dummy - drawer or acceptor DO	192	0.01	354	0.01	0.67
Dummy - acceptor in London	192	0.59	354	1.00	0.00***
Dummy - discounter=acceptor	192	0.58	354	0.35	0.00***
Dummy - drawer=acceptor	192	0.02	354	0.01	0.67
Dummy - acceptor=directors	192	0.00	354	0.17	0.07*
Dummy - acceptor=bank	192	0.00	354	0.14	0.00***
Dummy - acceptor=top acceptor	192	0.00	354	0.09	0.00***
Dummy - acceptor=top discounter	192	0.00	354	0.11	0.14
Dummy - acceptor in rating book	192	0.03	354	0.19	0.00***
Dummy - acceptor in acceptor book	192	0.01	354	0.08	0.00***
Dummy - drawer=bank	192	0.00	377	0.08	0.00***
Dummy - drawer in rating book	192	0.31	354	0.18	0.00***
Dummy - drawer in acceptor book	192	0.06	354	0.03	0.06*

crisis windows were on average of higher quality (e.g. "dummy - promissory note" and "dummy - discounter=acceptor") while others (e.g. "amount on bill (ln)") are not really informative with regard to the underlying quality of the bill. Therefore, the tests in Table 8 point towards a rejection of the RBR hypothesis.

To mention two important caveats for the bill-level tests we conduct in this paper, it should be noted first that the tests in Tables 6 - 8 do not account for different discounter identities. The tests only compare rejected to accepted bills (or bills submitted in crisis versus normal weeks), without stratifying for the underlying borrower. This lack of stratification might impact the test results: for example, although we find a general tendency for rejected bills to be longer maturity securities, maturity might be strongly correlated with borrower identity. What is really needed to make valid inferences at the bill-level is a statistical approach that allows us to compare likes with likes: we need to fix the borrower identity when comparing rejected and accepted bills. The second caveat concerns our sampling strategy: since the bills were sampled and unpacked with a specific goal in mind (c.f.

Table 8: T-tests on bills - normal times vs. crisis times

Variable	Normal (Obs)	Normal (Mean)	Crisis (Obs)	Crisis (Mean)	Two-sided p-value
Rejected bill	514	0.34	546	0.35	0.80
Days to maturity (ln)	514	3.96	546	3.91	0.24
Spread from mean maturity of 60 days	514	-1.05	546	1.60	0.12
Dummy - maturity>95days	514	0.01	546	0.01	0.43
Amount on bill (ln)	514	5.36	546	5.55	0.00***
Dummy - promissory note	514	0.05	546	0.01	0.00***
Dummy - drawer or acceptor failed	514	0.00	546	0.01	0.29
Dummy - drawer or acceptor bill broker	514	0.01	546	0.00	0.29
Dummy - drawer or acceptor DO	514	0.01	546	0.01	0.93
Dummy - acceptor in London	514	0.82	546	0.86	0.15
Dummy - discounter=acceptor	514	0.54	546	0.43	0.00***
Dummy - drawer=acceptor	514	0.03	546	0.01	0.14
Dummy - acceptor=directors	514	0.02	546	0.01	0.26
Dummy - acceptor=bank	514	0.08	546	0.09	0.64
Dummy - acceptor=top acceptor	514	0.04	546	0.06	0.11
Dummy - acceptor=top discounter	514	0.01	546	0.01	0.46
Dummy - acceptor in rating book	514	0.09	546	0.13	0.02**
Dummy - acceptor in acceptor book	514	0.05	546	0.05	0.93
Dummy - drawer=bank	514	0.06	546	0.05	0.52
Dummy - drawer in rating book	514	0.29	546	0.23	0.01**
Dummy - drawer in acceptor book	514	0.02	546	0.04	0.14

Section 4), the results might only be representative for one particular category of bills, namely those bills that are usually contained in packets with ten or less bills.

Overall, the mean equality tests at the packet- and the bill-level we implemented above are only fully consistent with the DR hypothesis. The BoE appears to have adopted somewhat different priorities for motivating decisions to grant loans in crisis weeks versus normal times. At the same time, there is no evidence for the hypothesis that crisis weeks saw lower quality applications than weeks of calm. It should be noted, however, that none of these tests is fully conclusive because they lack a *ceteris paribus* perspective and face selection issues. In the next section, we start to address these problems: more precisely, we conduct some *ceteris paribus* analyses using simple econometrics. Considerably more work, i.e. the completion of sample three mentioned in Section 4, is needed to deal with potential selection bias.

6 Econometric analysis

In this section, we formalize our mean equality tests using regression analysis. We thus implement several of the remaining tests summarized in Table 1. In a first subsection, we briefly describe our estimation equations and the precise estimation techniques we draw upon. We then turn to our packet-level regression results before discussing the regressions on our bill-level data.

6.1 Econometric models

In this paper, we employ three different econometric baseline models to take into account the different types of data and specifications we use. For the packet-level sample drawn from the daily transactional ledgers, we follow two complementary approaches. First, we estimate a simple discrete choice model in a logit regression framework. The dependent variable, R_p, for this first set of regressions is a binary variable indicating whether a given packet p had at least one of its bills rejected. This dummy variable does not discriminate between those packets which had many rejections (or were completely rejected) and packets from which only a single bill was refused. We thus estimate the probability that a packet had at least one rejection. The explanatory variables for the logit regressions, $\mathbf{X}_p$, are exactly the same covariates we employed for our mean equality tests in Section 5 above. We report summary statistics for the dependent variable and the covariate vector $\mathbf{X}_p$ in the next subsection. Model 1 summarizes our binary regression equation:

$$P(R_p|\mathbf{X_p}) = F(\alpha + \mathbf{\Gamma}'\mathbf{X}_p) \tag{1}$$

where $F(u = \alpha + \mathbf{\Gamma}'\mathbf{X}_p) = \frac{exp(u)}{1+exp(u)}$

where R_p is the dependent variable, a dummy that takes the value of 1 if packet p was (partially) rejected by the Bank (and zero otherwise) and $\mathbf{X_p}$ is the vector of packet-level characteristics we have collected and drawn upon for our mean equality tests in Section 5 above.

There are good reasons to believe that running a simple binary regression as in Model 1 might not be very informative in the case of packet-level data. A simple example illustrates this point. Given the way we code our dependent variable, a packet of 100 bills which had only a single bill rejected and a packet of two bills which had both components refused receive the same indicator value (i.e. one). This coding choice might spuriously associate some of our counterparty identity dummies with higher probabilities of rejections because, for example, bankers and bill brokers tended to submit large packets which often had some bills rejected, although the refused bills constituted only a small share of the total packet. To address this potential pitfall, we estimate another set of packet-level regressions with continuous dependent variables (S_p) representing the share of rejected bills relative to the total number of bills in packet p and, alternatively, the share of the amount on rejected bills relative to the total value of packet p. The shares constitute classic cases of limited dependent variables (taking values between 0 and 1 only) which should be explicitly treated in this way to avoid violations of the conditional normal distribution assumption and negative predicted values. We therefore estimate this second category of packet-level regressions using a Tobit model of the following form:

$$S^*_p = \alpha + \mathbf{\Gamma}'\mathbf{X}_p + \varepsilon_p | \mathbf{X}_p \sim Normal(0, \sigma^2) \tag{2}$$

$$\text{where } S_p = max(0, S^*_p), min(1, S^*_p)$$

and S^*_p represents the share of rejected bills relative to the total number of bills in packet p or the share of the amount on rejected bills relative to the total value of packet p. S^*_p is a latent variable that satisfies classical linear model assumptions. $\mathbf{X}_p$ is defined as before.

Models 1 and 2 represent our baseline equations. We first estimate these equations in the simple specification presented above. Second, we also estimate more demanding specifications including week fixed effects to purge the coefficients of any non-structural relationships with the dependent variable which are due to special events. Furthermore, proceeding as with our mean equality tests above, we split the packet-level sample into observations from crisis weeks and observations from normal weeks to check for systematic differences in the determinants of the BoE's loan decisions. In order to correct for potential clustering of standard errors around specific dates, we estimate Models 1 and 2 with clustered standard errors for days and weeks in addition to a non-clustered, standard specification.

