Self Evaluation

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1 Research statement

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   *Journal of Financial Economics*


iii. Macropurulent bank capital regulation in a competitive financial system (2014)

iv. Regulating deferred incentive pay (2014)
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2. Dynamic relationships and international trade/finance:

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My research analyzes strategic interactions between firms, regulators, and governments, most recently within the financial sector. Both the methodological as well as the topical
aspects are important in linking the various pieces of my research together. In the topics dimension, my research interests have shifted from an international emphasis, with governments as central economic agents, to the role of financial regulation, in which financial institutions respond to government policies. Methodologically, I have extensively studied dynamic relationships between economic agents using (infinitely) repeated games. The presumption in this line of work is that in many relevant environments in economics and finance, economic behavior should not be characterized by purely short-term interests, i.e., “one-shot” optimal actions, but instead by a trade-off between these short-term interests and the long-run cost of such actions.

I have organized my work as follows. First, I will summarize my most recent applied work on financial regulation, including current working papers that I hope to be relevant for the current policy debate, as well as an empirical paper on corporate control. Second, I will present my work on dynamic relationships and international finance/trade.

**Note:** Since my papers cross various fields, a meaningful overall ranking is difficult. Within financial regulation, the first 2-3 papers are most representative of my research agenda. I am fond of all papers on dynamic relationships, revealing my technical “side.” Among my papers on “international trade,” I consider the one on “tariff wars” most representative.

### 1.2 Corporate finance

#### 1.2.1 Financial regulation

The recent financial crisis has (once more) illustrated the special role of the financial sector for the functioning of the overall economy. The interconnectedness of institutions within the financial sector (as exemplified by the aftermath of the Lehman Brothers collapse in September 2008) and the spill-over effects on the real economy have caused governments across the world to massively intervene in the form of explicit and implicit bailouts. While such interventions may have prevented an even larger scale of the crisis ex post, it is well understood that the expectation of such policies creates ex-ante distortions. Economists usually refer to this as a classic time-inconsistency problem (going back to Kydland and Prescott (1977) in the context of monetary policy). Financial institutions, in particular too-big-to-fail institutions, do not fully internalize the cost of their own failure and, hence, have an incentive to take on more risks than would be socially optimal. In fact, they may even have an incentive to coordinate their actions to induce a system-wide collapse (see Farhi and Tirole (2012)). In such environments, banks are able to “privatize gains and
socialize losses,” implying a major reason for regulating the financial sector in the first place.

One of the primary tasks of financial regulation is to mitigate such misalignment of private and social incentives. Within this broad field of financial regulation, I am particularly interested in policies targeting risk-taking incentives: capital regulation, the measurement of risk within financial regulation (credit ratings), as well as the regulation of compensation targeting risk-taking. My studies can be mostly classified as applied theory. The theoretical approach is motivated by the fact that most proposed regulatory changes cannot be evaluated empirically before they are implemented. Moreover, relevant outcomes, such as exposure to unlikely tail events, are oftentimes not observed until it is “too late.” This creates the need for models that capture relevant economic trade-offs.

My view on financial regulation  A common feature of all my studies is that I take existing rules or proposals as given and do not presume that observed policies coincide with the socially optimal mix (within the large set of feasible policies). If regulation already maximized social welfare, it could, by definition, never “backfire,” making academic research and advice on regulatory proposals somewhat less relevant. Three factors shape my view on regulation. First, in contrast to market participants, regulators cannot immediately adjust actions in light of new information. This “stickiness” might render even previously optimal regulation suboptimal. Second, political-economy forces (see, for instance, Stigler (1971)) might induce systematic biases between social and industry objectives, while “political activism” after large crises can lead to regulation that might be “too restrictive.” Third, well-intended and heuristically motivated regulation might have unintended side consequences, in particular if feedback effects (see Bond et al. (2010)) or interactions with other regulations are neglected. Academic research can therefore have a valuable role in helping to identify innocent mistakes or to expose regulatory capture to the public.

While my research does not take an a-priori stance on the effectiveness of a particular reform, it takes the view that financial institutions rationally respond to these reforms, i.e., according to their own incentives. While I do not assert that the scale of the financial crisis can be purely explained by “incentive problems,” I do proceed on the assumption that (the lack of right) incentives significantly contributed to it, potentially exacerbating

1 For example, the most fundamental changes in financial regulation, the Glass-Steagall Act and the Dodd-Frank Act, are a direct response to the great depression and recession, respectively.
the effect of misjudgments in the run-up to the crisis.

