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DO BANKS TAKE UNUSUAL RISKS WHEN INTEREST RATES ARE EXPECTED TO STAY LOW FOR A LONG TIME?

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The financial woes that initiated the financial crisis of 2007/08 have, at least in part, been traced to excessive bank risk-taking. What induced this behavior? One explanation is the persistently low short-term interest rates during the mid-2000s. We exploit an extensive panel of matched Austrian banks and firms during 2000–2008 to investigate the effects of the European Central Bank's (ECB) policy of persistently low interest rates during 2003q3–2005q3. Our analysis suggests that this policy likely caused Austrian banks to hold risker loan portfolios than they would have in its absence.

Keywords: Monetary Policy, Risk-Taking, Financial Stability

1. INTRODUCTION

In the wake of the financial crisis of 2007/08, there has been growing interest in the relationship between the stance of monetary policy and the amount of risk taken in financial markets. In particular, researchers and policy makers have been discussing the possibility that a lower cost of external funds may increase banks' risk appetite and thereby cause an increase in the ex-ante risk taken in the market [e.g., Diamond and Rajan (2012)].[1] Within a growing body of empirical research on this so-called "risk-taking channel" of monetary policy, Jiménez et al.

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(2014) provide perhaps the most convincing evidence from Spain, finding that "a lower overnight interest rate induces lowly capitalized banks to grant more loan applications to ex ante risky firms and to commit larger loan volumes with fewer collateral requirements to these firms, yet with a higher ex-post likelihood of default."[2]

Our paper contributes to this literature along two dimensions: first, we exploit an extensive, matched firm-bank panel from Austria over the period 2000–2008, allowing us to study a lending market that is dominated by predominantly locally operating banks, that is exposed to largely exogenous monetary policy, and that experienced neither a major housing bubble nor massive influx of foreign capital.[3] We therefore argue that this market provides an ideal setup to disentangle the effects of monetary policy from other major factors that affected banks' cost of external funds in many countries around the globe (especially for real estate lending), far and foremost the United States.[4]

Second, we particularly focus on the effects of the European Central Bank's (ECB) stance of policy throughout 2003q3–2005q3 on the realized ex-ante risk in banks' loan portfolios, as opposed to the average effects of short-term changes in the policy rate. This period was special in at least two respects: the ECB kept its main refinancing rate constant at a then unprecedented low of 2% and there was widespread perception that policy interest rates would remain low for an extended period of time.[5]

To accomplish this, we first estimate the average impact of changes in the cost of short-term funds on ex-ante default risk within Austrian banks' business loan portfolios, confirming a now well-documented result [e.g. Jiménez et al. (2014)]:[2] a lower cost of short-term funds tends to induce more ex-ante risk in loan portfolios throughout the period 2000–2008, on average. In a second step, we then show that this average effect is almost entirely driven by the period 2003q3–2005q3. In fact, during the periods immediately before and immediately after this episode, Austrian banks (a) showed approximately the same response to changes in the cost of short-term funds and (b) a reduction in the cost of funds during these periods either had no statistically significant impact or, if anything, a slightly negative impact on the amount of ex-ante risk held in loan portfolios. Taken together, these results suggest that Austrian banks were likely taking differentially more ex-ante risk during the period of persistently low ECB policy rates.

Diamond and Rajan (2012) as well as Farhi and Tirole (2012) have recently argued that perfectly rational bank behavior may lead to such developments. Both studies argue that anticipated expansive monetary policy may serve as insurance against expected future liquidity risk and thus spur excessive investment into risky long-term assets. Moreover, Diamond and Rajan (2012) stress that the optimal policy to avoid inefficient risk buildup is "[raising] rates in normal times [beyond the level predicted by standard theory] to offset distortions from reducing rates in adverse times." While we cannot test whether Austrian banks' outcomes were efficient or not, we note that our results are in principle consistent with these theories, as they suggest that Austrian banks were likely taking differentially more ex-ante loan portfolio risk during the period of low and stable ECB refinancing rates (2003q3–2005q3), compared to the remaining periods throughout 2000–2008.

Despite its policy relevance and intuitive theoretical foundations, there are several challenges to a clean identification of such a mechanism. Our empirical strategy rests on three main identifying assumptions: first, the ECB conducts policy with the goal of stabilizing the euro area as a whole, and does not exclusively focus on the performance of individual member states. Second, we argue that the majority of Austrian banks predominantly focus on the local business cycle. Therefore, whenever the Austrian business cycle is sufficiently "out of sync" with the overall euro area cycle, the ECB's policy decisions are likely exogenous to Austrian banks. Under these assumptions, the difference between a Taylor (1993) rule for Austria and the euro area should then capture largely exogenous variation in the effective cost of short-term funds for Austrian banks, since a Taylor rule can be interpreted as both a measure of economic conditions and a prediction for the short-term policy rate. Third, we estimate the differential impact on lowly capitalized banks, as these banks face the largest moral hazard problems, and should therefore have the strongest incentive to take larger risks in response to changes in the cost of short-term funds [Holmstrom and Tirole (1997), Jiménez et al. (2014)].[6]

We show that our qualitative results neither depend on a specific measure of the cycle nor do we have to assume any particular ECB policy rule, beyond the assertion that the ECB seeks to generally support the macroeconomy in the overall euro area. However, for comparability with previous studies and because of its convenient dual interpretation as predicted policy rates, we use a Taylor rule—a weighted average of output and inflation gaps—as our main measure of the cycle. We also provide a variety of robustness checks and arguments for why our main results are likely not driven by any other policy or particular event during 2003q3–2005q3, beyond the stance of ECB policy.

Our work is most closely related to Altunbas et al. (2014), Dell'Ariccia et al. (2017), as well as Delis and Kouretas (2011), who focus on the effect of a bank's cost of external funds on the risk composition of its loan portfolio. These studies utilize an array of measures for banks' portfolio-risk—like expected default frequencies or risk-weighted assets—and postulate that changes in these measures capture changes in banks' risk-taking behavior. They then test whether changes in short-term interest rates have an effect on these measures of risk.

What differentiates our study is a unique data set from the Austrian credit register and our focus on the effect of policy interest rates that are (and are expected to remain) low and constant for an extended period, as opposed to the average effect of short-term adjustments to the policy rate. Maddaloni and Peydró's (2011) work is most closely related in the latter respect. They find that bank lending standards are significantly decreasing in the number of quarters that short-term interest rates stay below the prediction of a Taylor rule. Instead, we interpret the period 2003q3–2005q3 as a "unique episode" of persistently low policy rates and ask whether bank-risk-taking in response to changes in the short-term cost of funds during this period is substantially different from other periods. Again, this episode was "special" not only because the ECB kept its main refinancing rate at a then unprecedented low of 2%, but also because there was widespread perception that this policy rate would remain low and stable for an extended period.[5]

Finally, despite the many benefits of our data set, we note that our analysis does not allow us to *directly* disentangle supply of credit from demand for credit, as we do not have access to loan applications. However, there are several reasons for why we believe that our results are nevertheless insightful: first, our estimates for the average impact of changes in the cost of short-term funds are fully consistent with other studies that do have access to loan applications [e.g., Jiménez et al. (2014)]. Second, we find that our results are almost exclusively driven by lowly capitalized banks. Thus, unless we believe that there were systematic changes in the composition of Austrian credit demand precisely during 2003q3–2005q3, which were also systematically biased toward lowly capitalized banks, our estimates likely capture responses in credit supply, rather than credit demand. Finally, although the goal of this paper is to study loan portfolio risk at the bank level, we also confirm that an analogous analysis of risk at the firm-bank level, which allows us to control for unobserved firm heterogeneity, delivers qualitatively equivalent results.

