Course: Banking and Contract Economics

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June 6, 2025

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1 Julius Baer Write-Down of Private Debt Business and Relationship Banking for HNWI

1.1 The incentives and risks inherent in two-period lending relationships

Two-Period Setup

We model Julius Baer's lending relationship with Signa as a two-period lending game in a RNND environment:

- **Period 0**: Julius Baer incurs sunk information cost γ to underwrite and understand Signa's risk.
- **Period 1**: It lends 1 unit (bridge loan).
- **If Signa succeeds (with probability p)**: It receives follow-up funding in Period 2 (new loan of 1 unit), repaid with R_2.
- The total expected bank cost is $1 + \gamma + p$.
- The total expected repayment is $pR_1 + p^2 R_2$.

Under competitive zero-profit equilibrium, we have:

$$pR_1 + p^2 R_2 = 1 + \gamma + p \qquad \text{(3.23 of coursebook)}$$

Outside Bank Threat at $t = 1$

If Signa had tried to switch to another lender at $t = 1$, the new bank would have needed to incur the information cost γ for one period of lending, and thus:

$$R_2 = \frac{1+\gamma}{p} \qquad \text{(3.24 of coursebook)}$$

Combining these two conditions gives the equilibrium level of R_1:

$$R_1 = \frac{1 + \gamma \times (1-p)}{p} < R_2 = \frac{1+\gamma}{p} \qquad (1)$$

This creates a **lock-in effect**, where Julius Baer can extract profits from the ongoing relationship.

Incentives

- **Julius Baer** expected to earn future income from follow-on lending, repaid via R_2, without needing to re-invest in due diligence. As a result, the bank is willing to take a loss in the first period. The coursebook has extensive explanations on this part.

Risks

- Misaligned incentives and monitoring gaps: In a two-period relationship, low initial repayments and the need to preserve future profits can discourage banks from funding positive-NPV projects.
- Lock-in and excessive risk tolerance: The bank's ex-post monopoly power may lead to leniency in lending standards and an overcommitment to troubled clients. Long-term dependence can result in inefficient capital allocation and difficulty exiting relationships when risks materialize—turning relational strength into a liability.

1.2 The nature of information asymmetries and the role of monitoring costs, and the nature of collateral used in the failed deal

To enrich the two-period model, we introduce parameters of **illiquid collateral**, **information asymmetry**, and **ongoing monitoring costs**.

1. **In period 1**, the borrower offers illiquid collateral worth c, which the bank recovers if the projects fails at the first period.
2. **In period 2**, the borrower's true success probability deteriorates from p to $p' < p$. The bank can learn this only by paying a monitoring cost m.
3. **If the bank does not monitor**, it mistakenly prices the second-period loan as if the probability remains p, and expects repayment:

$$pR_1 + (1-p)c + p^2R_2 + p(1-p)c = 1 + \gamma + p$$

$$pR_2 + (1-p)c = 1 + \gamma$$

As in classical RNND model, we derive:

$$R_2 = \frac{1 + \gamma - (1-p)c}{p}$$

However, the **actual expected repayment** based on true probability p' and collapsed collateral ($c' = 0$) is:

$$p'R_2 + (1 - p') \times 0 = p'R_2 < pR_2 + (1 - p)c = 1 + \gamma$$

This implies that the bank suffers a **hidden expected loss**:

$$L = pR_2 + (1 - p)c - p'R_2 = 1 + \gamma - p'R_2$$

The bank can avoid this expected loss by monitoring at cost m. In equilibrium, we would have $L > m$. If we know in addition that this bad scenario happens at probability p_{bad}, $m = Lp_{bad}$. If we know that the bank monitors, adding the monitoring cost m will unavoidably increase the demand of R_2, leading to a higher R_2'.