Papers on financial regulation My paper “Rating agencies in the face of financial regulation” joint with Christian Opp and Milton Harris (Journal of Financial Economics, 2013, see also shorter Fame version) building on Opp and Opp (2009) takes as given that a large set of regulatory rules in the financial sector are mechanically tied (hard-wired) to credit ratings. Starting with ratings-based investment restrictions for banks in 1936, regulators have increasingly outsourced regulatory authority to credit-rating agencies. In 1975, the Securities and Exchange Commission (SEC) formalized this regulatory accreditation by establishing a list of nationally recognized statistical rating organizations for the purpose of dealer-broker capital requirements. In short, ratings do not just convey information to investors about the creditworthiness of borrowers, but also provide regulatory certification to regulated investors, such as banks, mutual funds, money market funds, and insurance companies. If regulatory constraints of institutional investors bind, investors value the rating label in itself (see empirical evidence by Kisgen and Strahan (2010)), independent of the underlying information content that the label conveys. Following this logic, institutions should rationally care about “obviously” inflated ratings.

By identifying this regulatory channel, our main point is almost immediate: The quasi-regulatory authority of credit-rating agencies feeds back into their incentives to acquire and disclose information, which ultimately affects the informativeness of ratings (a version of the Lucas-critique). Intuitively, our model predicts that the supply of highly rated securities increases with the regulatory shadow value of ratings while the effect on informativeness is ambiguous. However, if the regulatory value of ratings is above an endogenous threshold, delegated information acquisition is no longer sustainable and rating agencies inflate all ratings. The threshold, above which the regulatory shadow value triggers rating inflation, is the level at which the rating agency is just indifferent between acquiring the optimal amount of information and disclosing the “correct” rating on the one hand, and, simply capturing the regulatory benefit while producing no information on the other hand. Intuitively, if the credit rating agency spends costly resources to acquire valuable information, there is no point in diluting that information ex post. This type of rating inflation is more likely for complex securities (which are more costly to evaluate) or se-

2 If, instead, the only concern about financial institutions was their susceptibility to “misjudgments,” regulatory tools targeting ex-ante incentives of institutions would be ineffective. Then, the primary task of regulation should be either to ensure sufficient capital that buffers the real effects of misjudgments, or to massively intervene in the business (lending) decisions of banks.

3 In an “investor-pays” environment with multiple investors, an information provider may prefer to sell a noisy version of his signal (see Admati and Pfleiderer (1986)).
curities that offer more risk-taking benefits. Our predictions are, thus, consistent with empirical studies that document rating inflation only for complex structured securities, but not for corporate bonds (see Griffin and Tang (2012), Benmelech and Dlugosz (2009), Effing and Hau (Forthcoming) vs. Baghai et al. (Forthcoming) for evidence on corporate bonds). Vis-à-vis policy proposals, our paper would predict more informative ratings if the Dodd-Frank Act mandate “to remove all references to ratings in federal regulation” was implemented.

The previous theoretical study directly relates to an empirical examination of a regulatory reform of capital requirements. In ‘Regulatory reform and risk-taking: replacing ratings’ (under review), Bo Becker and I study the first large-scale attempt by regulators to remove ratings as a basis for risk measurement in capital regulation. Officially motivated by the failures of credit-rating agencies during the financial crisis, the National Association of Insurance Commissioners (NAIC) started using “expected-loss” estimates by Pimco and BlackRock instead of traditional credit ratings to determine capital requirements for insurers’ asset holdings of private-label mortgage-backed securities (MBS) starting 2009/2010.

Our study reveals that the calibration of the system to the new input implies capital buffers that only provide protection up to “expected losses,” but not against higher-than-average losses. Thus, the new regulation effectively applies the law of large numbers to payoffs from structured securities. Since structured securities are highly correlated and hence coined “economic catastrophe bonds” (see Coval et al. (2009)), the new regulation provides (virtually) no protection against negative macro scenarios. As a result of this regulatory reform, industry-level capital requirements for mortgage-backed securities have been reduced from $19.36bn (had the previous system been maintained) to $3.73bn, a “massive” reduction in capital buffers. Insurers strongly respond to this new regulatory regime with increased risk taking within their MBS portfolios, but not in other asset classes that are unaffected by the change, such as other asset-backed securities and corporate bonds. The regulation does not only apply to the existing stock of securities held by insurers, but also to new issues, i.e., the flow: while insurers’ new asset purchases consisted historically of 90% investment-grade securities, this percentage has dropped to 46.1% for

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4 In contrast to the suggestive term “inflation,” rating inflation is, thus, mostly a cross-sectional rather than a time-series phenomenon.