The remainder of this paper is organized as follows: Section 2 describes the details of our empirical strategy and the data set, while Section 3 presents the main empirical results. We offer some concluding remarks in Section 4.[7]

2. EMPIRICAL STRATEGY

Our empirical strategy is a variation of that employed by Maddaloni and Peydró (2011). They ask how changes in the short-term policy rate affect banks' lending standards. Moreover, they also ask whether persistence in low short-term interest rates matters for lending standards. To do so, they exploit arguably exogenous variation in Taylor rule residuals (relative to the observed policy rate) across European countries and the United States, to estimate the impact of changes in short-term interest rates. The main difference in our approach is that we ask a slightly different question: we treat the ECB's stance of policy during 2003q3–2005q3 as a unique event and ask whether the average effect identified by Maddaloni and Peydró (2011) is predominantly driven by this particular episode.

Another difference in our strategy is that we use the difference in Taylor rules between Austria and the euro area to proxy plausibly exogenous variation in the short-term cost of funds for Austrian banks, rather than the residual of an Austrian Taylor rule relative to the ECB policy rate. Although the difference in the two measures is not substantial, we prefer ours for two reasons: first, it captures the idea that ECB policy actions are exogenous, whenever the euro area cycle is sufficiently out of sync with that in Austria. Second, we do not need to postulate that the ECB's policy decisions actually adhere to a Taylor rule. We simply assert that the ECB aims to stabilize the euro area economy. Although a Taylor

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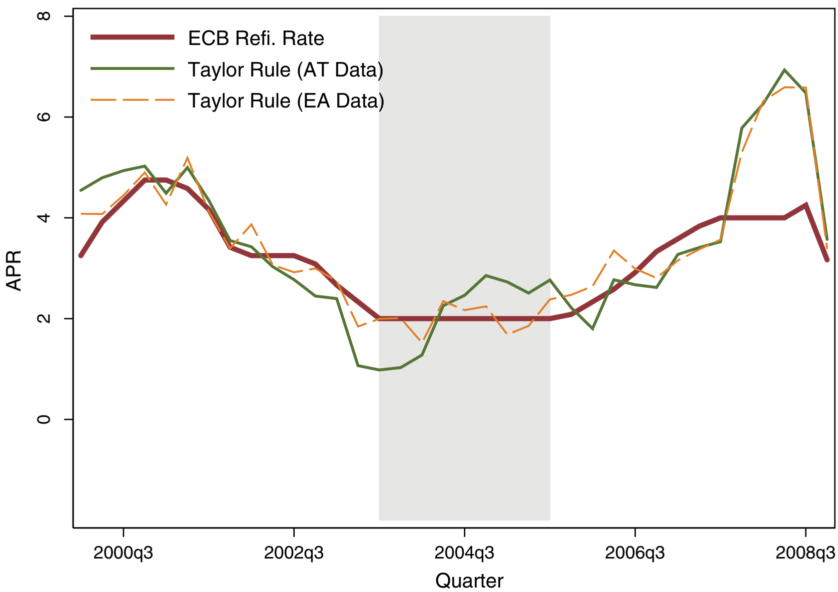

FIGURE 1. Taylor rules for Austria and the euro area. The figure displays the a Taylor rule for Austria (AT) and the euro area (EA), as utilized in equation (2). The thick solid line represents the ECB's main refinancing rate. All three measures are expressed in annual percentage rates (APR), where the Taylor rules are interpreted as the policy rate predicted by output gaps and inflation. The lightly shaded rectangular area illustrates the period during which the ECB refinancing rate was constant at 2%.

rule—a weighted average of output and inflation gaps—is clearly a measure of overall business conditions, the convenient dual interpretation as predicted interest rates will make it much easier to comment on the magnitude of the estimated effects.[8]

To illustrate the variation used in our empirical analysis, Figure 1 shows Taylor rules for Austria and the euro area, as well as the ECB's main refinancing rate, highlighting that the Taylor rule for Austria deviates substantially from both the euro area Taylor rule and the ECB's main refinancing rate.[9] In stark contrast, the Taylor rule for the euro area is almost always very close to the ECB's refinancing rate.

The main source of independent variation used in our analysis is the difference between these two alternative Taylor rules, depicted in Figure 2. We note that whenever this gap is very small, the stance of policy is as if the ECB was conducting policy to specifically stabilize Austria. Given that Austria is one of the "core" euro area countries, ECB policy decisions may thus be endogenous to Austria in such scenarios. Hence, an additional refinement to the empirical strategy used by Maddaloni and Peydró (2011) is to focus on episodes during which this gap is "sufficiently" large. Interpreting the Taylor rules as predicted nominal policy

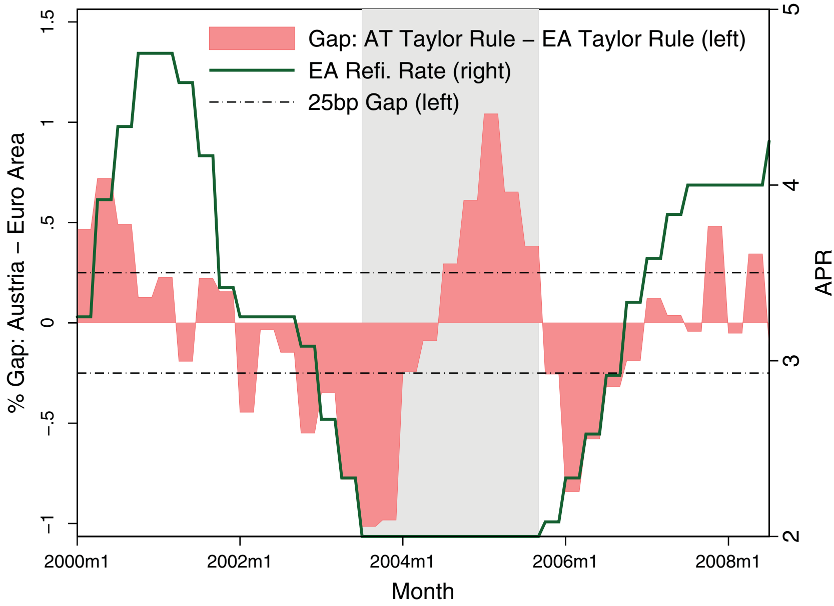

FIGURE 2. Economic conditions: Austria vs. euro area. The dark shaded areas display the gap between economic conditions in Austria (AT) and the euro area (EA), as specified in equation (2). The dashed lines indicate when this gap is 25 basis points in absolute value. The solid line is the ECB's main refinancing rate, expressed as an annual percentage rate (APR). The lightly shaded rectangular area illustrates the period during which the ECB refinancing rate was constant at 2%.

rates, a gap of 25 basis points in absolute value is a natural threshold, since the ECB typically changes its policy rate in increments of 25 basis points. The dashed horizontal lines in Figure 2 indicate this threshold.

Under the assumptions made above, the Taylor rule gap serves as a proxy for plausibly exogenous variation in the cost of funds for banks that predominantly focus on business conditions in Austria. Thus, one important aspect of the market we study is that, indeed, the vast majority of Austrian banks is small and operates predominantly locally. Specifically, when ranked by the amount of outstanding business loans, the top 4 out of the 316 banks in our sample (99th percentile and above) provide 17% of all business lending, and the top 16 (95th percentile and above) provide 47% of all business loans. The remaining 95% of Austrian banks are small and operate predominantly locally.

Finally, Figure 2 also illustrates that there is ample variation in the Taylor rule gap throughout our entire sample period (2000–2008). This will allow us to estimate the differential effect of changes in the cost of short-term funds during 2003q3–2005q3, in order to gauge the impact of the ECB's stance of policy during this period.

2.1. Regression Framework

We implement our empirical analysis in three steps: in analogy to the analysis by Maddaloni and Peydró (2011), we start with estimating the average effect of changes in the cost of short-term funds throughout the entire sample; in a second step, we refine the regression model to estimate the differential effect during 2003q3–2005q3 (henceforth the "treatment" period); and finally, in a third step, we follow Jiménez et al. (2014) and redo our baseline analyses separately for banks with different levels of capitalization, as we expect the effect to be largest for the least capitalized banks, facing more severe moral hazard problems in periods of low and stable short-term interest rates [Holmstrom and Tirole (1997)].