Application to Julius Baer and Signa

Information asymmetry: Signa was a highly complex and opaque conglomerate, making it difficult to assess the true risk. The asymmetry happens that Julius Baer knows less than the borrower about the credibility of the borrower. The demanding of collaterals and spending effort monitoring are both ways to reduce or protect against the asymmetry.
Monitoring costs: Effective relationship lending requires not only upfront screening (γ) but ongoing updates (m). Julius Baer's continued exposure indicates that it failed to respond to the information. The result aligns with the model: hidden expected losses emerged from relying on stale information (p) while avoiding spending m to acquire p'.
Collateral: The loans were secured by *illiquid shares*, not tangible and easy-to-sell assets. According to the model, such collateral lower the demand of R_2. But in fire-sell like in period 2 when borrower risk worsens, this kind of bad collateral could not provide proper recovery buffer. Without proper counting in the risk of collateral losing values, it jeopardizes the banking business by not earning as much returns when the loan is succesful and not able to recoup when the loan goes under.

1.3 Whether Julius Baer acted as a relationship bank or deviated into arm's-length finance practices

Julius Baer's lending relationship with Signa Group shows a mixed approach that didn't quite work out - it wasn't really doing either relationship banking or arm's-length finance properly. **Relationship banking** is about personal connections and soft information, while **arm's-length finance** is more about standard contracts with hard information and easy-to-sell collateral.

Some parts of what Julius Baer did looked like **relationship banking**. Signa's complicated structure needed special relationship knowledge, and that big CHF 144 million write-down shows they were in it for the long haul. When Signa started having problems, Julius Baer tried to work things out instead of just pulling the plug, which is typical relationship banking behavior.

But Julius Baer also had some **arm's-length finance** habits, especially with how they handled collateral. They used **illiquid share pledges** and didn't really check up on them regularly - they just assumed the initial deal was good enough. Because they weren't monitoring properly, they missed how the borrower's quality was getting worse (from p to p'). According to the RNND model, the bank should make extra returns (R_2) to make up for their initial costs (γ), but only if they're keeping an eye on things. Instead, Julius Baer kept lending based on old assumptions and collateral that was losing value.

To sum up, Julius Baer **looked like a relationship lender but didn't really act like one**. They spent money on initial screening and kept the relationship going, but they weren't monitoring or enforcing rules properly. This meant they didn't get the protection from being able to liquidate, and they didn't make the extra money from having special information - basically, they **didn't fully follow through on relationship banking**.

1.4 How this case illustrates the trade-offs between short-term client acquisition and long-term financial stability

This case highlights the core trade-off in relationship banking: banks may offer favorable terms upfront to attract clients, but doing so can endanger long-term financial stability if borrower risk evolves. In the two-period RNND model, banks incur a sunk cost γ and demands a discounted initial repayments (R_1). The cost is only recouped if the relationship survives into period two, where higher repayments R_2 restore the bank's margin:

$$pR_1 + p^2 R_2 = 1 + \gamma + p$$

However, if borrower risk deteriorates from $p \to p'$ and the bank does not monitor, it misprices R_2, and expected repayments fall short. Illiquid collateral worsens the situation—if it collapses in value, the loan becomes effectively unsecured.

Julius Baer's bridge loans to Signa demonstrate this trade-off. To win the client, Baer likely accepted a low R_1 or even discounted R_1: generous terms, minimal collateral enforcement, and a reliance on soft information. When Signa's risk worsened and collateral lost value, the bank was locked into the relationship and unable to recover its initial concessions. What began as a short-term acquisition strategy ultimately compromised long-term stability, as the repayment structure no longer covered the embedded risk.

1.5 Incentives for Individual Relationship Managers versus Centralized Risk Management

At a private bank like Julius Baer, **client relationship managers** are often rewarded based on how much new business they bring in, such as winning big clients or arranging large loans. To attract a major client like the Signa Group, they may offer **customized financing deals**, sometimes with fewer covenants, lower rates, or more flexible terms. These managers usually receive bonuses or recognition once a deal is signed, but there is often **no penalty later on** if the loan goes bad. This creates a kind of **"call option" incentive**, where they benefit from the upside but are not directly affected by the downside risks.

This structure can **conflict with the bank's central risk management approach**, which is supposed to:

- Check credit quality **independently of sales pressure**;
- Set strict standards for things like collateral and loan terms;
- Make sure that people who take risks are also responsible if those risks turn out badly.