5 Since ratings also intend to measure expected losses (Moody’s), one can understand the new inputs mostly in terms of “replacing raters” rather than “replacing ratings.” The new measures do not address one of the key concerns about using credit ratings in capital regulation, namely that ratings do not (directly) measure systematic risk, but instead also capture idiosyncratic risk (see Iannotta and Pennacchi (2012)). Since regulators ultimately care about events that cause (diversified) institutions to fail, systematic risk should be the key concern.
their MBS portfolios after the regulatory change, a fundamental change in risk-taking behavior. As the market for new issuance of commercial mortgage-backed securities (CMBS) is reviving, we expect this behavior to also significantly affect the overall riskiness of insurers’ portfolios, potentially sowing the seeds for the next financial crisis. We conclude that these potentially large long-run costs have to be weighed against the benefits of avoiding a fire sale (see, e.g., Shleifer and Vishny (1992)) on the existing stock of MBS held by insurance companies after the crisis.

In “Macroprudential bank capital regulation in a competitive financial system,” my co-authors (Milton Harris and Christian Opp) and I analyze how competitive public capital markets, an important alternative source of finance in the U.S. economy, influence the effectiveness of system-wide changes in bank capital requirements (see Admati et al. (2011) or Thakor (Forthcoming)). We argue that these alternative sources of finance can have important implications for the effectiveness of bank capital requirements, above and beyond the notion that, as potential substitutes, they can mitigate the negative fallout effects of reductions in bank lending.

Regulation via capital requirements is motivated by the fact that sufficiently levered banks (with access to insured debt financing) may find it privately profitable to provide loans for surplus-reducing projects as long as the project payoffs offer sufficient upside potential. In our general-equilibrium model, bank capital regulation thus primarily affects social surplus through the banking sector’s asset side, i.e., the scale and type of lending activities.

Going counter to the standard partial-equilibrium intuition that higher capital requirements lower banks’ risk taking due to greater skin in the game, our model reveals that the opposite may be true – over some ranges, increases in capital requirements can cause more banks to engage in value-destroying risk shifting, implying a non-monotonic relationship between capital requirements and risk taking by the average bank. The key insight for this non-monotonicity is simple: an increase in capital requirements affects not only per-unit returns on equity from different loans, but also the aggregate amount of bank lending (as long as raising additional equity capital is costly). When aggregate bank lending is

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6 A leading MBS analyst, Darrell Wheeler (Amherst Securities), claims that “the regulation changed the world” within the MBS space.

7 See, e.g., Becker and Ivashina (2014) for empirical evidence on firms’ substitution between bank and public debt in response to changes in the supply of bank credit.

8 While a bank’s leverage choice will indirectly affect social surplus creation through risk-taking incentives and, it does not directly create value, as for example in Hanson et al. (2014) and DeAngelo and Stulz (Forthcoming). We are currently working on including liquidity benefits of bank debt into our framework.
quantity-constrained by higher capital requirements, banks rationally shed their lowest-profitability borrowers first. However, the relative profitability of loans to borrowers of different types is generally not aligned with relative social surplus. Thus, as long as banks find it more profitable to provide loans to high-risk bad borrowers, an increase in capital requirements results in a decrease of financing of good projects funded by the banking sector. Paradoxically, increased efficiency of public markets can be the initial trigger of this deliberate adverse selection. As informed public market investors, who bear the full downside of lending to risky borrowers, can only compete with banks for borrowers that generate weakly positive surplus, they affect banks’ portfolio choice in an asymmetric – and socially adverse – way. Their financing offers only limit banks’ profits from lending to good borrowers, thus increasing the relative profitability of bad borrowers.

It is important to note that bank capital requirements are by no means a lost cause: for capital-ratio requirements above an endogenous threshold, the skin-in-the-game effect is always strong enough to ensure that funding borrowers with a socially negative NPV is privately suboptimal for banks. This threshold level generally depends on the relative funding efficiency of public markets and banks.

In our model increases in the level of equity in the banking sector are not per se welfare enhancing either. As long as banks’ marginal expected returns to risk-shifting are positive, an increase in bank equity can even be harmful, as it can allow for an increase in the scale of banks’ risk-shifting activities. In this case, even more substantial capital requirements are required to curtail banks’ risk-taking. Changes in equity levels and equity ratios naturally have distinct effects. Absent increases in equity ratio requirements, banks can just lever up against additional equity, so that only the size of their total balance sheet, the scale, changes while their leverage and riskiness is unaffected. Empirically, commercial banks tend to raise additional debt to maintain target leverage ratios in response to a positive shock on their assets (see Figure 3 in Adrian and Shin 2010).