Our main analyses are at the bank level, at monthly frequency, and we use the ex-ante expected default rate in each bank's business-loan portfolio as the relevant outcome variable in all models. We denote this variable as $\mathrm{EDR}_{b,t}$ for bank b at time t, defined as the fraction of lending that the bank expects to default, ex-ante:

$$\mathrm{EDR}_{b,t} = \frac{\sum_{f \in \mathcal{F}_{b,t}} p^h_{f,t} L_{f,b,t}}{\sum_{f \in \mathcal{F}_{b,t}} L_{f,b,t}}, \qquad (1)$$

where $p^h_{f,t}$ is firm f's ex-ante expected probability of default within the next h years, based on information known at time t, $L_{f,b,t}$ is the amount of lending from bank b to firm f at time t, and $\mathcal{F}_{b,t}$ is the set of all firms that bank b is lending to at time t. This measure captures the degree of default risk within each bank's loan portfolio. Appendix A spells out the details of how we estimate $p^h_{f,t}$ at the firm level.

The average effect of short-term interest rates. As described in the preceding section, our main source of "independent" variation is the gap between a Taylor rule for Austria and the euro area, defined as

$$\begin{aligned} \mathrm{gap}_t = {} & \left[\bar{r}^{\mathrm{AT}}_t + \bar{\pi}^{\mathrm{AT}}_t + (1+\phi_\pi)(\pi^{\mathrm{AT}}_t - \bar{\pi}^{\mathrm{AT}}_t) + \phi_y(y^{\mathrm{AT}}_t - \bar{y}^{\mathrm{AT}}_t)\right] \\ & - \left[\bar{r}^{\mathrm{EA}}_t + \bar{\pi}^{\mathrm{EA}}_t + (1+\phi_\pi)(\pi^{\mathrm{EA}}_t - \bar{\pi}^{\mathrm{EA}}_t) + \phi_y(y^{\mathrm{EA}}_t - \bar{y}^{\mathrm{EA}}_t)\right], \qquad (2) \end{aligned}$$

where π^j_t and y^j_t represent HICP inflation and real gross domestic product (GDP) in geographic region $j \in \{\mathrm{AT} = \text{Austria}, \mathrm{EA} = \text{euroarea}\}$ at time t, respectively. $\bar{r}^j_t$, $\bar{y}^j_t$, and $\bar{\pi}^j_t$ denote equilibrium (or "target") levels of real interest rates, real GDP, and inflation in regions j. Finally, ϕ_π and ϕ_y represent policy weights on inflation and output stabilization, respectively. For our benchmark estimates we use Taylor's (1993) original suggestion of equal weights on output and inflation stabilization, i.e., $\phi_\pi = \phi_y = 0.5$. Further, we approximate all equilibrium values for each region j using a Hodrick–Prescott (HP) filter with a smoothing parameter of $\lambda = 1,600$.[10] Figure 2 illustrates this measure at quarterly frequency.

In analogy to Maddaloni and Peydró (2011), we estimate the average effect of "exogenous" changes in banks' cost of short-term funds on the ex-ante risk in

Austrian banks' loan portfolios using regressions of the following form:

$$\mathrm{EDR}_{b,t} = \alpha + \beta \mathrm{gap}_t + \gamma X_{b,t} + \epsilon_{b,t}, \tag{3}$$

where β is the coefficient of interest, capturing the impact of a one percentage point decrease in effective short-term interest rates (an increase in gap_t), and $X_{b,t}$ is a vector of both aggregate and bank-specific control variables, potentially including monthly time trends and a complete set of bank fixed effects.

The differential effect of (expected) persistently low interest rates. The main focus of our paper is to estimate a potentially differential effect of changes in the short-term cost of funds during the period 2003q3–2005q3 (treatment), relative to 2000q1–2003q2 ("pre"-treatment) and 2005q4–2008q3 ("post"-treatment). As a first step, we start with re-estimating equation (3) for the three periods separately. If the pre and post periods suggest approximately equal estimates of β, it is then meaningful to jointly test the differential effect of the treatment period relative to the two counterfactual periods, by augmenting regression model (3) as follows:

$$\mathrm{EDR}_{b,t} = \alpha_0 + \alpha_1 \mathrm{TREAT}_t + \alpha_2 \mathrm{gap}_t + \beta \left(\mathrm{gap}_t \times \mathrm{TREAT}_t\right) + \gamma X_{b,t} + \epsilon_{b,t}, \tag{4}$$

where TREAT_t is an indicator variable for the treatment period, and again β is the coefficient of interest, now capturing the differential impact of a one percentage point decrease in effective short-term interest rates during the treatment period, relative to the two control periods. Put differently, it measures whether Austrian banks reacted more/less strongly to perceived changes in the cost of short-term funding, during a period when the ECB kept the policy rate fixed and at a then unprecedented low.

Due to the potential endogeneity concerns discussed in the preceding section, we propose an additional refinement to our baseline estimates, by focusing explicitly on periods during which the Taylor rule gap was "sufficiently" large. To do so, we define an additional indicator variable:

$$\mathrm{GAP}_t^{\mu} = \begin{cases} 1 \text{ if} & gap_t \geq \mu \\ 0 \text{ if} & gap_t \leq -\mu \end{cases}, \tag{5}$$

isolating periods during which gap_t is at least μ in either direction. Thus, conditional on a particular threshold level μ, a modified version of regression model (4) is then

$$\mathrm{EDR}_{b,t} = \alpha_0^{\mu} + \alpha_1^{\mu} \mathrm{TREAT}_t + \alpha_2^{\mu} \mathrm{GAP}_t^{\mu} + \beta^{\mu} \left(\mathrm{TREAT}_t \times \mathrm{GAP}_t^{\mu}\right) + \gamma^{\mu\prime} X_{b,t} + \epsilon_{b,t}^{\mu}, \tag{6}$$

where β^{μ} is the coefficient of interest. We will illustrate in Section 3, that the interpretation of the estimated coefficient within our baseline analysis (with $\mu = 0.25$) will again (approximately) correspond to the differential effect of a one percentage point decrease in the short-term rate during the treatment period, relative to the two counterfactual periods.

The differential effect of capitalization. Finally, we make use of the cross-sectional dimension in our data set and investigate the impact of capitalization on the effects identified above. Based on theoretical arguments by Holmstrom and Tirole (1997), Jiménez et al. (2014) make the case that due to both moral hazard and search for yield considerations, lowly capitalized banks should have a larger incentive to take on more risk in response to low interest rates than well-capitalized banks. Thus, if the effects identified by the analyses described above are driven by the same mechanism envisioned by Jiménez et al. (2014), then we should expect to see a significantly larger effect for lowly capitalized banks. To assess this hypothesis, we simply run regressions (6) separately for banks with low, medium, and high capitalization.

2.2. The Data Set

Our empirical analysis draws on four main data sources. First, in order to assess individual borrowers' creditworthiness, we utilize annual balance sheets and income statements from an unbalanced panel of 8,653 Austrian firms over the years 1993–2009. This data is collected by the Oesterreichische Nationalbank (OeNB) in the course of its refinancing activities and is stored in a balance sheet register (BILA). The data set also records various auxiliary characteristics, such as the firms' age, legal form, industry classification, and the number of employees. Furthermore, we observe whether a firm went bankrupt and, if so, on which date it filed for bankruptcy protection. Our sample records a total of 533 bankruptcies, which we employ as a proxy for the event of default.

Table 1 displays summary statistics of the firm-level characteristics utilized in this study. One can see that our sample consists of relatively large business whose total assets range from 5 million to 20 billion euros. Further, 72% of the firms in the sample are limited liability companies (GmbH) and 36% operate in the manufacturing sector. On average, firms' liabilities amount to 66% of total assets, whereas bank-liabilities make up 26% of total assets.