In the Signa case, relationship managers at Julius Baer were focused on building the relationship and winning the client. They helped arrange **complex loans backed by illiquid equity** and participated in **related-party transactions** that were hard to evaluate and monitor. These kinds of structured products may have helped close the deal, but they **did not fit well into the bank's standard risk controls**.

The **CHF 144 million write-down** shows that the risks in these deals were not properly included in the bank's central oversight. While the managers got credit for bringing in Signa, the losses were absorbed by the bank as a whole. In the end, the strategy led to **regulatory criticism and investor concern**, and revealed a big problem: **the incentives of individual managers were not aligned with the bank's long-term risk management goals**.

To address this misalignment, banks need to implement **longer-term incentive structures** that consider loan performance over time. This could include: Deferred compensation that vests over the life of the loans; Clawback provisions for bonuses if loans go bad; Performance metrics that balance new business with portfolio quality.

Additionally, the central risk management function needs **stronger independence and authority** to review and approve complex transactions before they're finalized; monitor ongoing exposure to high-risk clients; enforce consistent standards across all relationship managers.

The Signa case demonstrates that without these safeguards, the pursuit of short-term gains by the client manager can lead to significant long-term losses for the bank. The bank's reputation and financial stability ultimately depend on maintaining proper risk controls, even when competing for prestigious clients.

2 DiD

2.1 Assumptions

The author implements a **difference-in-differences (DiD)** design comparing firms that had pledged floating liens before the reform (treated group) to those that had not (control group), before vs. after 2004. The DiD regression includes firm fixed effects and year fixed effects, so identification comes from a **differential change** in outcomes for treated firms relative to control firms after 2004, netting out any time-invariant firm traits and common time shocks. The estimated DiD coefficient β thus measures the *extra* impact of the law change on firms that were reliant on floating liens. This design rests on the crucial **parallel trends assumption** – that if the reform had not happened, treated and control firms would have followed similar outcome paths.

The authors address identification assumptions by noting the absence of other policy changes or macroeconomic shocks during 2000-2006 that could differentially affect treated and control firms. They find no concurrent legal reforms related to credit or collateral. They also assume no spillovers between groups, meaning the reform's impact on treated firms does not indirectly affect control firms. While not explicitly tested, this could be challenged by credit reallocation or market competition. Matching on industry helps control for common shocks but raises the possibility of within-industry spillovers that could bias the DiD estimates. The key identification assumption is parallel trends. The paper supports this by matching firms on observables and showing that pre-2004 trends in outcomes were nearly identical across treated and control groups. This suggests the two groups would have evolved similarly absent the reform. They further run robustness checks with group-specific time trends, finding that the treatment effect persists. While the parallel trends assumption remains untestable beyond the pre-period, the evidence presented lends strong support to its validity.

2.2 Treatment–Control Group

The treatment definition—firms using floating liens before 2004—raises concerns about selection bias. These firms differed systematically from controls: they were larger, more leveraged, and growing faster, suggesting endogenous treatment linked to firm characteristics. The authors attempt to mitigate this by matching on establishment year and detailed industry, improving comparability. However, even after matching, treated firms remain significantly different, which complicates causal inference. While firm fixed effects help control for time-invariant unobserved heterogeneity, time-varying unobservables remain a risk. The authors argue that floating lien use reflects persistent firm or lender "style," mostly absorbed by fixed effects.

2.3 Dynamic Effects

One potential issue is **dynamic effects** and timing. The authors effectively assume no **anticipation** of the reform prior to 2004 – i.e. firms did not adjust behavior in 2003 in expectation. This seems plausible (the paper does not report any pre-2004 policy announcement shocks), and the empirical test is that no pre-trend divergence is observed. After the reform, the DiD coefficient β captures the average impact over 2004–2006. The authors find a sharp, economically significant drop in collateral use by treated firms relative to controls post-2004, alongside reductions in debt financing and investment in the treated group. Notably, **no effect is seen prior to 2004** and the divergence occurs after the law change, consistent with a causal interpretation. If the effects got stronger or weaker over time, and the authors used a simple post-2004 dummy, this could miss the evolving pattern. But in this case, the authors see effects happening quickly, so they believe a simple average is good enough.