Turning to our bill-level sample, we estimate binary choice regressions similar to Model 1 above, but we draw on a slightly different estimation technique. Discrete choice models are a must for our bill-level data because our dependent variable is binary by definition: a given bill b is either accepted or rejected. However, as we explained above, we have collected our second sample with a specific goal in mind: our aim is to test the predictive power of bill characteristics after fixing both discounter identity and date. In a binary choice setting, this objective is achieved econometrically by relying on so called conditional (fixed effect) logistic regression. Conditional logit is an estimation technique explicitly designed for matched case-control data that fits our purpose: we match, i.e. compare, rejected and accepted bills from a single packet submitted by a single discounter on a given day to determine the bill-level drivers of BoE decisions. Hence, we use Model 3 below to purge our bill-level data from underlying discounter and date effects. Akin to fixed effects or first difference estimators in panel data environments, the conditional logistic estimator does not explicitly estimate the fixed effects and thus conveniently saves degrees of freedom:

$$P(R_b | \mathbf{X_b}, D_d) = G(\alpha + \mathbf{\Gamma}'\mathbf{X}_b + \delta D_d) \tag{3}$$

$$\text{where } G(z = \alpha + \mathbf{\Gamma}'\mathbf{X}_b + \delta D_d) = \frac{exp(z)}{1+exp(z)}$$

where R_b is the dependent variable, a dummy that takes the value of 1 if bill b was rejected by the Bank (and zero otherwise). $\mathbf{X_b}$ stands for the vector of bill-level characteristics we have collected and drawn upon for our mean equality tests in Section 5 above and D_d is a discounter-date fixed effect which is controlled for but not explicitly estimated.

In our bill-level regressions, we cannot (or rather, we do not need to) control for week fixed effects because the conditional logistic estimator automatically implements date fixed effects when we only compare bills from a single packet. In order to estimate correct standard errors in the fixed effects logit, we need to cluster standard errors at the packet-level (i.e. for all bills belonging to a single packet) to control for intra-group correlation and the matched case-control structure of our data. As for our packet-level regression, we also split the sample to estimate coefficients separately for crisis days and normal days in 1847.

6.2 Packet-level regressions: credit rationing and the role of discounter identity

We begin the discussion of our econometric results with the packet-level regressions. Table 9 provides the summary statistics for R_p, the two specifications of S_p and vector $\mathbf{X}_p$ for our sample drawn from the daily transactional ledgers. In Table 10, we report the results for our packet-level logit regressions using a simple dummy as dependent variable. Table 11 and Table 12 report our Tobit regression results, drawing on the share of rejected bills and the share of the rejected amount as dependent variables respectively.

Before we discuss the coefficients and our interpretation of the results in detail, some general remarks are warranted. First, we report general goodness-of-fit measures at the end of the results tables. Overall, judging by the pseudo R-squared measure, we only explain a small fraction of the variation in our dependent variables with the logit and Tobit models. The AUC (area under the curve) statistic, commonly reported as an alternative goodness-of-fit measure for binary choice models, confirms that our regressions do not cross the 0.8 threshold frequently taken to represent a good fit. In the split specifications including week fixed effects (column 7 and 8 of the three results tables respectively), we reach a maximum R-squared of 18%. The fact that we only explain a fraction of the variation can mean either or both of two things. On the hand, we could be lacking relevant explanatory variables in our covariate vector $\mathbf{X}_p$. On the other hand, the low goodness-of-fit statistics could merely

Table 9: Summary statistics for packet-level regressions

Variable	Obs	Mean	Std. Dev.	Min	Max	P1	P5	P25	P50	P75	P95	P99
Rejection dummy (at least 1 bill rejected)	1000	0.30	0.46	0.00	1.00	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Share of rejected bills	1000	0.15	0.29	0.00	1.00	0.00	0.00	0.00	0.00	.13	1.00	1.00
Share of rejected amount	1000	0.16	0.31	0.00	1.00	0.00	0.00	0.00	0.00	0.14	1.00	1.00
Total number of bills on day (ln)	1000	3.47	0.34	2.48	4.23	2.71	2.94	3.22	3.43	3.71	4.04	4.19
Total value of bills on day (ln)	1000	11.84	0.60	10.31	13.26	10.65	10.93	11.39	11.83	12.33	12.87	13.08
Packet's rank on day (chronological)	1000	0.51	0.29	0.02	1.00	0.03	0.06	0.25	0.50	0.76	0.97	1.00
Packet's rank on day (value)	1000	0.53	0.29	0.02	1.00	0.03	0.07	0.27	0.53	0.79	0.97	1.00
Packet's total value	1000	7.76	1.16	4.70	12.32	5.30	5.81	6.94	7.74	8.58	9.52	10.87
Packet's total bills	1000	1.74	1.12	0.00	5.60	0.00	0.00	1.10	1.79	2.40	3.66	4.75
Dummy - discounter with DO account	1000	0.10	0.30	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - discounter in rating book	1000	0.30	0.46	0.00	1.00	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Dummy - discounter in acceptor book	1000	0.08	0.27	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - discounter is banker	1000	0.01	0.11	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00
Dummy - discounter is bill broker	1000	0.03	0.18	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00
Dummy - discounter fails in 1847	1000	0.07	0.25	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - discounter is top discounter	1000	0.03	0.17	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00

reflect a point we have raised in Section 5 above: the Bank decided whether to accept or reject bills, not packets. From this perspective, it might be expected that we cannot explain a whole lot of variation using packet-level covariates and associated discounter characteristics, simply because what mostly mattered for individual rejections, i.e. bill-level characteristics, is omitted from the regressions. To be sure, we would caution against interpreting these remarks concerning the goodness-of-fit as evidence confirming the "frosted glass" hypothesis proposed by Capie (2002): we might indeed have missed variables which should be included in $\mathbf{X}_p$ and, more importantly, the "raised eyebrow" hypothesis does not require that discounter identity explains the majority, let alone all of the variation in loan decisions. From the perspective of Flandreau and Ugolini (2011, 2013, 2014), the discounter identity should have mattered, at least on the margin, but the authors remain agnostic just how essential it was relative to the characteristics of individual bills. Ultimately, a test of the two hypotheses needs to consider the predictive power of individual covariates.

Second, it should be noted that all the coefficients on continuous variables in our results tables are marginal effects reported for standardized versions of these covariates. In other words, Tables 10, 11 and 12 – but also the bill-level results displayed in the next subsection – show the marginal effect on the probability of rejection or on the share of rejections of a one standard deviation increase in the covariate. This standardization procedure makes the marginal effects of covariates directly comparable to each other (within the same models). For all dummy variables, the marginal effect is calculated for a discrete change from zero to one.

Third, we would like to stress that our three major robustness checks do not change the interpretation and conclusions we draw even from the simplest of our models. As mentioned in the previous subsection, we cluster our standard errors at different levels in order to check the robustness of results to intra-group correlation. The first three columns in Tables 10, 11 and 12 always represent regression

Table 10: Logit regressions (packet-level): (partially) rejected packets vs accepted packets