Another variable of key importance for our analysis is the ratio of interest expenditure to "gross debt".[11] We interpret this ratio as a proxy for an average firm-level interest rate on firms' debt. In that sense, Austrian businesses in our sample, on average (over time and across different types of debt), paid a real interest rate of 2.9% during our sample period.[12]

In addition to annual firm-specific information, the OeNB collects monthly data on individual loans between Austrian firms and banks in its central credit register (GKE).[13] The sample includes the stocks of credit by Austrian banks to Austrian firms whose total liabilities to Austrian banks exceed 350,000 euros, recorded at monthly frequency. We have access to a matched BILA-GKE sample for the years 2000–2009, which covers 316 Austrian banks and 6,815 firms whose detailed characteristics are also recorded in BILA. Table 2 reports summary statistics for this matched sample. Unfortunately, the OeNB does not record annual balance sheets and income statements for all of the firms whose financial obligations are

TABLE 1. Summary statistics (BILA)

Firm-level information (BILA): 1993–2009	Obs.	Mean	Std. dev.	Min	p^{25}	p^{75}	Max
Accounting ratios							
Liab./assets	47,673	0.66	0.23	0.00	0.52	0.84	1.00
Bank liab./assets	47,661	0.26	0.24	0.00	0.03	0.43	1.00
Liab. short/assets	47,661	0.30	0.21	0.00	0.14	0.42	1.00
Liq. assets/liab short	47,153	1.73	2.04	0.00	0.90	1.87	25.73
Acc. payab./net sales	45,581	0.09	0.12	0.00	0.03	0.10	1.53
Gross profit/Exp. labor	42,420	3.05	3.78	−8.14	1.64	2.90	45.34
Ord. bus. inc./assets	47,652	0.059	0.12	−1.47	0.01	0.10	1.41
Exp. interest/gross debt	47,509	0.03	0.02	0.00	0.01	0.04	0.36
Legal form (indicator)							
Publicly traded (AG)	47,673	0.11	0.32				
Limited liability (GmbH)	47,673	0.72	0.45				
Limited partnership (KG)	47,673	0.13	0.33				
Other	47,673	0.04	0.19				
Industry (indicator)							
Manufacturing	47673	0.36	0.48				
Construction	47,673	0.06	0.23				
Wholesale and trade	47,673	0.22	0.42				
Transportation and Ssorage	47,673	0.04	0.20				
Prof., scient., and tech.	47,673	0.080	0.27				
Admin. and support	47,673	0.01	0.12				
Other	47,673	0.23	0.42				
Age (years)	47,473	18.87	17.66	0.00	7.00	26.00	140.00
Total assets (bill. euros)	47,673	0.07	0.37	0.00	0.01	0.04	20.15
Insolvent within (indicator)							
1 year	47,673	0.001	0.032				
2 years	47,673	0.003	0.052				
3 years	47,673	0.005	0.068				
4 years	47,673	0.007	0.082				
5 years	47,673	0.009	0.093				

Notes: Our measures for firm's legal form, industry as well as insolvency are indicator variables taking the values 0 and 1. The legal form GmbH represents limited liability companies, AG stands for Aktiengesellschaft (equity firms), and KG refers to Kommanditgesellschaft (limited partnerships with at least one fully liable partner). The insolvency indicators summarized here are defined in Appendix A. The columns labeled with p^{25} and p^{75} display the 25th and 75th percentiles, respectively.

in the GKE sample. This is due to the fact that GKE reports are mandated by law, whereas reporting the balance sheet is voluntary. Consequently, our sample of firms is biased toward relatively large and sound businesses, and, therefore, any results on risk-taking found in this study must be interpreted as an estimate of a lower bound for the true amount of risk-taking. In addition to the raw data, Table 2 also summarizes the expected default rate within banks' business-loan portfolios

TABLE 2. Summary statistics (GKE, MONSTAT, and ECB)

	Obs.	Mean	Std. dev.	Min	p^{25}	p^{75}	Max
Bank-level information (GKE and MONSTAT): 2000–2008							
$\text{EDR}_{b,t}$	27,082	0.52	0.64	0.00	0.23	0.62	18.46
Bank: Capitalization (indicator, 1–5)							
Cap. 1	27,082	0.14	0.35				
Cap. 2	27,082	0.18	0.39				
Cap. 3	27,082	0.35	0.48				
Cap. 4	27,082	0.30	0.46				
Cap. 5	27,082	0.03	0.17				
Bank: Cash ratio (indicator, 1–3)							
Cash Rat. 1	27,082	0.32	0.47				
Cash Rat. 2	27,082	0.39	0.49				
Cash Rat. 3	27,082	0.29	0.45				
Bank: Size by assets (indicator, 1–3)							
Size 1	27,082	0.661	0.473				
Size 2	27,082	0.26	0.44				
Size 3	27,082	0.08	0.28				
Bank: No. of loans	27,082	26.55	102.89	1.00	3.00	12.00	1847.00
Aggregate characteristics (ECB): 2000–2008							
i_q^{ECB}	27,082	3.09	0.91	2.00	2.00	4.00	4.75
gap_q^{TR}	27,082	0.21	0.95	−1.27	−0.47	0.60	2.93
$y_q - y_q^*$	27,082	0.32	1.33	−1.56	−0.80	1.61	3.09
π_q^{AT}	27,082	2.03	0.61	1.07	1.73	2.20	3.70
$i_q^{10,\text{AT}} - i_q^{3,\text{EA}}$	27,082	1.04	0.80	−0.41	0.33	1.65	2.26
$i_q^{10,\text{AT}} - i_q^{10,\text{EA}}$	27,082	0.02	0.05	−0.06	−0.03	0.06	0.13
AT bank-;oans/total assets	27,082	0.36	0.02	0.33	0.35	0.37	0.39
GKE credit/AT banklLoans	27,082	0.43	0.02	0.40	0.41	0.46	0.46

Notes: Our measures for banks' capitalization, cash ratio, and size by assets are indicator variables taking the values 0 and 1. The columns labeled with p^{25} and p^{75} display the 25th and 75th percentiles, respectively.

($\text{EDR}_{b,t}$) as well as the Austrian Taylor rule gap (gap_t), which are defined in equations (1) and (2), respectively.

EMU member states are further required to collect detailed balance sheet information on their monetary and financial institutions (MONSTAT).[14] Unfortunately, due to Austrian data confidentiality restrictions, we were not allowed to match this detailed bank-level information at the bank level to our sample of matched firm–bank pairs. However, we were allowed to merge discrete categories of key bank-level characteristics that vary on an annual frequency. The top panel of Table 2 illustrates our measures for banks' capitalization, liquidity, and size for the matched BILA-GKE sample.[15]

Finally, all aggregate data are drawn from the ECB's statistical data warehouse.[16] The bottom panel of Table 2 reports summary statistics on these aggregate

TABLE 3. The average effect of a lower short-term rate

	Dependent variable: Ex-ante expected default rate ($EDR_{b,t}$)				
	(1)	(2)	(3)	(4)	(5)
A: Continuous regressor (gap_t)					
gap	−0.0087	−0.0066	−0.0031	0.033**	0.040***
	(0.019)	(0.018)	(0.020)	(0.016)	(0.015)
Bank controls		Yes	Yes	Yes	Yes
Bank FEs			Yes	Yes	Yes
AT controls				Yes	Yes
Trend					Yes
No. banks	316	316	316	316	316
Obs.	27,082	27,082	27,082	27,082	27,082
B: Discrete regressor ($GAP_t^{0.25}$)					
GAP	−0.024	−0.021	−0.020	0.016	0.038
	(0.026)	(0.025)	(0.025)	(0.029)	(0.026)
Bank controls		Yes	Yes	Yes	Yes
Bank FEs			Yes	Yes	Yes
AT controls				Yes	Yes
Trend					Yes
No. banks	316	316	316	316	316
Obs.	15,827	15,827	15,827	15,827	15,827

Notes: The dependent variable is the ex-ante expected default rate ($EDR_{b,t}$) at the bank level. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are two-way clustered on bank and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

variables. An important statistic for the purpose of this study is the average proportion of business loans within banks' balance sheets, which was 36%, on average, and was ranging between 33% and 39% between 2000 and 2008, respectively.