Our analysis provides a rationale for “narrow-banking” policies that aim to restrict banking activities. The model shows that, holding capital requirements fixed, banks have comparatively large incentives to engage in inefficient risk taking when they have little or no social-efficiency advantage relative to investors in public markets (such as proprietary trading of publicly traded securities vs. relationship loans). As a result, capital requirements should depend not only on asset-risk characteristics but also on banks’ comparative advantages in the financial system.

The idea behind capital regulation is that it affects shareholder’s incentives and will, thus, ultimately feed back into how shareholders incentivize their managers, the actual
decision makers. In contrast, recent proposals mandating deferral of bonuses or clawback clauses directly target the structure of compensation contracts. Many reports have asserted that current compensation practices in banking induce short-term decisions.\footnote{See, for instance, the Squam Lake Working Group’s 2010 report on Financial Regulation or for a comprehensive list of proposals, the Financial Stability Board’s thematic review on compensation in their 2011 Peer Review Report.} Since the G20 in Pittsburgh endorsed the Financial Stability Board principles for sound compensation practices in September 2009, several policies have been adopted. In the European Union, a new directive adopted in 2010 includes strict rules for bank executives’ bonuses with long deferral and retention requirements (e.g., 5 years in the case of Austria).

The heuristic motivation for such regulation seems intuitive: the results of many risk-taking decisions are often only observable with considerable delay. By imposing a minimum period until a bonus truly “vests,” this regulation ensures that bonus payments are only made after more information about the quality of investments has come out. In ‘\textit{Regulating deferred incentive pay}’ (joint with Florian Hoffmann and Roman Inderst, reject and resubmit, \textit{Journal of Finance})\footnote{The editor invited a resubmission, with the caveat that there are substantial differences between the current draft and one that would be acceptable to both referees and the associate editor.}, we theoretically analyze the validity of this argument in a multi-task framework where bankers have to be incentivized both to generate business volume and to exercise diligence. While volume is immediately observable, diligence (quality) reduces the arrival rate of a subsequent stochastic bad event which is damaging to society (and only partially internalized by the bank). For simplicity, both volume and diligence are one-time decisions.

One robust insight of our model is related to the importance of a bank’s private incentives for volume. When these are high, a binding mandatory deferral of incentive pay will lead to higher equilibrium “diligence,” whereas such regulation backfires otherwise. The latter result highlights potential interactions with the proposals on capital regulation: if higher capital requirements are effective in increasing shareholders’ incentives for requiring quality, imposing additional mandatory deferral constraints is more likely to backfire.

1.2.2 Corporate control

Many existing theories suggest that the choice of the medium of exchange in takeovers is related to, broadly speaking, private information (see, for example, Rhodes-Kropf and Viswanathan (2004), Fishman (1989), Shleifer and Vishny (2003)). While it is generically difficult to test directly for private information, one route to potentially make progress in identifying the information content of merger bids is to carefully examine failed trans-
actions. Consistent with the prominent role of the medium of exchange in finance theories, in “Target Revaluation after failed takeover attempts – cash versus stock” (conditionally accepted at the Journal of Financial Economics), my co-authors (Ulrike Malmendier and Farzad Saidi) and I document that cash- and stock-financed takeover bids induce strikingly different revaluations among targets. In the sample of unsuccessful takeover bids between 1980 and 2008, targets of cash offers are revalued on average by +15% after deal failure, whereas stock targets return to their pre-announcement level. These results also hold in the subsample of deals that failed due to plausibly exogenous reasons.

We analyze the role of future takeover activities and subsequent changes in operating policies to explain these differences. The former analysis is motivated by the conclusion of the seminal paper by Bradley et al. (1983): “Acquisitions are attempts by bidding firms to exploit potential synergies, not simply superior information.” However, we neither find differences in the timing nor the value of future offers between cash and stock targets, ruling out anticipated future takeover activity as a driver for our (differential) results. Similarly, we cannot detect differential operational policies among cash and stock targets following failed bids. Based on this evidence, we conclude that the differential revaluations of cash and stock targets are most consistent with a pure information story, in which cash bids reveal a previous undervaluation of the target to the market. We reconcile our conclusion with the one of Bradley et al. (1983) by pointing out a significant sample-selection bias affecting their analysis.

1.3 Dynamic relationships / International trade

Dynamic relationships In my job market paper “Expropriation risk and technology” (Journal of Financial Economics, 2012), I aim to address a basic yet fundamental question. Motivated by Hugo Chavez’s expropriation of ConocoPhilips in Venezuela in 2007, I try to understand the economics and dynamics of expropriation risk. Given the lack of contractual enforcement possibilities when the deviating party is the government, such a study begs the question of how (and why) such investments are made in the first place.