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total number of bills on day (ln)	0.05**	0.05**	0.05**	-0.00	0.05	0.03	0.04	-0.06
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.04)
Total value of bills on day (ln)	-0.01	-0.01	-0.01	-0.01	-0.04	0.04	-0.08**	0.10***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Packet's rank on day (chronological)	-0.01	-0.01	-0.01	-0.01	-0.00	-0.01	-0.01	-0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Packet's rank on day (value)	0.01	0.01	0.01	-0.00	-0.07	0.10	-0.08	0.08
	(0.05)	(0.04)	(0.05)	((0.05)	(0.07)	(0.07)	(0.08)	(0.06)
Packet's total value (ln)	-0.05	-0.05	-0.05	-0.04	0.02	-0.13*	0.04	-0.11*
	(0.05)	(0.05)	(0.05)	((0.05)	(0.08)	(0.07)	(0.08)	(0.07))
Packet's total bills (ln)	0.06***	0.06***	0.06***	0.07***	0.07**	0.05**	0.08**	0.06**
	(0.02)	(0.02)	(0.02)	((0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Dummy - discounter with DO account	-0.10**	-0.10**	-0.10**	-0.07	-0.05	-0.16***	-0.03	-0.15*
	(0.05)	(0.05)	(0.04)	(0.05)	(0.07)	(0.05)	(0.07)	(0.09)
Dummy - discounter in rating book	0.04	0.04	0.04	0.04	0.07	0.02	0.05	0.02
	(0.04)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)
Dummy - discounter in acceptor book	-0.09*	-0.09**	-0.09**	-0.08	-0.05	-0.13***	-0.07	-0.18**
	(0.05)	(0.04)	(0.05)	(0.06)	(0.08)	(0.05)	(0.09)	(0.08)
Dummy - discounter is banker	0.29**	0.29**	0.29**	0.30**	0.20	0.45**	0.19	0.47***
	(0.15)	(0.13)	(0.14)	(0.14)	(0.19)	(0.18)	(0.16)	(0.16)
Dummy - discounter is bill broker	0.10	0.10	0.10	0.19**	0.34**	-0.17**	0.39***	-0.12
	(0.11)	(0.09)	(0.10)	(0.09)	(0.14)	(0.07)	(0.15)	(0.20)
Dummy - discounter fails in 1847	0.21***	0.21***	0.21***	0.16***	0.27***	0.14*	0.24***	0.11*
	(0.07)	(0.06)	(0.06)	(0.05)	(0.08)	(0.08)	(0.07)	(0.07)
Dummy - discounter is top discounter	-0.14*	-0.14*	-0.14*	-0.24*	-0.29***	0.10	-0.51***	0.12
	(0.08)	(0.08)	(0.08)	(0.13)	(0.06)	(0.21)	(0.17)	(0.14)
Observations	1,000	1,000	1,000	956	500	500	489	456
Sample	Total	Total	Total	Total	Crisis	Normal	Crisis	Normal
Week FE	No	No	No	Yes	No	No	Yes	Yes
Clustered SE	No	Week	Day	Day	Day	Day	Day	Day
Pseudo R-squared	0.04	0.04	0.04	0.13	0.04	0.06	0.12	0.18
AUC	0.63	0.63	0.63	0.74	0.62	0.60	0.70	0.66

Dependent variable: probability of (partial) rejection; (robust) standard errors in parentheses
Marginal effects for one stand. dev. increase in covariate (except for discrete variables, when change from 0 to 1)
*** p<0.01, ** p<0.05, * p<0.1

results for the entire sample of 1000 packets and differ merely with regard to how the standard errors are computed. Column 1 reports default standard errors, column 2 clusters standard errors for each week, and column reports the results for clusters at the day-level. Since the results are virtually unchanged by the clustering choices, we estimate the remaining models in columns 4 to 8 with clusters at the day-level. We also add week fixed effects to the total sample regression in column 4 and to the split sample regressions in column 7 and 8 of each table. As can be seen, the large majority of coefficients remains unchanged following the inclusion of week fixed effects. Thus, the main conclusions we draw from our packet-level regressions are unaffected by this robustness check. Finally, when we compare the sign and significance of marginal effects in our logit to the two Tobit models, we find that all our results and interpretations hold up.

Focusing on the simple logit model, the substantive regression results for our packet-level data can be summarized as follows. The full sample specifications without fixed effects consistently show that both, the total number of bills submitted with all packets on a given day and inside the packet we examine, are associated with a statistically significant increase in the probability of rejection by 5-6% per standard deviation increase. Discounter identity appears to matter too: discounters with Drawing Accounts, acceptor book entries and top discounters on average appear 10-14% less likely to see their packet (partially) rejected. Perhaps even more interesting, the marginal effects on our banker dummy is statistically significant and large, suggesting that this class of discounters faced severe negative discrimination. Bankers are associated with a 30% higher probability of receiving (partial) rejections. Of similar size, the marginal effect on our dummy flagging discounters that failed in 1847 suggest a statistically highly significant increase of 20% in the likelihood of rejections. A list of covariates, however, consistently show no effect at all. The chronological or value rank of a packet on a given day appear to not have mattered for loan decision. Moreover, the total amount of submissions on a given day and the total amount of the examined packet did not influence the likelihood of (partial) rejections. Bill brokers and discounters with rating book entries do not emerge as having experienced any special treatment by the Bank in our full sample specifications. Finally, the inclusion of week fixed effects induces some minor changes in our full sample results.[26] First, as might have been expected, the total number bills submitted on a given day is no longer a significant predictor once we control for more general time trends week by week. Interestingly, the Drawing Office dummy and the acceptor book dummy also lose significance, while bill brokers now appear to have experienced negative discrimination. We take this results as an incentive to dig deeper: if time trends are important, maybe we can make more sense of these changes in significance and coefficient size once we split our sample into crisis weeks and normal weeks.

Before turning to the split sample specifications and an interpretation of our results with regard to the three credit rationing hypotheses, we would like to emphasize a crucial point. All the marginal effects discussed above are not to be confused with causal effects. We are well aware of the threats to causal identification in the context of our study, where the danger of omitted variable looms large. Simultaneity bias is not ruled out either: in fact, the dummy variable for failed companies in 1847 is a classic candidate in this regard. While the Bank might have discriminated against companies it suspected to be particularly weak, it might have also precipitated their failure.[27] As a corollary, we consider the point estimates as correlations, and would like to seem them interpreted as such.

[26]Due to multicollinearity, we lose some observations when including the full set of week dummies.

[27]In this context, we are currently working on an identification strategy that will allow us to address the problem of reverse causality. We are looking to instrument the failed companies dummy with a dummy indicating whether or not a firm was involved in the corn trade. As briefly described in Section 3, corn prices collapsed suddenly in 1847 and we would consider this price collapse as a largely exogenous shock to corn traders' financial viability.

Nevertheless, it should be remembered that we do not rely on perfectly identified causal effects for our analysis discriminating between different explanations for central bank credit rationing. Since we are not interested in the exact marginal effect of any single covariate but the overarching patterns, we are confident that the lack of a separate identification strategy does not invalidate our findings. Hence, our hope, and indeed belief, is that our tests and results are clear enough to yield at least suggestive evidence for or against a specific hypothesis.

The split sample results are at the core of our discussion of central bank credit rationing. We find that several of our covariates are still generally strong predictors of rejections but the set of variables that predict (partial) rejections in crisis times are substantially different compared to episodes of calm. For normal times, we find that the important predictors are more or less similar (and bear the same sign) to the full sample results discussed in the previous paragraph. The picture changes (in some instances dramatically) when we consider the crisis part of our sample. Most importantly, discounter identity emerges as a key factor for understanding the dynamics at the Bank of England's discount window in 1847. In crisis times, good names are strongly negatively associated with rejections. In the specification without (with) week fixed effects, top discounters are 30% (51%) less likely to experience rejections (c.f. columns 5 and 7). In contrast, we find that being a top discounter did not yield any specific advantages at all during normal weeks (c.f. columns 6 and 8). In crisis weeks, discounters which failed later on in 1847 had a 24-27% higher probability of seeing their loan application (partially) rejected. The corresponding marginal effect for normal weeks is 13% lower and much less precisely estimated. Furthermore, the Bank appears to have rationed credit to bill brokers in crisis times (increase in the probability of rejections by 34-39%, depending on whether fixed effects are included). In normal times, the results without fixed effects suggest some positive discrimination in favor of bill brokers, but the statistical significance of this effect evaporates once week fixed effects are included. The results for our banker dummy are equally fascinating. In normal times, the Bank appears to have very much disliked transacting with banks through the discount window (45-47% higher probability of getting at least partial rejections), potentially reflecting the commonly cited conjecture that the BoE still considered bankers as competitors at the time. In crisis weeks, however, this discriminatory treatment completely disappears. It is tempting to interpret these results as evidence for early signs of Bagehotian LLR behavior: while discriminating against bankers in normal times, the Bank provided the necessary liquidity in times of need.