3. EMPIRICAL RESULTS

As a baseline, we begin our empirical analysis with estimates based on regression model (3), which are summarized in Table 3.[17] Panel A effectively confirms the results by Maddaloni and Peydró (2011) for the case of Austrian business lending, suggesting that lower perceived short-term interest rates induce significantly higher expected default rates throughout 2000–2008, on average. Although Maddaloni and Peydró (2011) specifically focus on lending standards, their results on weakened lending standards are fully consistent with our finding of increased expected default rates. These baseline results are also consistent with a host of other studies that estimate the effect of changes in short-term interest rates on various measures of bank-risk [e.g., Jiménez et al. (2014)].[18]

Column (1) of Table 3 illustrates the unconditional relationship between ex-ante expected default rates at the bank level and the Taylor rule gap (TR gap)

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described in Section 2. As discussed in Section 2, a rise in this gap captures a perceived decrease in the short-term cost of external funds from the perspective of Austrian banks. Columns (2)–(5) consecutively add bank-level and aggregate control variables, a full set of bank fixed effects, as well as linear and quadratic time trends. Specifically, to control for bank-level heterogeneity, column (2) includes control variables for banks' capitalization, cash ratio, and size by total assets. On top of that, we add a complete set of bank fixed effects in column (3), in order to absorb any additional time-invariant, unobserved bank heterogeneity. These bank-level control variables in part capture the effects of changes in financial regulation that were going on during this period—first and foremost the structural changes due to (the preparation for) the Basel II accord, which became legally binding in Austria as of January 1, 2007.

In order to credibly compare the effects of changes in short-term interest rates at different points in time throughout our sample, it is key to also control for general dynamics in the aggregate state of the economy. Therefore, specification (4) includes a number of aggregate control variables: first, we account for the general business cycle by including Austrian output gaps ($y_t^{\mathrm{AT}} - \bar{y}_t^{\mathrm{AT}}$) and inflation ($\pi_t^{\mathrm{AT}}$). We further presume that both term and country-risk-premia might play a role for the measured risk in banks' portfolios. We address this concern by including the spread between Austrian 10-year bond yields and 3-month EA money market rates, $i_t^{10,\mathrm{AT}} - i_t^{3,\mathrm{EA}}$, to proxy term-spreads and the spread between the yields of Austrian 10-year and EA 10-year bonds, $i_t^{10,\mathrm{AT}} - i_t^{10,\mathrm{EA}}$. As we point out in our discussion of the most recent credit cycle in Austria in Appendix B, there was a substantial increase in real business-lending activity toward the end of the treatment period. To make sure that our estimates are not entirely driven by this significant credit expansion, particularly during the year 2005, we also include real Austrian business-loan growth, as depicted in panel (B) of Figure B.1 in Appendix B. Another concern is the restriction of our analysis to business-lending only. While business lending in Austria amounts to roughly 40% of all lending, alternative sources of external funding became significantly more popular throughout our sample period from 2000 to 2008. Thus, we include the aggregate fraction of Austrian business lending in banks' total assets to rule out that changes in the importance of business lending are driving our results. Apart from concerns about aggregate changes in the Austrian economy, we are also worried about changes in the aggregate representativeness of our unbalanced panel of firm–bank pairs. For that reason, we further include the ratio of aggregate lending within our GKE sample as a fraction of overall Austrian business lending. Finally, to control for medium-term trends in expected default rates that are unrelated to general economic conditions, specification (5) adds linear and quadratic monthly time trends.

Fully consistent with Maddaloni and Peydró (2011), model (5) suggests that, conditional on the aggregate state of the economy, a lower external cost of funding tends to increase the ex-ante expected default rate in banks' loan portfolios throughout 2000–2008. However, as we have argued above, there is concern that the estimates in panel A may be biased whenever economic conditions are very similar in Austria and the euro area as a whole. To address this concern, we restrict the analysis to periods in which there is at least a 25 basis point TR gap in either direction, by replacing the main regressor with the indicator GAP_t^{μ} (with $\mu = 0.25$), defined in equation (5).

Panel B of Table 3 summarizes the results and shows quantitatively similar coefficients as in panel A, yet the estimates are less precise and therefore no longer statistically significant. We note that the sample averages $\text{Avg}[\text{gap}_t|\text{GAP}_t^{0.25} = 1] = 0.55$ and $\text{Avg}[\text{gap}_t|\text{GAP}_t^{0.25} = 0] = -0.6$ suggest that comparing periods when $\text{GAP}_t^{0.25} = 1$ to periods when $\text{GAP}_t^{0.25} = 0$ approximately reflects the effects of a one unit increase in the TR gap (gap_t). Thus, the magnitude of the coefficients in panel B should be comparable to those in panel A. Indeed, the point estimate for our preferred, fully satiated specification (5) is only marginally smaller in magnitude. However, the standard errors are now larger, despite capturing the effects of comparable "shocks".[19]

One way to interpret the results from Table 3 is that the highly significant estimates in panel A are perhaps largely identified from small changes in perceived short-term rates, ones that are more likely due to ECB policy actions that are endogenous to Austrian economic conditions. However, as we will show throughout the remainder of this section, an alternative possibility is that the risk-taking appetite of banks crucially depends on the medium-term policy stance of the relevant monetary authority, and an "average" estimate over different policy regimes may largely wash out.

In particular, we focus on the hypothesis that the ECB's stance of policy during the treatment period may have altered the way banks respond to perceived changes in the short-term cost of funds [e.g., Borio and Zhu (2008), Diamond and Rajan (2012)]. As a first step toward investigating this hypothesis, we redo the above analysis for three separate sub-samples (pre: 2000q1–2003q2, treat: 2003q3–2005q3, and post: 2005q4–2008q3) and the results are summarized in Table 4. This table reports fully satiated specifications, analogous to model (5) in Table 3, and reveals a striking result. The positive overall coefficient appears to stem entirely from the treatment period. Moreover, this result is highly significant, and robust toward excluding periods of very small changes in the perceived cost of funds (panel B).

Moreover, given that the point estimates for the pre and post periods in panel B are surprisingly similar (both in sign and magnitude), this suggests that the pre and post periods may serve as meaningful counterfactual periods, representing "usual" policy, in which the policy rate is adjusted largely in lockstep with economic activity. We will use this insight in order to tease out the differential impact of the treatment period, during which the policy rate was largely expected to remain low and constant for an extended period.

To quantify this differential impact, Table 5 shows the results from regression models (4) and (6), respectively using the continuous (gap_t) and discrete ($\text{GAP}_t^{0.25}$) versions of our TR gap measure. In analogy to the results shown in Table 3, model

TABLE 4. Split sample: Pre/treatment/post

	A: Continuous regressor (gap_t)			B: Discrete regressor ($GAP_t^{0.25}$)		
	Pre (A.1)	Treat (A.2)	Post (A.3)	Pre (B.1)	Treat (B.2)	Post (B.3)
Dependent variable: Ex-ante expected default rate ($EDR_{b,t}$)						
gap	-0.024^{**}	0.064^{**}	0.050			
	(0.0093)	(0.024)	(0.040)			
GAP				-0.24^{**}	0.19^{**}	-0.46^{***}
				(0.087)	(0.083)	(0.067)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
AT controls	Yes	Yes	Yes	Yes	Yes	Yes
Trend	Yes	Yes	Yes	Yes	Yes	Yes
No. banks	282	288	310	280	288	310
Obs.	9,903	7,186	9,991	4,950	5,616	5,260

Notes: The dependent variable is the ex-ante expected default rate ($EDR_{b,t}$) at the bank level. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are two-way clustered on bank and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

(1) shows the unconditional differential impact of the treatment period, whereas specifications (2)–(5) consecutively add the same aggregate and bank-specific control variables as in Table 3.