Bradley et al. (1983) consider revaluations of targets in failed bids that are not taken over in the 5 years following the bid. Long-run returns in such a sample, however, capture the joint effect of the failed takeover attempt and the absence of a subsequent takeover attempt. As a result, the long-run returns (starting from pre-announcement of the failed bid) produce downward-biased estimates of the effect of the failed bid on the target’s stand-alone value. This is because the ex-ante market price incorporates the possibility that the firm would be taken over within the subsequent 5 years, but the forward-looking sample construction implies zero – i.e., abnormally low – takeover activity.

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Typically, we think about property rights varying at the country level, such as through the channel of the legal system (La Porta et al. (1998)) or the channel of institutions (Acemoglu et al. (2001)). In my dissertation, I adopt a more granular perspective, and argue that property rights within a country vary across industrial sectors, linked to the technological level of the sector. Intuitively, the higher the comparative advantage of a firm (sector) to manage an asset, the less it has to fear expropriation by a government.

To understand firm investment dynamics in environments without resort to a legal system, contracts have to be self-enforcing. That is, neither party should have an incentive to deviate from the contract at any point in time. I model an environment in which a firm – e.g., a large multinational oil firm – interacts repeatedly with an impatient government (think about Venezuela, Russia). Formally, impatience implies that the government discounts the future at a higher rate than the multinational firm, which is supposed to capture the notion that a government is making short-sighted decisions and/or potentially starving for cash. By allowing for discount-rate shocks (induced by, for example, the election/coup of a new government), it is possible that a government expropriates in equilibrium – which is not typically a feature of repeated games. Upon such a discount-rate shock, expropriation of sectors is, however, not “random,” but rather evolves according to a “rational” pecking order, in which the government selects sectors which are relatively easy to expropriate.

My study has empirical implications: there is a tendency that expropriation occurs in those sectors that seemingly got a “good deal” from previous governments (see, for example, Yukos in Russia). That is, the typical official government narrative is that firms are expropriated as a punishment for paying “too” little for the projects ex ante and very few taxes afterwards (often implied by generous tax holidays in the first years). In my model, the same phenomenon occurs as an equilibrium outcome, but the causality is reverse. In the presence of strong expropriation risk, the only way to entice firm investment is to require a very low upfront payment and to grant tax holidays. As long as the regime stays in place, firm profits are “extraordinarily” high to account for the possibility that the firm loses everything upon expropriation. In contrast, when expropriation risk is modest, large upfront payments, satisfying the immediate cash need of the government, are feasible. Interestingly, governments may not necessarily benefit from being weak: while government weakness makes it possible to achieve higher joint surplus (to be shared between the firm and the government), it limits the threat point of the government, i.e., the amount of surplus that the government can extract. Firms, in turn, can actively manage expropriation risk by optimally bundling activities in high- and low-expropriation risk sectors, consistent with the wide-spread phenomenon of conglomerates in emerging-market countries.
While “Expropriation risk and technology” features a dynamic contracting environment, its contribution lies mainly in the application. Building on my dissertation, the paper titled "Impatience vs. incentives" (joint with John Zhu, *Econometrica*) instead analyzes the general theoretical implications of dynamic contracting with an impatient agent (think about a firm repeatedly incentivizing a worker, or a parent using candy to incentivize a child to study). This paper has been included in the reading list for Ph.D. topics courses by Debraj Ray (NYU Economics), Tim Worrall (Edinburgh Economics) and Willie Fuchs (UC Berkeley Finance). There are two reasons why this purely theoretical exercise is relevant. First, the assumption of an impatient agent has become common in finance (see Demarzo and Duffie (1999), DeMarzo and Fishman (2007)) and economics (Acemoglu et al. (2008)). A rigorous understanding of its effect in a canonical economic model therefore seems important. Second, an impatient agent introduces an interesting theoretical tension for dynamic contracting: going back to the insights of Harris and Holmstrom (1982) and Becker and Stigler (1974), backloading rewards to an agent has been considered the robust optimal compensation scheme under equal discounting. Intuitively, rewarding an agent early on is bad from an incentives perspective, since rewards do not affect incentives as soon as they are paid out (think about giving candy to your child before studying). On the other hand, the impatience force makes it valuable to provide such early rewards.

How do optimal contracts resolve this tension between impatience (early rewards) and incentives (deferred rewards)? To answer this question, we consider a parsimonious dynamic-agency framework in the spirit of the general treatment by Ray (2002). Each period, a self-enforcing agreement stipulates a transfer and an action from a potentially rich action space (e.g., effort or investment). The key distinction between the principal and the agent is that the agent may renege on the current agreement. Agreements are sustainable if the agent never has an incentive to deviate and the principal’s participation constraint is always satisfied. While the formal model setup is rather abstract, we reveal its applicability using various examples from the relevant literature in finance and applied microeconomics (e.g., Albuquerque and Hopenhayn (2004)).