Although one might expect some spurious correlations between the simple rejection dummy and discounter types in the logit models, our Tobit regression results using the shares of rejected bills/amounts effectively confirm all of the split sample results above.[28] Hence, we will not discuss Table 11 and Table 12 separately here. Overall, then, the packet-level regression results suggest that the Bank's decisions to reject packets are at least partly predictable. In particular, discounter identity appears to matter. Our evidence at the packet-level thus seems to reject the hypothesis that pure credit rationing alone can account for the credit restrictions during the crisis of 1847. Predictability is not the only evidence against PR: more directly, we find that neither the intra-day rank of packets (chronological rank and value rank), nor the size of the packet (in terms of bills and value) have predictive power. In order to discriminate between RBR and the DR hypothesis, it is necessary to re-emphasize the sometimes large difference in marginal effects we find between crisis and non-crisis weeks. RBR would seem to be consistent only with the existence of similar rules for rejections during both crisis windows and normal times. The fact that rules appear in several cases to be drastically different suggests that credit restrictions during the crisis of 1847 were driven by the Bank's discriminatory supply side policies.

To conclude this subsection, we also draw on the logit regression coefficients obtained from the packet-level regressions on the sample of normal weeks only (c.f. Table 10, column 6) in order to make out-of-sample predictions of the probability of rejections during crisis weeks. We then compute the residuals of these out-of-sample predictions to test whether they are statistically distinguishable from the zero average residuals obtained from in-sample predictions. On average, the out-of-sample residuals are positive (mean of 0.08). We comfortably reject the null of equality of in- and out-of-sample residuals as well as the null that the out-of-sample predictions are on average smaller than zero. Since the t-statistic for these tests is -2.89, we obtain p-values well below 0.00. When we use the coefficients obtained from OLS regressions[29] explaining the share of bills in a packet that are rejected (both in terms of numbers and in terms of value), the results are even stronger (t-statistics of -3.40 and -3.12 respectively). The fact that we clearly underpredict rejections when we use out-of-sample coefficients for the crisis episodes constitutes evidence against the RBR hypothesis. These results again suggests that the DR hypothesis might be a good explanation for credit restrictions at the packet-level.

Overall, the econometric evidence at the packet-level suggests that the BoE discriminated against

[28]The coefficients estimated in the Tobit specifications are generally bigger than those found in the logit results, but due to different underlying models these two groups of coefficients cannot be directly compared. The Tobit coefficients reflect the coefficients on the latent variable ${S^*}_p$, and not those on S_p. Coefficients from Tobit models must be evaluated at chosen reference values (e.g. the variable mean). Average partial effects can then be computed for the so-called "unconditional expectation" of S_p, which is $E(S_p|\mathbf{X}_p)$. The conditional expectation would amount to $E(S_p|S_p > 0, S_p < 1, \mathbf{X}_p)$.

[29]For technical reasons, Tobit regressions results do not allow for this test. Therefore, we re-estimate our Tobit regressions using simple OLS.

Table 11: Tobit regressions (packet-level): share of rejected bills per packet (number)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total number of bills on day (ln)	0.12**	0.12**	0.12*	-0.01	0.07	0.08	0.09	-0.14
	(0.06)	(0.06)	(0.06)	(0.06)	(0.09)	(0.08)	(0.10)	(0.09)
Total value of bills on day (ln)	-0.01	-0.01	-0.01	-0.02	-0.09	0.11	-0.20**	0.25**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.08)	(0.10)	(0.10)	(0.10)
Packet's rank on day (chronological)	-0.01	-0.01	-0.01	-0.01	-0.02	-0.00	-0.03	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.05)	(0.05))
Packet's rank on day (value)	-0.00	-0.00	-0.00	-0.05	-0.23	0.26	-0.28	0.19
	(0.13)	(0.12)	(0.14)	(0.14)	(0.20)	(0.19)	(0.23)	(0.19)
Packet's total value (ln)	-0.06	-0.06	-0.06	-0.01	0.20	-0.33	0.24	-0.26
	(0.14)	(0.15)	(0.16)	(0.16)	(0.23)	(0.21)	(0.26)	(0.21)
Packet's total bills (ln)	0.01	0.01	0.01	0.04	0.01	0.02	0.04	0.04
	(0.05)	(0.06)	(0.05)	(0.05)	(0.08)	(0.07)	(0.08)	(0.07)
Dummy - discounter with DO account	-0.33**	-0.33**	-0.33**	-0.27*	-0.14	-0.69***	-0.13	-0.53**
	(0.15)	(0.17)	(0.15)	(0.14)	(0.19)	(0.25)	(0.19)	(0.23)
Dummy - discounter in rating book	0.13	0.13*	0.13	0.12	0.18	0.07	0.14	0.08
	(0.09)	(0.08)	(0.09)	(0.08)	(0.11)	(0.12)	(0.10)	(0.12)
Dummy - discounter in acceptor book	-0.26	-0.26	-0.26	-0.21	-0.13	-0.42**	-0.16	-0.44**
	(0.16)	(0.16)	(0.16)	(0.16)	(0.23)	(0.21)	(0.25)	(0.21)
Dummy - discounter is banker	0.38	0.38*	0.38*	0.40*	0.19	0.69***	0.11	0.89***
	(0.31)	(0.19)	(0.21)	(0.22)	(0.28)	(0.25)	(0.92)	(0.28)
Dummy - discounter is bill broker	0.21	0.21	0.21	0.36	0.63**	-0.72	0.80**	-0.40
	(0.24)	(0.18)	(0.23)	(0.22)	(0.29)	(0.47)	(0.41)	(0.38)
Dummy - discounter fails in 1847	0.42***	0.42***	0.42***	0.35***	0.45***	0.36*	0.41**	0.34*
	(0.15)	(0.13)	(0.12)	(0.12)	(0.16)	(0.18)	(0.20)	(0.18)
Dummy - discounter is top discounter	-0.37	-0.37	-0.37	-0.50	-0.89**	0.10	-1.09**	0.11
	(0.30)	(0.27)	(0.30)	(0.31)	(0.42)	(0.37)	(0.51)	(0.31)
Constant	-0.55***	-0.55***	-0.55***	-0.59**	-0.44***	-0.57***	-6.04***	-0.72**
	(0.07)	(0.07)	(0.07)	(0.26)	(0.09)	(0.11)	(0.64)	(0.29)
Observations	1,000	1,000	1,000	1,000	500	500	500	500
Sample	Total	Total	Total	Total	Crisis	Normal	Crisis	Normal
Week FE	No	No	No	Yes	No	No	Yes	Yes
Clustered SE	No	Week	Day	Day	Day	Day	Day	Day
Pseudo R-squared	0.02	0.02	0.02	0.12	0.02	0.04	0.10	0.17
Log pseudo-likelihood	-732.57	-732.57	-732.57	-656.02	-407.62	-310.12	-373.57	-267.76