The coefficient measuring the differential impact of the treatment period is the interaction between the treatment dummy and the measure of the TR gap (continuous in panel A and discrete in panel B). We note that this coefficient is sizable, highly significant, and incredibly robust across all five specifications. Moreover, the differential impact of the treatment period is equally robust in both the continuous and discrete specifications. In fact, the discrete specification, which we argue is less likely biased due to endogeneity of the ECB's policy decisions, now suggests an even larger effect than in the continuous specification.

Thus, if the impact of perceived changes in the cost of funds during the pre and post periods indeed reflect the typical risk-taking response in the presence of the "normal ECB policy"—when policy rates are adjusted in lockstep with economic activity—, then these estimates suggest that the stance of policy during the treatment period induced a substantially larger risk-taking response. In particular, the response to a perceived one percentage point decrease in the cost of short-term funds was approximately 0.15–0.2 percentage points larger during the treatment period than it was during the two counterfactual periods, which amounts to a sizable 28–38% increase relative to the average expected default rate of 0.525% throughout our sample.

TABLE 5. Differential treatment effect

	Dependent variable: Ex-ante expected default rate ($EDR_{b,t}$)				
	(1)	(2)	(3)	(4)	(5)
A: Continuous regressor (gap_t)					
TREAT	0.10***	0.10***	0.11***	0.054*	0.031
	(0.034)	(0.034)	(0.033)	(0.027)	(0.028)
gap	−0.097***	−0.089***	−0.11***	−0.055**	−0.027
	(0.026)	(0.026)	(0.025)	(0.025)	(0.024)
TREAT×gap	0.14***	0.13***	0.19***	0.12***	0.12***
	(0.029)	(0.031)	(0.027)	(0.028)	(0.031)
Bank controls		Yes	Yes	Yes	Yes
Bank FEs			Yes	Yes	Yes
AT controls				Yes	Yes
Trend					Yes
No. banks	316	316	316	316	316
Obs.	27,082	27,082	27,082	27,082	27,082
B: Discrete regressor ($GAP_t^{0.25}$)					
TREAT	−0.026	−0.014	−0.035*	−0.072***	−0.10***
	(0.022)	(0.025)	(0.021)	(0.021)	(0.032)
GAP	−0.12***	−0.11***	−0.13***	−0.071	−0.042
	(0.027)	(0.028)	(0.026)	(0.048)	(0.082)
TREAT×GAP	0.21***	0.19***	0.23***	0.16***	0.17**
	(0.035)	(0.036)	(0.037)	(0.051)	(0.085)
Bank controls		Yes	Yes	Yes	Yes
Bank FEs			Yes	Yes	Yes
AT controls				Yes	Yes
Trend					Yes
No. banks	316	316	316	316	316
Obs.	15,827	15,827	15,827	15,827	15,827

Notes: The dependent variable is the ex-ante expected default rate ($EDR_{b,t}$) at the bank level. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are two-way clustered on bank and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

3.1. Robustness Checks

Despite the many aggregate control variables, discussed in the preceding section, one might raise the concern that the two counterfactual periods, pre (2000q1–2003q2) and post (2005q4–2008q3), perhaps represent two episodes with fundamentally different economic environments and should therefore not be lumped together. For example, the pre period was a downturn and the post period was an expansion in Austria, both in absolute terms and relative to the euro area. In order to investigate this concern, Table 6 compares three variations of model (5)

TABLE 6. Separate pre and post counterfactuals

	A: Continuous regressor (gap_t)			B: Discrete regressor ($GAP_t^{0.25}$)		
	Pre–treat (A.1)	Treat–post (A.2)	Pre–treat–post (A.3)	Pre–treat (B.1)	Treat–post (B.2)	Pre–treat–post (B.3)
Dependent variable: Ex-ante expected default rate ($EDR_{b,t}$)						
gap	−0.027***	−0.023	−0.039***			
	(0.0079)	(0.053)	(0.010)			
TREAT	−0.051	−0.059	0.067*	−0.011	−0.31***	−0.059***
	(0.056)	(0.052)	(0.038)	(0.0071)	(0.091)	(0.015)
TREAT × gap	0.054**	0.098**	0.15***			
	(0.027)	(0.047)	(0.022)			
POST			0.092			0.16***
			(0.067)			(0.056)
POST × gap			0.022			
			(0.036)			
GAP				−0.28**	−0.18*	−0.15
				(0.13)	(0.10)	(0.091)
TREAT × GAP				0.33***	0.38***	0.35***
				(0.11)	(0.077)	(0.12)
POST × GAP						0.12
						(0.10)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
AT controls	Yes	Yes	Yes	Yes	Yes	Yes
Trend	Yes	Yes	Yes	Yes	Yes	Yes
No. banks	296	312	316	295	312	316
Obs.	17,089	17,178	27,082	10,566	10,877	15,827

Notes: The dependent variable is the ex-ante expected default rate ($EDR_{b,t}$) at the bank level. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are two-way clustered on bank and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

from Table 5, both for the continuous (panel A) and discrete (panel B) versions of the main regressor. First, columns (A.1) and (B.1) exclude the post period and therefore look at the differential impact of the treatment period relative to the pre period. Columns (A.2) and (B.2) analogously exclude the pre period. Reassuringly, we find that the differential effect of the treatment period relative to either counterfactual period in isolation is still positive and significant, with the indication that the effect in the post period might actually be slightly more pronounced. Finally, columns (A.3) and (B.3) use the entire sample but separately control for the differential impact of the post period. Again, the differential effect of the treatment period is significant and positive, yet slightly larger than the original estimates reported in Table 5.

Moreover, note that the additional interaction term in specifications (A.3) and (B.3) captures the differential impact in the post period, relative to the pre period. It is reassuring that the point estimates for this interaction term in both the continuous and discrete specification are small and statistically insignificant. This suggests that the estimated effects in the pre and post periods are indeed statistically indistinguishable, and therefore give further credence to our identification strategy.

In addition, this insight further suggests that other relevant policy changes during the treatment period, which we cannot directly control for, are likely not the main drivers of our results. Examples for such changes are financial innovation, systematic changes in risk-management practices, as well as more restrictive capital adequacy requirements—in preparation for the Basel II accord. In Austria, all these structural changes took place during the treatment period and *remained* in place thereafter. Thus, if they were driving the results, then one should see a significant differential impact in the pre comparison but not in the post comparison. Or alternatively, we would expect the additional interaction term in specifications (A.3) and (B.3) to be similar in magnitude to the treatment interaction—a hypothesis we can confidently reject.

An additional concern is that our identification strategy critically hinges on the assumption that Austrian banks predominantly base their lending decisions on economic conditions in Austria. As argued in the preceding sections, the majority of Austrian banks is very small, and almost half of business lending is concentrated within the largest 5% of banks (16 out of 316 banks in our sample). Table 7 shows our test when excluding the largest 1% of banks (17% of all lending, columns A.2 and B.2) as well as the largest 5% of banks (47% of all lending, columns A.3 and B.3). These alternative specifications clearly show that our results are not driven by these largest 16 banks, as the estimated differential treatment effect based on the remaining 300 banks is in fact marginally larger.