We show that adding an infinitesimal amount of relative impatience on the agent side will cause virtually all Pareto-optimal contracts to oscillate around a unique Pareto-optimal steady state. These contracts may feature oscillation of transfers with fixed actions, or may induce comovement of action distortions with transfers. Two features imply oscillation. First, impatience implies that Pareto-optimal contracts generally require the incentive-compatibility constraint (IC) of the agent to bind at all times. If IC was slack, one could exploit dynamic trading gains by increasing the agent’s rewards today while still
respecting IC. Second, a higher promised transfer today must (partially) relax the agent’s IC constraint. Taken together, an above-steady-state transfer must then be followed by a below-steady-state transfer (and vice versa), leading to oscillation.

How should one interpret these surprising results in standard economic environments? First, they highlight that the assumption of impatience is far more than purely an auxiliary assumption. It can have “strong” implications for contract design, above and beyond the notion that it is more difficult to sustain contracts with an impatient contracting party. Different discount rates create dynamic trading gains and, thus, introduce surplus from a borrowing-lending relationship on top of the surplus created by actions such as effort or investment. Second, on a more positive note, our framework highlights how one can build simple models that feature endogenous cycles via the interaction of impatience and incentives.

Based on my extensive work on dynamic games and my colleagues’ expertise in asset-pricing models, Christine Parlour, Johan Walden, and I teamed up with the goal of formally developing “industrial-organization implications” for asset pricing. In particular, we were interested in the question of how collusive behavior (markups) at the industry level will affect firm-level cash flows, the sensitivity of cash flows to aggregate shocks (riskiness) and, hence, ultimately expected returns in asset markets. However, in the process of developing such a framework for what one could call “Industrial Asset Pricing,” we obtained a set of results that highlighted, instead, the “asset-pricing implications” for collusion (rather than vice versa). Moreover, by embedding industry-level strategic behavior into a standard general-equilibrium framework with a continuum of goods (see my work in international trade discussed below), our model produced interesting aggregate implications originating from strategic behavior at the industry level. Since these results were interesting in their own right, we separated the project into two parts, i.e., “Markup cycles, dynamic misallocation, and amplification” (Journal of Economic Theory, 2014) and “Industrial Asset Pricing” (work in progress).

The published paper has two main results. First, counter to the conventional wisdom based on the oligopolistic model of Rotemberg and Saloner (1986) as well as Rotemberg and Woodford (1992), industry markups may be procyclical with regard to aggregate shocks, consistent with recent empirical evidence by Nekarda and Ramey (2013). Intuitively, while high product demand in good times increases firms’ incentives to undercut competitors to reap immediate rewards (the conventional force of oligopolistic competition), the marginal utility of an additional dollar reaped in good times is also low, making it less valuable to deviate, which is the novel asset-pricing force we highlight. With risk-averse preferences, the
latter force can dominate the “demand” effect and thereby lead to procyclical markups.\textsuperscript{12} This set of results has important implications for the design of monetary policy since the cyclicality of markups is a key building block of leading Neo-Keynesian macroeconomic models (see, among others, \textit{Goodfriend and King (1998)} or \textit{Christiano et al. (2005)}).\

Second, if industries are heterogeneous in terms of their competitiveness or their exposure to (exogenous) aggregate shocks, intra-industry behavior can amplify shocks, or even produce endogenous fluctuations, in the absence of aggregate shocks. These aggregate implications arise from the fact that heterogeneous industries will set different markups state-by-state. This markup dispersion across industries leads to resource misallocation (see \textit{Lerner (1934)}) and, hence, affects aggregate consumption. Following standard asset-pricing insights, changes in aggregate consumption affect agents’ marginal utilities across states and thereby the valuation of firms’ future cash flows; this, in turn, feeds back into the firms’ ability to sustain collusion, leading to a rich set of implications.