Dependent variable: share of rejected bills in packet ([0,1]); (robust) standard errors in parentheses
Marginal effects for one stand. dev. increase in covariate (except for discrete variables, when change from 0 to 1)
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Tobit regressions (packet-level): share of rejected bills per packet (value)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total number of bills on day (ln)	0.14**	0.14**	0.14**	0.01	0.09	0.09	0.12	-0.15
	(0.06)	(0.06)	(0.07)	(0.06)	(0.09)	(0.08)	(0.10)	(0.10)
Total value of bills on day (ln)	-0.03	-0.03	-0.03	-0.04	-0.12	0.11	-0.23**	0.27**
	(0.06)	(0.07)	(0.07)	(0.07)	(0.08)	(0.10)	(0.10)	(0.11)
Packet's rank on day (chronological)	-0.01	-0.01	-0.01	-0.01	-0.01	-0.00	-0.02	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.05)	(0.06)
Packet's rank on day (value)	0.01	0.01	0.01	-0.04	-0.22	0.29	-0.28	0.21
	(0.13)	(0.13)	(0.15)	(0.15)	(0.20)	(0.21)	(0.25)	(0.20)
Packet's total value (ln)	-0.09	-0.09	-0.09	-0.04	0.17	-0.38*	0.23	-0.30
	(0.14)	(0.15)	(0.17)	(0.17)	(0.23)	(0.23)	(0.28)	(0.22)
Packet's total bills (ln)	0.02	0.02	0.02	0.05	0.02	0.03	0.05	0.05
	(0.05)	(0.06)	(0.06)	(0.06)	(0.08)	(0.07)	(0.09)	(0.07)
Dummy - discounter with DO account	-0.34**	-0.34*	-0.34**	-0.28*	-0.15	-0.73***	-0.13	-0.55**
	(0.16)	(0.19)	(0.15)	(0.14)	(0.19)	(0.27)	(0.19)	(0.25)
Dummy - discounter in rating book	0.13	0.13*	0.13	0.11	0.19	0.07	0.13	0.08
	(0.10)	(0.08)	(0.09)	(0.08)	(0.11)	(0.13)	(0.10)	(0.12)
Dummy - discounter in acceptor book	-0.21	-0.21	-0.21	-0.16	-0.03	-0.45**	-0.07	-0.46**
	(0.17)	(0.15)	(0.17)	(0.16)	(0.24)	(0.22)	(0.27)	(0.22)
Dummy - discounter is banker	0.42	0.42**	0.42*	0.45*	0.26	0.71***	0.18	0.93***
	(0.32)	(0.21)	(0.22)	(0.23)	(0.30)	(0.27)	(0.76)	(0.31)
Dummy - discounter is bill broker	0.22	0.22	0.22	0.36	0.66**	-0.77	0.81**	-0.41
	(0.25)	(0.19)	(0.24)	(0.23)	(0.30)	(0.50)	(0.36)	(0.43)
Dummy - discounter fails in 1847	0.45***	0.45***	0.45***	0.38***	0.49***	0.39**	0.45**	0.38*
	(0.15)	(0.14)	(0.13)	(0.13)	(0.16)	(0.19)	(0.18)	(0.19)
Dummy - discounter is top discounter	-0.38	-0.38	-0.38	-0.53	-0.93**	0.12	-1.14***	0.09
	(0.31)	(0.28)	(0.31)	(0.32)	(0.43)	(0.39)	(0.40)	(0.34)
Constant	-0.57***	-0.57***	-0.57***	-0.53*	-0.46***	-0.60***	-6.14***	-0.70**
	(0.07)	(0.07)	(0.07)	(0.30)	(0.09)	(0.12)	(0.49)	(0.33)
Observations	1,000	1,000	1,000	1,000	500	500	500	500
Sample	Total	Total	Total	Total	Crisis	Normal	Crisis	Normal
Week FE	No	No	No	Yes	No	No	Yes	Yes
Clustered SE	No	Week	Day	Day	Day	Day	Day	Day
Pseudo R-squared	0.02	0.02	0.02	0.12	0.02	0.04	0.10	0.17
Log pseudo-likelihood	-741.02	-741.02	-741.02	-656.52	-410.31	-315.97	-376.72	-274.05

Dependent variable: share of rejected bills in packet ([0,1]); (robust) standard errors in parentheses
Marginal effects for one stand. dev. increase in covariate (except for discrete variables, when change from 0 to 1)
*** p<0.01, ** p<0.05, * p<0.1

certain types of loan applicants in crisis times. In particular, bill brokers, companies that turned out to fail in 1847 and applicants that were not among the top notch names in the market experienced significantly higher probabilities of having (parts of) their submitted packets rejected. At the same time, forms of discrimination present in normal weeks disappeared in times of distress: the Bank apparently set different priorities in different circumstances. Thus, one plausible explanation for the episodes of credit rationing observed by Bignon et al. (2012) is that the impact of the BoE's new discriminatory practices in crisis weeks outweighed the pausing of special treatments (positive for customers with DO accounts or with entries in the acceptor rating book, and negative in the case of bankers) it usually practiced outside crisis windows. Finally, and subject to the caveats mentioned above, the results presented in this subsection show that Capie's "frosted glass" hypothesis is difficult to defend in the case of the BoE's 19$^{\text{th}}$ discount window policies: we find that the identity/characteristics of discounters can predict the (partial) rejection of packets, at least at the margin.

6.3 Bill-level regressions: credit rationing and the role of bill characteristics

We now turn to our second, bill-level sample to estimate conditional logistic regressions. The purpose of the bill-level regressions is threefold at least. First, as mentioned above, loan decisions by the BoE were not taken at the packet-level but at the level of the individual bills. Hence, the bill-level regressions serve as a useful robustness check for the main conclusions we have drawn on the basis of our packet-level data. Second, since both Capie (2002) and Flandreau and Ugolini (2011, 2013, 2014) suggest that bill-level ("collateral") characteristics were important for the BoE's loan decisions, an analysis of the predictive power of these covariates allows us to plausibilize this conjecture. Third, the conditional logistic regressions allow for a proper *ceteris paribus* analysis we are not yet able to implement for the packet-level data. The matched case-control method enables us to estimate the "purged" marginal effects of bill-level characteristics on the probability of bill rejections by fixing discounter identity and date.

Summary statistics for the bill-level data are displayed in Table 13 and the regression results for the conditional logistic estimator can be found in Table 14. Table 14 is organized in similar way to our packet-level tables above: the first three columns use the full sample specification to estimate the coefficients on the bill-level characteristics, while columns 4 and 5 represent the split sample regression results. In contrast to the packet-level results, we do not need to include week fixed effects because we already use day fixed effects due to our research design. Furthermore, we cluster standard errors by default at the packet-level to control for intra-packet correlation. As before, marginal effects on

Table 13: Summary statistics for bill-level regressions

Variable	Obs	Mean	Std. Dev.	Min	Max	P1	P5	P25	P50	P75	P95	P99
Rejection dummy	1060	0.35	0.48	0.00	1.00	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Days to maturity (ln)	1057	3.94	0.65	0.69	5.96	1.79	2.56	3.76	4.13	4.41	4.52	4.55
Spread from mean maturity of 60 days	1060	0.32	27.55	-328.00	90.00	-35.00	-32.00	-22.00	-2.00	17.50	48.00	55.00
Dummy - maturity>95days	1060	0.01	0.09	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Amount on bill (ln)	1060	5.46	1.00	2.3	8.01	3.14	3.69	4.79	5.46	6.21	7.09	7.68
Dummy - promissory note	1060	0.03	0.17	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00
Dummy - drawer or acceptor failed	1060	0.01	0.08	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Dummy - drawer or acceptor bill broker	1060	0.00	0.06	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Dummy - drawer or acceptor DO	1060	0.01	0.09	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Dummy - acceptor in London	1060	0.84	0.37	0.00	1.00	0.00	0.00	1.00	1.00	1.00	1.00	1.00
Dummy - acceptor in London (strict)	1060	0.83	-0.38	0.00	1.00	0.00	0.00	1.00	1.00	1.00	1.00	1.00
Dummy - discounter=acceptor	1060	0.48	0.50	0.00	1.00	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Dummy - discounter=acceptor (strict)	1060	0.45	0.50	0.00	1.00	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Dummy - drawer=acceptor	1060	0.02	0.14	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00
Dummy - acceptor=directors	1060	0.02	0.12	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00
Dummy - acceptor=bank	1060	0.09	0.28	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - acceptor=top acceptor	1060	0.05	0.22	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.50	1.00
Dummy - acceptor=top discounter	1060	0.01	0.10	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Dummy - acceptor in rating book	1060	0.11	0.31	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - acceptor in acceptor book	1060	0.05	0.22	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - drawer=bank	1060	0.06	0.24	0.00	1.00	0.00	0.00	0.00	0.00	0.00	1.00	1.00
Dummy - drawer in rating book	1060	0.26	0.44	0.00	1.00	0.00	0.00	0.00	0.00	1.00	1.00	1.00
Dummy - drawer in acceptor book	1060	0.03	0.17	0.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00

continuous variables are standardized and for discrete variables the effects are computed for a change from zero to one. Instead of representing three different levels of clustering, columns 1-3 in Table 14 differ in the variable for bill maturity. As we explained in Section 5 above, we can draw on three alternative proxies for this purpose: the days to maturity, the spread from the average maturity of accepted bills, and a dummy for bills which exceed the threshold maturity of 95 days. Since we find that the first two of these variables have no predictive power for rejections, we focus on the dummy variable – which is highly significant, both from a statistical and an economic perspective – when doing split sample regressions.[30]

Two further remarks apply. First, as explained in Section 4 above, we draw 200 packets for our second sample and impose a series on restrictions on dates and number of bills/rejections. The 200 packets in our sample contain 1,060 individual bills[31] which we have unpacked for the regressions in Table 14. The 100 crisis packets include a total of 546 bills, and the 100 packets sampled from normal weeks contain 514 bills. Second, as can be seen from the pseudo R-squared statistic, our bill-level models explain a much larger fraction of the variation in our dependent variable (between 67% and 72% depending on the specification).