Moreover, all results presented here are conditional on our particular choice of gap measure, which we used to identify periods during which ECB policy is out of sync with Austrian economic conditions. We alternatively use inflation as well as real GDP gaps individually, to compute the AT–EA gap measure and do not find any significant qualitative differences. The resulting estimates (for our main specifications) are reported in Appendix C. We also check whether the particular threshold of $\mu = 0.25$ for the discrete regressor plays a significant role and use $\mu = 0.15$, $\mu = 0.2$ and $\mu = 0.3$ as alternatives. The resulting estimates (for our main specifications) are reported in Appendix E. Although the magnitude of the estimated effects with these alternative gap measures and thresholds is harder to interpret (as the estimated impact no longer directly corresponds to a "one unit" change in a gap measure that resembles short-term interest rates), none of these sensitivity checks substantially change the qualitative results.

Finally, since we are estimating the effect on the overall expected default rate at the bank level, we cannot directly condition on observed and unobserved borrower characteristics. Although the main goal of this paper is to examine the overall effect on the bank's portfolio, we can nevertheless check whether the effects identified at

TABLE 7. The role of bank size (market share)

	A: Continuous regressor (gap_t)			B: Discrete regressor ($\text{GAP}_t^{0.25}$)		
	All (A.1)	Bottom 99% (A.2)	Bottom 95% (A.3)	All (B.1)	Bottom 99% (B.2)	Bottom 95% (B.3)
Dependent variable: Ex-ante expected default rate ($\text{EDR}_{b,t}$)						
TREAT	0.031	0.032	0.035	-0.10^{***}	-0.11^{***}	-0.11^{***}
	(0.028)	(0.028)	(0.029)	(0.032)	(0.033)	(0.035)
gap	−0.027	−0.026	−0.028			
	(0.024)	(0.024)	(0.025)			
TREAT×gap	0.12^{***}	0.12^{***}	0.13^{***}			
	(0.031)	(0.031)	(0.033)			
GAP				−0.042	−0.042	−0.048
				(0.082)	(0.084)	(0.089)
TREAT×GAP				0.17^{**}	0.18^{**}	0.20^{**}
				(0.085)	(0.087)	(0.092)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
AT controls	Yes	Yes	Yes	Yes	Yes	Yes
Trend	Yes	Yes	Yes	Yes	Yes	Yes
No. banks	316	312	300	316	312	300
Obs.	27,082	26,670	25,434	15,827	15,587	14,867

Notes: The dependent variable is the ex-ante expected default rate ($\text{EDR}_{b,t}$) at the bank level. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are two-way clustered on bank and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

the bank level are similar at the firm-bank level. To do so, we estimate regressions (4) and (6) at the firm-bank level, using the risk-weighted balance at the firm-bank level as the dependent variable. Specifically, using the same notation as in equation (1), we define the risk-weighted balance between firm f and bank b at time t as

$$\text{RWB}_{f,b,t} = \frac{p_{f,t}^{h} L_{f,b,t}}{\sum_{f \in \mathcal{F}_{b,t}} L_{f,b,t}}. \tag{7}$$

Notice that we still normalize the risk-weighted balance with the total outstanding loan balance of bank b at time t, in order to make this number comparable across banks. The benefit of moving the analysis to the firm-bank level is that we can now include firm fixed effects, to control for any time invariant unobserved firm characteristics, beyond the expected default rate $p_{f,t}^{h}$. We note that we cannot include other relevant firm characteristics as regressors, as these have been used in the estimation of $p_{f,t}^{h}$, and would therefore be endogenous.

Reassuringly, columns (A.1) and (B.1) of Table 8 reveal that the firm-bank-level estimates qualitatively mirror the bank-level results. However, although the

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TABLE 8. Firm-bank-level estimates

	A: Continuous regressor (gap_t)				B: Discrete regressor ($GAP_t^{0.25}$)			
		Capitalization				Capitalization		
	All (A.1)	Low cap. (A.2)	Med. cap. (A.3)	High cap. (A.4)	All (B.1)	Low cap. (B.2)	Med. cap. (B.3)	High cap. (B.4)
Dependent variable: Ex-ante risk-weighted balance ($RWB_{b,f,t}$, fraction of bank's total outstanding loan balance)								
TREAT	−0.0017	−0.00068	−0.0051	0.012	−0.0028*	−0.0010	−0.0044	0.0096
	(0.0015)	(0.00098)	(0.0047)	(0.021)	(0.0017)	(0.0014)	(0.0070)	(0.023)
gap	0.0018	0.0010	−0.000045	−0.0075				
	(0.0027)	(0.0011)	(0.0040)	(0.019)				
TREAT×gap	0.0035**	0.0018*	0.0014	−0.00077				
	(0.0015)	(0.00092)	(0.0045)	(0.016)				
GAP					−0.0026	−0.00056	0.00082	−0.017
					(0.0023)	(0.00090)	(0.0074)	(0.014)
TREAT×GAP					0.0068***	0.0027**	0.0030	0.011
					(0.0021)	(0.0013)	(0.0063)	(0.019)
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. banks	316	202	235	211	316	201	235	211
No. firms	5,396	4,225	3,607	2,864	5,383	4,208	3,591	2,855
Obs.	551,886	307,212	155,887	88,688	445,018	251,556	116,644	76,714

Notes: The dependent variable is the ex-ante risk-weighted balance between borrower (firm) f and bank b ($RWB_{r,b,t}$) expressed as a fraction of bank b's total loan balance in month t. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are multi-way clustered on bank, firm and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

bank-level estimates of the treatment interaction (reported in Table 5) suggest a 28–38% differential increase relative to the average expected default rate of 0.525%, the firm-bank-level estimates indicate a slightly smaller 13.5–26.3% differential increase relative to the average risk-weighted balance at the firm-bank level of 0.0258%. This suggests that our main results at the bank level are likely not exclusively driven by the fact that we aggregate to the bank level or by unobserved firm heterogeneity.

3.2. The Role of Bank Capitalization

The analysis in the preceding sections has exclusively relied on variation over time in order to identify the differential impact of the treatment period on expected default rates within Austrian banks' business loan portfolios. Although we have controlled for a variety of potentially confounding aggregate developments, it is still possible that other, unobserved, time-specific events may be biasing our results. In order to address this concern, we conduct one final test that exploits the cross-section of our bank panel. In particular, we appeal to the argument that more pronounced moral hazard problems may cause lowly capitalized banks to react more strongly to the risk-taking incentive from cheap short-term funds, compared to other, better capitalized banks [Holmstrom and Tirole (1997), Jiménez et al. (2014)].

Within our framework, we can check this hypothesis by splitting our sample into banks with low, medium, and high capitalization. Table 9 shows this analysis for both the continuous and discrete specifications, revealing that the estimated effect for lowly capitalized banks (columns A.1 and B.1) is very close to the overall estimated effects in column (5) of Table 5. To the contrary, the estimated treatment effect for banks with medium capitalization is about half the size and insignificant (columns A.2 and B.2), whereas it is marginally negative and insignificant for highly capitalized banks (columns A.3 and B.3). Reassuringly, these results are mirrored by the analogous firm-bank-level estimates reported in columns (A.2)–(A.4) and (B.2)–(B.4) of Table 8.

This alternative test also suggests that our estimates are likely due to credit supply, i.e., the bank's choice, rather than credit demand. We make this argument because we think it is unlikely that loan applications during the treatment period were *both* systematically more risky *and* systematically biased toward lowly capitalized banks, relative to the control periods. Thus, we conclude that the estimates shown throughout Section 3 are likely due to a risk-taking incentive for banks, which particularly materializes in times when policy rates are expected to remain low and constant.