\textbf{International trade}  While I was officially enrolled as a graduate student in finance at the University of Chicago, I have always been also interested in broader subjects, in particular international trade and finance (see also my job market paper). In \textit{Tariff wars in a Ricardian model with a continuum of goods} (\textit{Journal of International Economics}, 2010), I model the strategic interaction of countries in setting welfare-maximizing import tariffs\textsuperscript{13} The issue of optimal protectionism is one of the classical and yet still relevant economic questions, as the ongoing negotiations about a free-trade agreement between the United States and the European Union suggest. My contribution to the literature is to analyze the implications of technology differences between two countries (absolute productivity advantage and comparative advantage) for optimal tariff rates and the resulting Nash equilibrium of tariffs. My general-equilibrium analysis takes place in a Ricardian model with a continuum of goods in the spirit of \textit{Dornbusch et al. (1977)}. Within this canonical framework, I show that the optimal import tariff rate is uniform across goods. Tariffs are an increasing function of productivity-adjusted relative size, increasing in the potential gains from trade (comparative advantage), and decreasing in transportation cost. Intuitively, the larger the potential gains from trade, (the smaller the barriers to trade,) the less harmful a given tariff will be. The size of the non-traded sector is thus a function of both exogenous and endogenous barriers to trade. If a country is sufficiently large, it

\textsuperscript{12} Bagwell and Staiger (1997) provide an alternative explanation for procyclical markups based on the growth rate of cash flows (for details, see literature discussion in paper).

\textsuperscript{13} While political considerations are certainly important, Bagwell and Staiger (1999) show that political factors do not imply a separate role for the traditional economic “terms-of-trade” driven incentive to impose tariffs.
will prefer the globally inefficient Nash equilibrium of tariffs over free trade. This required threshold size is increasing in comparative advantage and decreasing in transportation cost. The static Nash-equilibrium analysis of my paper can be used as a stepping stone to analyze the sustainability of trade agreements in a dynamic setting.

In “Rybczynski’s theorem in the Heckscher-Ohlin world – anything goes” (joint with Hugo Sonnenschein and Christis Tombazos, *Journal of International Economics*, 2009), we revisit one of the fundamental theorems of international trade, the Rybczynski theorem, in general equilibrium. Our setup features a classical Heckscher-Ohlin environment, i.e., 2 countries, 2 production factors (capital and labor), and homothetic preferences. The Rybczynski theorem (see Rybczynski (1955)) makes predictions about how changes in factor endowments, e.g., changes in the labor force in China, would affect its production. In its basic form, the Rybczynski theorem states that for each price \( p \) an increase in labor supply leads to an increase in the equilibrium supply of the labor-intensive good and a decrease in the supply of the capital-intensive good. The theorem has received wide attention in trade theory in the context of a home economy that is small relative to the rest of the world, so that \( p \) is determined by the rest of the world. Thus, the key to understanding its robustness in general equilibrium is to depart from the assumption of a “small” home economy, so that world market prices adjust. Our main contribution is to show that price effects can indeed be so strong that the comparative-statics predictions of the Rybczynski theorem are reversed in sign. If such an outcome obtains, factor growth must harm the welfare of the home economy.

1.4 Impact

Impact can be illustrated along various dimensions, which I will now try to address.

1.4.1 Visibility

Over the course of my career of 6 years (since 2008), I have presented my research papers 72 times at invited seminars and academic conferences (113 times including presentations by co-authors, see breakdown by paper at the end of Section 1.4.2). The division of presentations across authors is also a good indicator of my contribution to the various projects. Among invited seminars, I have presented at virtually all distinguished institutions (some of them multiple times): Columbia, Duke (twice), London School of Economics, MIT (twice), NYU (twice), Harvard, Princeton, Stanford (4 times), UCLA, University of Chicago (twice), University of Pennsylvania (Wharton and Economics). Similarly, I have
presented my research at major conferences, such as the *NBER Summer Institute Corporate Finance* (2011, 2014), the *NBER Summer Institute Credit Ratings* (2011, 2013, 2014), the *European Finance Association meeting* (2010, 2012), the *Western Finance Association meeting* (2012, 2014), the *American Economic Association meeting* (2012, 2013) and the *Econometric Society meeting* (2011, 2015 (scheduled)).

It is particularly worth highlighting that my papers were represented on every program of the *NBER Summer Institute Credit Ratings* ever since the group was institutionalized in 2011 (organized by Chester Spatt). This line of research has also gained much interest among regulators with presentations by myself at the Federal Reserve Bank of San Francisco and the Bundesbank, as well by my co-authors at the Federal Reserve Bank of New York, Federal Reserve Board of Governors, the European Central Bank, and the Bank of Spain. In November 2013, I was asked to lead a workshop for European regulators on “replacing ratings in capital regulation,” one of the key issues regulators across the world are struggling with. This broad and strong interest is one reason why I expect my work on this topic, in particular “[Regulatory reform and risk-taking: replacing ratings”](#) (with Bo Becker), to gain more traction in the future.

In addition to presenting my own research, I have been selected to discuss research papers of colleagues at major academic conferences such as the NBER Corporate Finance meetings, the AFA, the WFA, and the EFA. The discussed papers (see below) mainly fall in the field of financial intermediation, and, in particular credit ratings. For example, at the NBER Corporate Finance meeting in the Fall of 2012, I had the task of discussing two papers on credit ratings (one empirical and one theoretical paper).