Generally, we find that bill characteristics are highly statistically significant and partly also economically very significant – despite the fact that we control for discounter and date fixed effects. Whatever the impact of applicant identity, bill characteristics certainly played an important role in

[30] In a second paper parallel to the present one, we find that maturities are crucial for determining the rate at which a packet was discounted. Hence, while maturity *per se* did not matter for rejections, the Bank used price discrimination tools to signal it disliked long maturities.

[31] We lose seven of these 1,060 observations when using the natural logarithm of the days to maturity as our proxy variable for maturity. This happens because some of the bills were apparently due before they were submitted, resulting in negative days to maturity.

Table 14: Conditional logistic regressions (bill-level): rejected bills vs accepted bills

VARIABLES	(1)	(2)	(3)	(4)	(5)
Days to maturity (ln)	-0.00				
	(0.00)				
Spread from mean maturity of 60 days		0.01			
		(0.00)			
Dummy - maturity>95days			0.37***	0.26***	0.40***
			(0.06)	(0.05)	(0.12)
Amount on bill (ln)	0.01***	0.02***	0.01***	0.01***	0.02**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - promissory note	0.03***	0.03***	0.03***	0.05***	0.03***
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Dummy - drawer or acceptor failed	0.04***	0.04***	0.05***	0.05***	0.06***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - drawer or acceptor bill broker	0.02	0.01	0.02	-0.10***	0.06***
	(0.02)	(0.02)	(0.01)	(0.00)	(0.01)
Dummy - drawer or acceptor DO	-0.12***	-0.12***	-0.12***	-0.10***	-0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - acceptor in London	-0.60***	-0.60***	-0.60***	-0.58***	-0.61***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Dummy - discounter=acceptor (strict)	0.07***	0.07***	0.07***	0.05***	0.10***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Dummy - drawer=acceptor	-0.03	-0.03	-0.03	0.01*	-0.04
	(0.03)	(0.03)	(0.03)	(0.01)	(0.05)
Dummy - acceptor=directors	-0.12***	-0.12***	-0.12***	-0.10***	-0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - acceptor=bank	-0.12***	-0.12***	-0.12***	-0.10***	-0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - acceptor=top acceptor	-0.12***	-0.12***	-0.12***	-0.10***	-0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - acceptor=top discounter	-0.12***	-0.12***	-0.12***	-0.03	-0.13***
	(0.00)	(0.00)	(0.00)	(0.03)	(0.01)
Dummy - acceptor in rating book	-0.05**	-0.05**	-0.05**	-0.06*	-0.04
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Dummy - acceptor in acceptor book	-0.03	-0.03	-0.04	-0.00	-0.13***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.01)
Dummy - drawer=bank	-0.12***	-0.12***	-0.12***	-0.10***	-0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Dummy - drawer in rating book	-0.01	-0.02	-0.01	0.01	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Dummy - drawer in acceptor book	0.02	0.02	0.02	0.01	0.05***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)
Bills	1,053	1,060	1,060	546	514
Packets	200	200	200	100	100
Sample	Total	Total	Total	Crisis	Normal
Discounter-date FE	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.67	0.67	0.67	0.63	0.72

Dependent variable: probability of rejection; (robust) standard errors in parentheses
Marginal effects for one stand. dev. increase in covariate
(except for discrete variables, when change from 0 to 1)
*** p<0.01, ** p<0.05, * p<0.1

the BoE's decisions whether to accept or reject individual bills. Thus, we are fairly confident that our results confirm the conjectures about the importance of "collateral quality" in Capie (2002) and Flandreau and Ugolini (2011, 2013, 2014). More precisely, we find that both the characteristics of the bills themselves (maturity, amount) and the names on the bills mattered. As before, positive marginal effects mean that the probability of rejection is increased and vice versa. For example, holding discounter and date fixed, bills with remaining maturities higher than 95 days were between 26% and 40% more likely to be rejected. To be sure, this finding is interesting in its own right: we find that the 95 days maturity threshold was in fact not a fixed rule because it does not perfectly predict rejections once discounter and date are controlled for. The largest marginal effect is associated with the London dummy (60% less likely to be rejected). Looking at the summary statistics, this finding is not surprising: literally all the bills that were discounted by the Bank had an acceptor located in London. However, London acceptors do not perfectly predict the Bank's decision to discount a bill either: the BoE also rejected some bills accepted in London, suggesting that the right location of the acceptor was not a perfect guarantee for a successful loan application. We speculate that the reason for the importance of the London dummy is related to the existence of BoE branches and their role in the Bank's credit policies. As we discussed in Section 3 above, the ledgers in the BoE archives only contain data for the loan decisions of the London headquarter. Hence, it seems logical that the London arm of the Bank would reject bills that were accepted elsewhere in England: bills accepted outside London should have probably been submitted to local BoE branches, where the local staff would have a better understanding of whether the name was good or of insufficient quality and the transaction costs of collecting payment at maturity were lower.

Rather than describing all the remaining marginal effects in Table 14 in detail, we would like to focus on the general conclusions of direct relevance for the credit rationing hypotheses we are testing. One obvious conclusion from Table 14 is that the rejection of bills did most certainly not occur in a purely randomized fashion. The fact that many of our covariates are highly significant predictors suggests that pure credit rationing is not likely to be the only or most convincing explanation central bank credit restrictions in 1847. At least some rules existed which were followed consistently by Discount Office clerks when they examined submitted bills. Moreover, the results from our split sample regressions show that most of the marginal effects have the same sign and very similar sizes in both crisis weeks and normal times. Although this finding would be evidence in favor of RBR, we cannot definitely reject the DR hypothesis on the basis of Table 14: a small number of marginal effects are substantially different in crisis windows when compared to normal weeks (e.g. bill broker signatures as drawers or acceptors, top discounter effects and entries in the acceptor book). Interestingly, all these differences suggest that names on the bills (not to be confused with the discounter name) were less important during crisis weeks. In the case of bill brokers, there even seems to be a positive discrimination effect (as opposed to a negative one in normal times).

To shed more light on the DR vs RBR "horse race", we implement the same out-of-sample test we discussed for our packet-level results. We use the coefficients obtained from the split sample regression for normal weeks only to compute the in sample and out-of-sample residuals for normal and crisis weeks respectively. When we test for differences in the residuals using the bill-level data, we cannot reject the null that residuals are on average equal (t-statistic of -0.83). Thus, while the bill-level regressions allow for a fairly clear rejection of the PR hypothesis, the results seem to favor RBR over DR. This finding stands in stark contrast to our packet-level regressions. One interpretation of these result might be that the Bank discriminated on the basis of discounter identity but followed consistent rules at the bill-level, once the discounter identity was determined. However, more work is needed to implement the reverse test in a proper manner: as mentioned in Section 4, we are currently working on a representative data set that will allow us to see whether discounter identity still matters once we control for the quality of the bills in the packet submitted.