2.4 Robustness

The author conducts several robustness checks to address internal validity. Remaining concerns include possible spillovers (e.g., banks shifting credit to control firms) and sample attrition (e.g., if treated firms failed more). While spillovers can't be fully ruled out, controls didn't improve post-2004 — suggesting this kind of bias is limited.

2.5 Some additional thoughts

While the authors' DiD approach is convincing, a few alternative strategies could complement or validate their findings. A **propensity score matching plus DiD** could be used to ensure treated and control firms are balanced on a wider range of observables in the pre-period (the authors effectively do a version of this by matching on multiple characteristics in robustness checks). This would formally address observable selection bias and then apply DiD to control for unobservable time-invariant differences. In addition, **placebo tests** could be conducted by randomly selecting some years pre-2004 as the treatment period and see if the results are still significant.

In summary, the author applies the difference-in-differences methodology with a high degree of rigor. They clearly articulate identification assumptions and take steps to validate them (parallel pre-trends, absence of other shocks). The treatment and control groups, while inherently different, are handled through matching and fixed effects to mitigate bias. Key threats such as unobserved heterogeneity, non-parallel trends, and endogenous selection are recognized and probed via robustness tests. The results appear robust and causally interpretable. Minor concerns like potential spillovers or longer-term effects, or dynamic effects as previously discussed do not change the fact that it is a high quality paper.

3 Switching Discounts

Credit switching discounts - the interest rate reductions banks offer to poach another bank's borrowers - are a hallmark of markets with switching costs. Empirical studies consistently find that when firms switch lenders, they receive significantly lower loan rates than similar firms staying with their incumbent bank. These introductory discounts, followed by later rate hikes once the borrower is "locked in," reflect banks' strategies to overcome rivals' informational advantages and recover switching costs. However, pinning down the causal drivers of such discounts is challenging. The current literature employs a range of empirical methodologies - from panel data models and matching methods to natural experiments - to identify switching cost effects. Building on these, future research is poised to further refine identification techniques, exploit richer data, and apply novel econometric approaches.

3.1 Current Empirical Approaches and Identification Strategies

Barone, Felici, and Pagnini employ a dynamic mixed logit model to show that firms' loyalty to their main bank cannot be explained solely by time-invariant preferences – true state dependence exists, indicating significant switching costs. In parallel, panel regressions of loan pricing with firm or relationship fixed effects help control for unobserved borrower risk. Ioannidou and Ongena's seminal analysis of Bolivian loans tracks the same firms before and after switching banks, revealing an average initial rate cut of 89 bps when a firm changes its main lender. Crucially, they document a "loan rate cycle": the new bank lures the borrower with a low rate, then gradually raises the rate over the next 3-4 years until the borrower is "back to square one" at the original rate. This pattern is identified by exploiting within-firm variation over time, lending credibility to the result.

A complementary strategy is to compare switching firms to otherwise similar non-switchers at the same point in time. Propensity score matching or careful regression controls create a counterfactual benchmark for what interest rate a switching firm would have paid had it stayed. Indeed, studies often report switching discounts "compared to similar non-switching loans". For example, Bonfim, Nogueira, and Ongena (2020) note that their calculated 63 bps discount for voluntary switchers is obtained by contemporaneously comparing against comparable firms that did not switch. This matching-style approach strengthens identification by accounting for observable characteristics that influence loan pricing. Barone et al. likewise incorporate controls for selection bias - in their interest rate regressions, they adjust for the non-random likelihood of switching and include firm-level covariates or fixed effects.