4. CONCLUDING REMARKS

Our empirical findings point to a channel of the transmission mechanism of monetary policy, which is triggered by an extended period of expected accommodating

TABLE 9. The role of capitalization

	A: Continuous regressor (gap_t)			B: Discrete regressor ($GAP_t^{0.25}$)		
	Low (A.1)	Medium (A.2)	High (A.3)	Low (B.1)	Medium (B.2)	High (B.3)
Dependent variable: Ex-ante expected default rate ($EDR_{b,t}$)						
TREAT	0.031	−0.010	0.032	−0.029	−0.060	−0.18*
	(0.035)	(0.042)	(0.068)	(0.027)	(0.060)	(0.092)
gap	−0.015	−0.051	0.046			
	(0.016)	(0.033)	(0.039)			
TREAT×gap	0.11***	0.057	−0.021			
	(0.031)	(0.050)	(0.10)			
GAP				−0.12	−0.16	0.33
				(0.075)	(0.094)	(0.30)
TREAT×GAP				0.25**	0.14	−0.11
				(0.094)	(0.11)	(0.23)
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
AT controls	Yes	Yes	Yes	Yes	Yes	Yes
Trend	Yes	Yes	Yes	Yes	Yes	Yes
No. banks	202	235	211	189	230	211
Obs.	8,792	9,472	8,817	4,889	5,420	5,517

Notes: The dependent variable is the ex-ante expected default rate ($EDR_{b,t}$) at the bank level. The table summarizes the main coefficients of interest. Detailed regression results are presented in Appendix E. Standard errors are reported in parentheses below each coefficient and are two-way clustered on bank and year-month following Cameron et al. (2011). Significance levels are indicated by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

monetary policy. Borio and Zhu (2008) have dubbed this phenomenon the "risk-taking channel" of monetary policy, and one possible theory for such a mechanism was recently put forward by Diamond and Rajan (2012). In particular, they argue that this mechanism has the property that its consequences for real activity need not materialize within a short period of time—in contrast to the traditional transmission channels.[20]

In fact, as the direct effect of this mechanism is a deterioration of financial institutions' risk-positions, it might not result in any significant implications for the real economy under "normal" circumstances. However, in the unlikely event of a significant disruption of financial markets—like the failure of Lehman Brothers in the fall of 2008, which resulted in a global panic among investors roughly 3 years after the deterioration of banks' balance sheets had taken place—more "fragile" bank balance sheets might significantly amplify the repercussions of a "shock" to the financial system. Diamond and Rajan (2012) not only argue that the additional portfolio risk is inefficient but also that it increases the probability for future bailouts. The former is because private agents do not internalize the social cost of future policy interventions, whereas the latter is due to increased aggregate risk exposure. Although our results offer no insights regarding efficiency, they are nevertheless consistent with these theories, as they suggest differentially more risk-taking among Austrian banks during the period of low and constant ECB policy rates.

In light of these arguments it is not surprising that Roubini and Mihm compare the mechanisms that lead to the 2007/2008 financial turmoil to the fault lines that eventually lead to an earthquake [Roubini and Mihm (2010), p. 62]:

> [...] [T]he pressures build for many years, and when the shock finally comes, it can be staggering. [...] The collapse revealed a frightening truth: the homes of subprime borrowers were not the only structures standing on the proverbial fault line; countless towers of leverage and debt had been built there too.

Moreover, the deterioration of banks' balance sheets during the mid-2000s was likely amplified by the substantial increase in the quantity of lending, which is consistent with traditional channels of monetary policy. This amplification is likely to have happened, as the outstanding boom in lending activity during the 2000s significantly increased the size of the financial sector, and hence, made any sudden failure of this market even more detrimental to the overall economy.

Thus, the peculiar nature of this so-called "risk-taking channel" suggests that future monetary policy should perhaps take possible effects on financial stability more explicitly into account. In particular, Diamond and Rajan (2012) argue that central banks should preempt this channel and "raise rates in normal times [beyond the level predicted by standard theory] to offset distortions from reducing rates in adverse times." Farhi and Tirole (2012) make a similar case, yet they favor "macro-prudential supervision" over undirected interest rate policy.

NOTES

1. For similar arguments, see Allen and Gale (2000) as well as broader discussions of this so-called "risk-taking channel" of monetary policy by Borio and Zhu (2008) and Adrian and Shin (2010).

2. Similar empirical studies include, for example, Delis and Kouretas (2011), Maddaloni and Peydró (2011), Altunbas et al. (2014), Buch et al. (2014a,b), Paligorova and Santos (2017), or Dell'Ariccia et al. (2017) and references therein.

3. The data set is strictly confidential and was provided by the Oesterreichische Nationalbank (OeNB). Access to the anonymized individual data is granted by the OeNB on a case-by-case basis. Contact information can be found at www.oenb.at/.

4. See, for example, Schneider (2013), who documents stagnant Austrian housing prices during our sample period. Moreover, Redak and Weiss (2004) show that the use of credit derivatives at that time was very limited in Austria, particularly compared to other countries.

5. Despite the fact that the ECB never explicitly announced to keep its main refinancing rate constant for an extended period, there is evidence that markets were expecting overnight rates to stay low for an extended period. For example, the 1-year euro overnight swap rate was on average 2.2% during 2003q3–2005q3. Based on monthly observations and Newey–West standard errors, this average was significantly below 2.25%. This implies that, up until the end of 2005, markets were expecting overnight rates to remain on average below 2.25% for at least one more year. See, for example, Taylor and Williams (2009) for an explicit formulation of the no-arbitrage argument underlying this interpretation of overnight swap rates.

6. The main argument is that lowly capitalized banks have the least "skin in the game" and therefore have a larger incentive to place riskier bets. This amplifies the moral hazard problems described by Diamond and Rajan (2012).

7. Supplementary materials are available in an Online appendix, which is available at Paul Gaggl's research website and is structured as follows: Appendix A presents the construction of the main empirical measure of portfolio-risk (based on logit models for firms' probability of default), and detailed regression tables are provided in Appendix E. Appendices B–D report other supplementary materials.

8. In Appendix C, we present qualitatively equivalent results for several alternative measures of economic conditions. However, the magnitudes of the effects are obviously harder to interpret.

9. Our sensitivity analysis in Appendix D shows that this feature does not depend on the particular parameterization of the Taylor rule.

10. See Appendix D for investigations on the robustness to alternative Taylor rule specifications.

11. We define "gross debt" as liabilities net of long-term reserves as well as provisions for pensions and other social transfers.

12. In order to reduce the impact of measurement error on our results we drop "implausible" observations, such as negative values for total assets, entries where detailed balance sheet positions do not correctly sum up to reported aggregates, etc. Further, we identify observations that exceed five times the distance between the 5th and 95th percentile of the cross-sectional distribution in either direction as statistical outliers. We run our empirical analyses with and without the identified outliers and find no significant qualitative differences.

13. Details on the data collection criteria can be found in the official standards for reporting to the central credit register, which are publicly available at http://www.oenb.at/. The individual data on both firms and banks are strictly confidential. Access to the anonymized individual data, as employed in this study, is granted by the OeNB on a case-by-case basis. Contact information can be found at www.oenb.at/.

14. For details see http://europa.eu/legislation_summaries/economic_and_monetary_affairs/institutional_and_economic_framework/l25044_en.htm.

15. The matching of the two data sets was conducted by the OeNB's credit department and the matched version was delivered to us in completely anonymized form.

16. See http://sdw.ecb.europa.eu/.

17. Throughout the paper, we show summary tables reporting only the key coefficients of interest, but we provide detailed tables with all coefficient estimates in Appendix E.

18. For other examples, see Delis and Kouretas (2011), Altunbas et al. (2014), Buch et al. (2014a,b), Paligorova and Santos (2017), and Dell'Ariccia et al. (2017).

19. We note at this point, that standard errors for all bank-level regressions throughout this paper are two-way clustered [Cameron et al. (2011)] on bank and year-month in order to allow for serially correlated errors within banks as well as correlated errors within a given month across banks.

20. Traditional transmission channels tend to work fairly "quickly." See, for instance, Christiano et al. (1996) or Christiano et al. (2007), who find that real activity tends to respond within about a year to temporary movements in short-term policy interest rates. Furthermore, the latter study also finds fairly quick responses of borrowers' net worth.

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