7 Conclusion

It is well-known that quantitative credit restrictions, rather than Bagehot-style "free lending" constituted the standard response to financial crises in the early days of central banking. But why did central banks in the past frequently restrict the supply of loans during financial crises? Answering this question is important for two reasons, at the very least. First, an investigation into the drivers of credit rationing might help accounting for the underlying reasons of painful financial crises and their severe real effects in the past. Second, a better understanding of the reasons for central bank credit restrictions could inform policy-making targeted at reducing the frictions hampering an elastic supply of central bank liquidity during financial crises.

In this paper, we exploited a large novel, hand-collected loan-level data set to study the Bank of England's policy response to the crisis of 1847. We chose the crisis of 1847 because it represents an archetypical case of credit rationing which is often discussed in the contemporary sources of the time and the secondary literature. We tested whether credit restrictions during this crisis were likely due to residual imperfect information (Stiglitz and Weiss, 1981) or rather driven by active discriminatory practices on the supply side and/or an increase in rules-based rejections due to lower quality submissions (demand-side driven rejections) in times of acute financial distress.

We find that pure credit rationing à la Stiglitz-Weiss alone cannot be a convincing explanation for quantitative credit restrictions during the crisis of 1847 and provide preliminary evidence which could suggest that discriminatory credit rationing on the basis of loan applicants' type and identity characterized the BoE's response to the crisis of 1847. Our results also show that "collateral" characteristics played an important role in the BoE's loan decisions, even after one controls for the identity of loan applicants. This finding confirms the hypothesis in Capie (2002) and Flandreau and Ugolini (2011, 2013, 2014) that the characteristics of bills of exchange submitted to the discount window mattered. Since our results suggest that the Bank also took decisions on the basis of the identity of loan applicants, our preliminary findings would seem to challenge Capie's "frosted glass" metaphor, but more work is required to confirm these conjectures.

As a next step we are currently working on additional data samples in order to shed more light on the relative importance of loan applicant identity and collateral characteristics. Our goal is to design a "horse race" analysis which allows both sides to be included in a regression framework estimated with representative loan-level data. These new data should enable us to check our preliminary results presented in this paper and make a more definitive statement with regard to the "frosted glass" versus "raised eyebrow" debate in the financial history literature focusing on central bank operations.

8 Appendix

Figure 6: Credit rationing during the crisis of 1847: market rate for prime bills, weighted Bank rate and the market-Bank spread

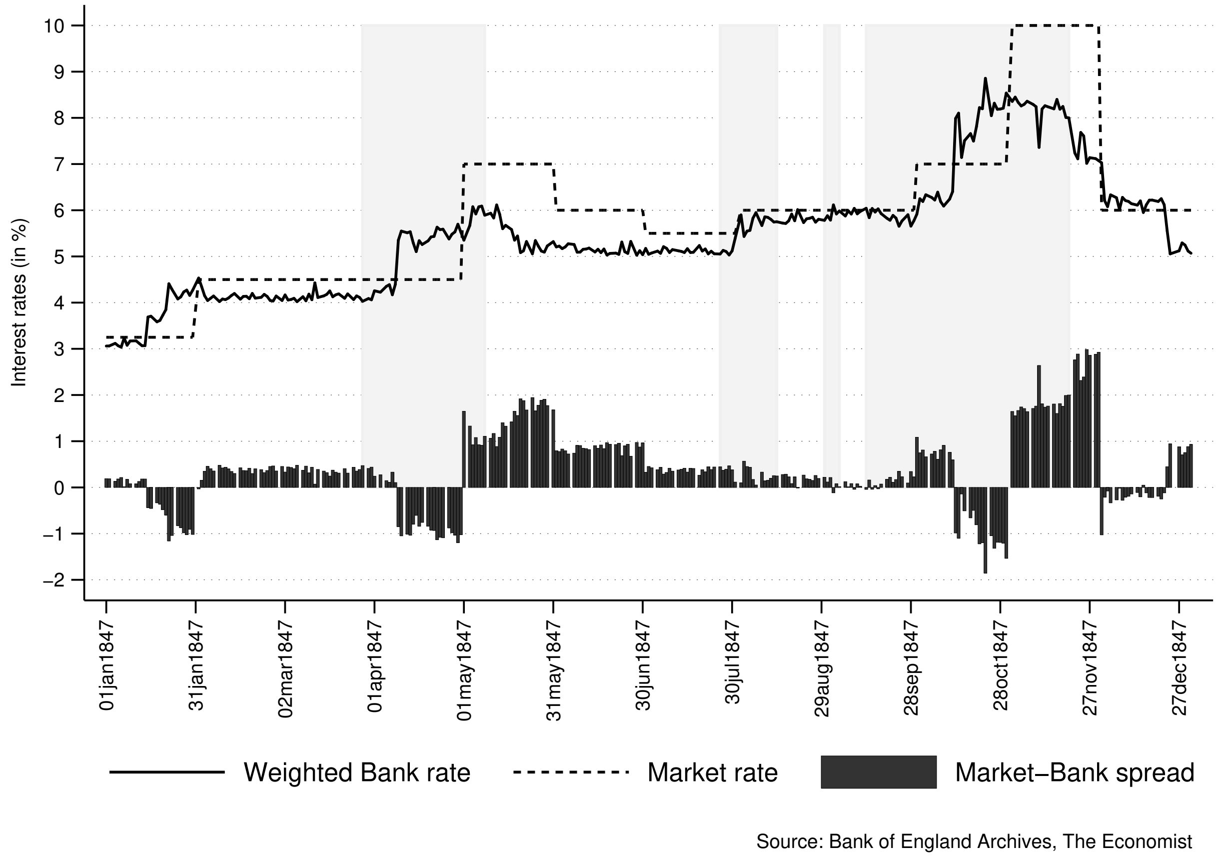
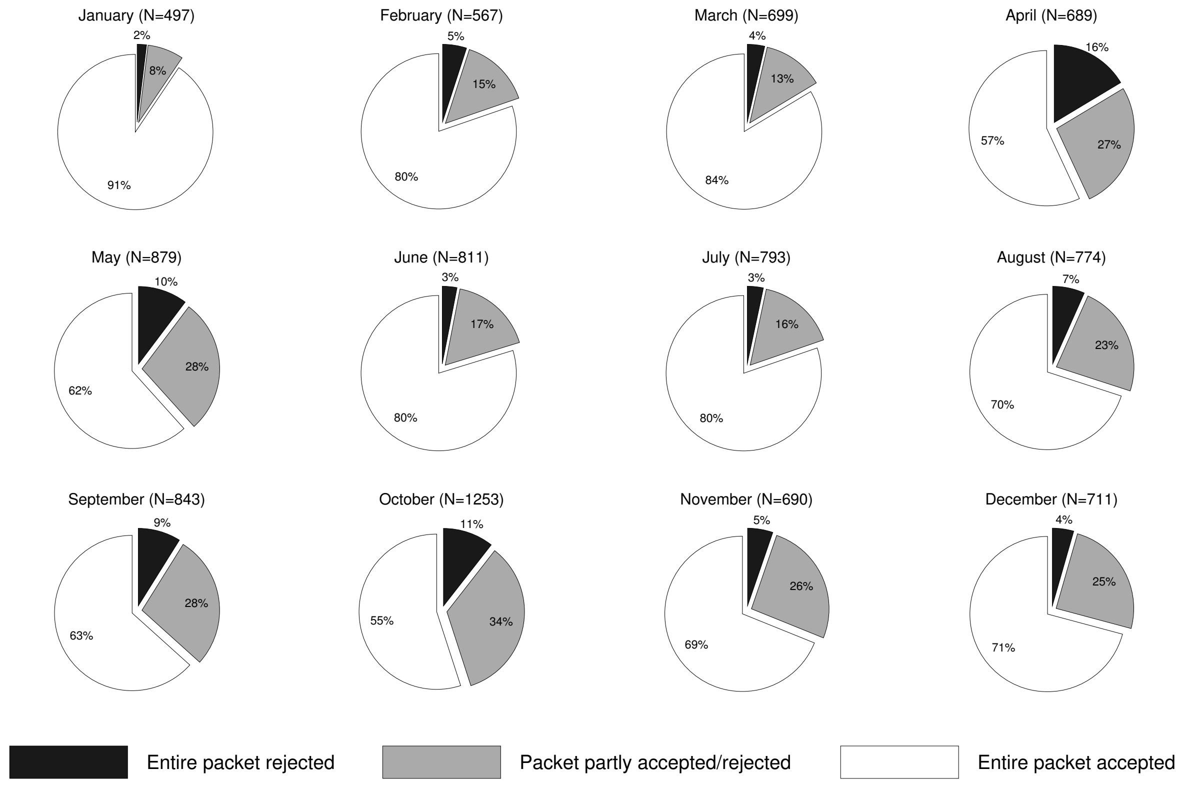