More recent work pursues quasi-experimental designs to bolster causal identification. A prime example is the analysis of bank branch closures by Bonfim et al. (2020). When banks were forced to shut down local branches, some firms suddenly had to "transfer" their loan to a new bank without the luxury of shopping around. This scenario provides a natural experiment: the switching decision is largely exogenous to the firm's quality or preferences. Strikingly, the authors find no interest rate discount at all for these forced switchers – the new loans carried equivalent rates to the old, unlike in normal times where proactive switchers secure substantial discounts. By comparing loan terms after branch closures (treatment group) with those for voluntary switchers in unaffected areas or periods (control group), the study isolates the effect of switch timing and bargaining power. The absence of a discount in emergency transfers suggests that when a relationship ends abruptly, incumbent banks no longer need to outbid each other with low rates, and well-informed outside banks may be cautious in offering concessions. Moreover, Bonfim et al. observe that these hurriedly transferring firms had lower default rates than regular switchers, implying they were on average safer borrowers. In other words, the usual positive correlation between switching and getting a lower rate is not due to switchers being riskier – if anything, in this setting the switchers were "better" firms, yet they still received no discount.

Another methodological advance is the use of expanded datasets that measure informational channels. A paper exploits unique nationwide data linking every firm's deposit accounts across banks. This allows researchers to observe an outside bank's informational footing before a switch occurs. The findings show that if a firm maintained a deposit relationship with a non-lender bank, that bank can leverage the firm's cash-flow history to mitigate the usual "winner's curse" in lending. In effect, outside banks with prior transactional knowledge of the firm compete more aggressively, eroding the incumbent's information monopoly. Empirically, such studies might compare interest rates or acceptance rates for switching loans where the new lender had a pre-existing deposit tie versus those where it didn't. By including variables for deposit relationship length, depth, or scope, one can control for differing information asymmetry across switches. The Norwegian evidence exemplifies how new data dimensions enable refined tests of theory: it was the first to empirically demonstrate that deposit-taking can directly impact lending competition and pricing.

3.2 Future Directions and Impact of Evolving Methods

I think, future research can continue to refine empirical strategies to understand switching discounts more deeply. One direction is the increased use of quasi-experimental and design-based methods. Researchers may seek out other natural experiments to observe how interest rate differentials respond. Such studies could employ DiD or RDD to achieve clean identification of switching cost effects. By comparing outcomes before and after an exogenous change, scholars can better distinguish causation from correlation. More rigorous identification may either validate prior estimates or adjust them if earlier methods left bias. For instance, if selection effects were inflating the perceived discount, a well-crafted natural experiment might find a smaller true effect. So far, the evidence indicates that stronger identification often adds nuance rather than overturning results: the presence of discounts is robust, but their size and duration can depend on competition intensity, information structure, and borrower characteristics. As methodological tools sharpen, future papers will likely report more precise, context-dependent discount estimates with considering more heterogeneity.

Another promising avenue is exploiting more data. The integration of credit registers, payment systems data, and even unstructured data could allow researchers to observe previously unmeasurable facets of bank–firm interactions. With machine learning and big data, future studies might predict a firm's propensity to switch and use that to control for selection on unobservables, or to find better matches for non-switching comparators. While speculative techniques must be applied carefully, they offer the potential to reduce omitted-variable bias and better isolate the pure effect of switching.

Future research can also do more on mechanism explanation what causes credit switching. For instance, combining an experiment with detailed competition metrics could parse how much of the discount is due to information vs. market power. Likewise, tracking longer-run outcomes with panel data can reveal if certain borrowers systematically benefit or lose under different switching regimes.

4 Heteroskedastic Model

Forecast Errors in Analyst Earnings Predictions by Firm Characteristics

Forecasting errors—measured by Standardized Unexpected Earnings (SUE) or earnings surprises—often exhibit heteroskedasticity driven by firm-level factors such as size, industry complexity, or analyst coverage. The SUE is calculated as:

$$SUE_{i,t} = \frac{\text{Actual EPS}_{i,t} - \text{Analysts' estimate consensus EPS}_{i,t}}{\sigma_{\text{Analysts' estimate consensus EPS}_{i,t}}} \tag{2}$$

where $EPS_{i,t}$ is the actual earnings per share for firm i at time t and $\sigma_{i,t}$ is the standard deviation of earnings estimates if such estimates are posted by x analysts; Similarly,

$$EarningsSurprise_{i,t} = \frac{\text{Actual EPS}_{i,t} - \text{Analysts' estimate consensus EPS}_{i,t}}{|\text{Analysts' estimate consensus EPS}_{i,t}|} \tag{3}$$

I have run some statistical regression that suggests large firms have systematically higher and more predictable SUE, earnings surprise than smaller firms, indicating that the variance in forecast accuracy is not random and might have some systematic patterns that are conditioned on observable firm traits, which could be categorized into size, valuation, industry complexity, and analyst coverage.