Figure 7: Packets submitted to the Bank of England's discount window in 1847 (N=9,206; by month)

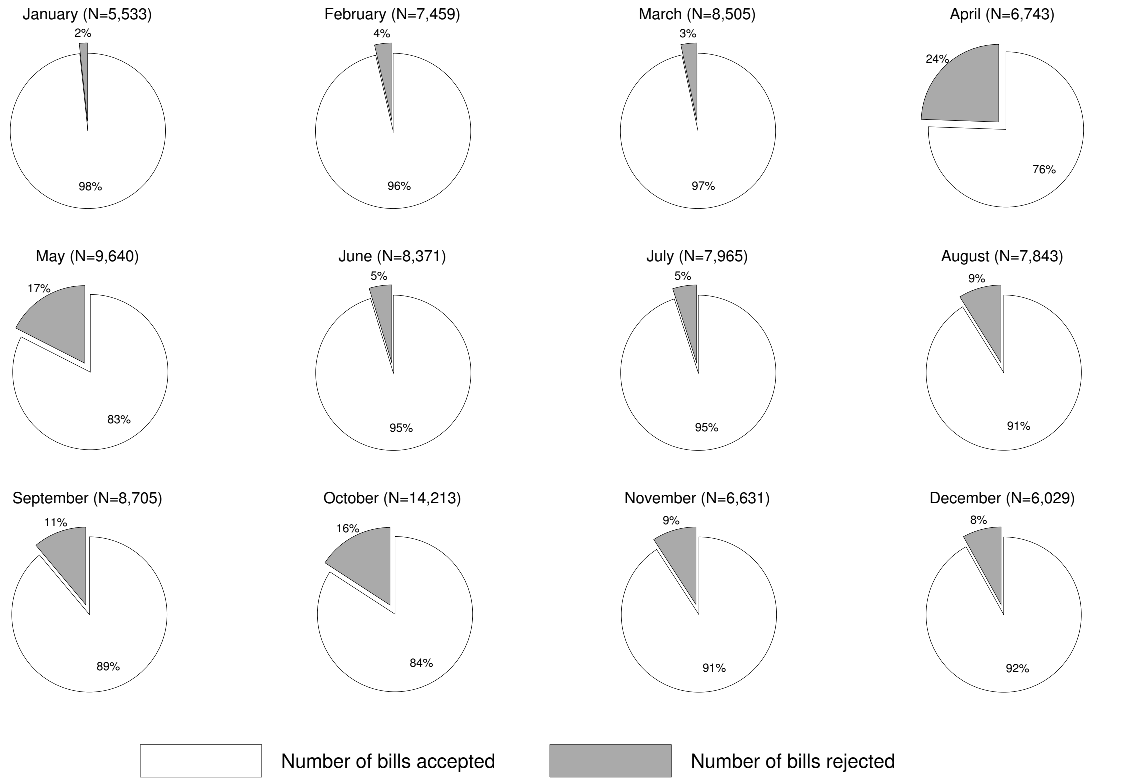


Source: BoE daily ledger 1847

Table 15: T-tests - rejected vs accepted (total sample)

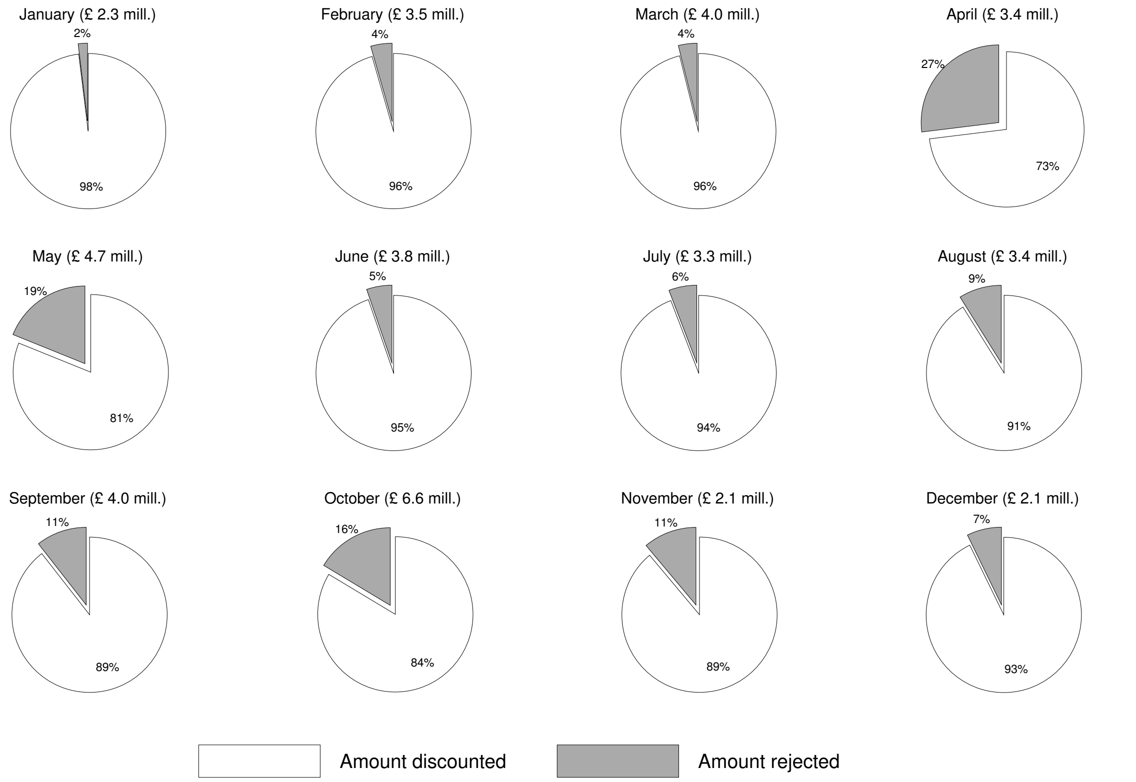
Variable	Packets with rejections (Obs)	Packets with rejections (Mean)	All accepted (Obs)	All accepted (Mean)	Two-sided p-value
Total number of bills on day (ln)	299	3.52	701	3.45	0.00***
Total value of bills on day (ln)	299	11.90	701	11.82	0.08*
Packet's rank on day (chronological)	299	0.50	701	0.51	0.55
Packet's rank on day (value)	299	0.54	701	0.52	0.48
Packet's total value	299	7.80	701	7.75	0.53
Packet's total bills	299	1.89	701	1.68	0.01***
Dummy - discounter with DO account	299	0.06	701	0.11	0.02**
Dummy - discounter in rating book	299	0.32	701	0.29	0.42
Dummy - discounter in acceptor book	299	0.06	701	0.09	0.12
Dummy - discounter is banker	299	0.02	701	0.01	0.03**
Dummy - discounter is bill broker	299	0.04	701	0.03	0.57
Dummy - discounter fails in 1847	299	0.10	701	0.05	0.00***
Dummy - discounter is top discounter	299	0.02	701	0.03	0.32

Figure 8: Bills submitted to the Bank of England's discount window in 1847 (N=97,637; by month)



Source: BoE daily ledger 1847

Figure 9: Monetary value submitted to the Bank of England's discount window in 1847 (total of £ 43.1 mill.; by month)



Source: BoE daily ledger 1847

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