Heteroskedastic modelling could uncover structured patterns in predictability across the cross-section of firms.

Heteroskedasticity in PEAD by Firm Characteristics

Post-Earnings Announcement Drift (PEAD) refers to the well-documented phenomenon where stock prices continue to drift in the direction of earnings surprises after the announcement, rather than adjusting immediately. While most studies focus on the average drift conditional on the sign or magnitude of the earnings surprise, far less attention has been paid to the cross-sectional variance in the drift, which may also be systematic.

I venture to suggest that the magnitude and variability of post-earnings returns differ across firm types. For example, there has been researches suggesting that smaller firms often exhibit more volatile drifts. But I think more can be done to tell what factors might have influence on the magnitude of PEAD that is not already subsumed by size, for example, maybe we could check earnings quality, recent price momentum and institutional ownership.

Let $CAR_{i,t+1:t+k}$ denote the cumulative abnormal return for firm i in the k-day window following earnings announcement at time t, and let $SUE_{i,t}$ be the standardized unexpected earnings. Then we can model the conditional expectation and conditional variance of the drift as:

$$CAR_{i,t+1:t+k} = \alpha + \beta \cdot SUE_{i,t} + \varepsilon_{i,t}, \quad \text{with } \varepsilon_{i,t} \sim N(0, \sigma^2_{i,t}) \tag{4}$$

and

$$\log(\sigma^2_{i,t}) = \gamma_0 + \gamma_1 \cdot \log(\text{MarketCap}_{i,t}) + \gamma_2 \cdot \text{Illiquidity}_{i,t} + \gamma_3 \cdot \text{Coverage}_{i,t} + \dots \tag{5}$$

This heteroskedastic structure allows us to test whether the predictability of PEAD itself varies systematically across firms.

Heteroskedasticity in Crypto Volatility Forecast Errors Under Public Sentiment and Price Anchors

In cryptocurrency markets, volatility forecasts are often inaccurate, especially for smaller or speculative tokens. Unlike traditional assets, crypto volatility is not only influenced by liquidity or fundamentals but is also highly sensitive to public sentiment, social media narratives, and even commentary by influential individuals such as Elon Musk or Donald Trump. Additionally, investor behavior appears to differ based on the notional price of the coin itself—tokens with very low price-per-unit levels often attract retail traders who exhibit more volatile trading patterns.

These features suggest that forecast errors in crypto volatility models may be heteroskedastic, with the variance of these errors conditioned on external sentiment variables and price-level effects.

Let the volatility forecast error for crypto asset i at time t be:

$$\text{Error}_{i,t} = \widehat{\text{Vol}}_{i,t} - \text{Realized Vol}_{i,t+k} \tag{6}$$

We can then model heteroskedasticity as:

$$\text{Error}_{i,t} = \mu + \varepsilon_{i,t}, \quad \varepsilon_{i,t} \sim N(0, \sigma_{i,t}^2) \tag{7}$$

$$\log(\sigma_{i,t}^2) = \gamma_0 + \gamma_1 \cdot \text{Sentiment}_t + \gamma_2 \cdot \log(\text{NotionalPrice}_{i,t}) + \gamma_3 \cdot \text{InfluencerShock}_t + \dots \tag{8}$$

Here, Sentiment_t can be derived from crypto Twitter indices, Reddit-based NLP scores, or Google Trends data. InfluencerShock_t is a binary or continuous variable measuring the timing or magnitude of posts or announcements by known market movers. $\text{NotionalPrice}_{i,t}$ reflects whether investor behavior differs for low-price vs. high-price coins, even after controlling for market cap. The implied Vol could be gotten from some crypto exchanges like Binance, or famously for derivative trading: Deribit.

Researches have been done on conditional heteroskedasticity model with GARCH and some other machine learning models. But forecast error variance as a function of sentiment or price level I beleive is not in the classic to-read literature.