1. AFA annual meeting 2015: “Did Government Regulations Lower Credit Rating Quality?” (Patrick Behr, Darren Kisgen, and Jérôme Taillard)

2. NBER Corporate Finance meeting Fall 2013: “Financing as a Supply Chain: The Capital Structure of Banks and Borrowers” (Will Gornall and Ilya Streubulaev)

3. WFA 2013: “Credit Ratings and Security Design” (Jens Josephson and Joel Shapiro)

4. NBER Corporate Finance meeting Fall 2012: “Who Should Pay for Credit Ratings and How?” (Anil Kashyap and Natalia Kovrijnykh)

5. NBER Corporate Finance meeting Fall 2012: “Do Security Analysts Discipline Credit Rating Agencies?” (Kingsley Fong, Harrison Hong, Marcin Kacperczyk, and Jeff Kubik)
6. WFA 2012: “Precision of Ratings” (Anastasia Kartasheva and Bilge Yilmaz)


While my work on the timely topic of “Financial regulation” has received most of the attention, I also want to note that my other papers are well received in the relevant (but smaller) fields. In particular, the paper “Impatience vs. incentives” is already featured on the readings list of Ph.D. topics courses taught by experts on dynamic contracting, i.e., Debraj Ray (NYU), Tim Worrall (University of Edinburgh), and Willie Fuchs (UC Berkeley).


1.4.2 Statistics

The stock of my citation count (258 Google cites, 32 Web of Science cites, and 15 cites in top journals (top-3 finance, top-5 general interest economics, Journal of International Economics)) is in line with “finance” theorists in my cohort at top institutions. In addition to the common delay in citation counts for theory papers, there is an idiosyncratic reason why I believe my citation count will grow in the near future. I switched fields (from “International” to “Financial regulation”) in 2010/11, which affects the current stock of citations. Despite this recent switch, the paper “Rating agencies in the face of

financial regulation,” published only in 2013, has already received 144 Google cites, 19 Web of Science cites and is cited in 12 articles forthcoming or published in top journals.


1. **Rating agencies in the face of financial regulation** (joint with Christian Opp and Milton Harris)

   (a) **Main insight:** The regulatory use of ratings feeds back into the ratings of profit-maximizing credit rating agencies. Rating inflation is expected to occur for complex securities.

   (b) **Publication:** Journal of Financial Economics (Top-3 finance journal)

   (c) **Citations** (Google, Web of Science, Top): 144, 19, 12

   (d) **Seminar and Conference Presentations:** 23


2. **Regulatory reform and risk-taking: replacing ratings** (joint with Bo Becker)

   (a) **Main insight:** We expose a reform of capital regulation for insurance companies in 2009/2010 which eliminated (to a first-order approximation) capital requirements for holdings of mortgage backed securities. Insurance companies strongly increased their risk-taking behavior as a response to this regulatory change.
3. **Macroprudential bank capital regulation in a competitive financial system**
   (joint with Milton Harris and Christian Opp)

   (a) **Main insight:** If banks face fierce competition from public markets, a small increase in capital requirements can cause an increase in the riskiness of the banking sector. Sufficiently large capital requirements can eliminate risk-taking by banks, but potentially lead to underfunding of socially valuable projects.

   (b) **Publication Status:** under revision

   (c) **Citations** (Google, Web of Science, Top): 5, n/a, 0

   (d) **Seminar and Conference presentations:** 15


4. **Regulating deferred incentive pay** (with Florian Hoffmann and Roman Inderst)

   (a) **Main insight:** Regulatory restrictions on compensation design such as mandatory deferral of bonus payments can lead to increased risk in the financial sector. Such “backfiring” is particularly likely if capital requirements are high.
5. **Target revaluation after failed takeover attempts – cash versus stock** (with Ulrike Malmendier and Farzad Saidi)

(a) **Main insight:** In contrast to a stock offer as a means of payment by the bidder, capital markets interpret a cash offer as a positive signal about the true value of target resources. We expose a significant bias affecting the previous literature on this topic.

(b) **Publication Status:** conditionally accepted at Journal of Financial Economics (Top-3 finance journal)

(c) **Citations** (Google, Web of Science, Top): 16, n/a, 0

(d) **Seminar and Conference presentations:** 15


6. **Expropriation risk and technology** (sole-authored)

(a) **Main insight:** Property rights (within a country) vary across industrial sectors according to their technology intensity, leading to a pecking order of expropriation. Firms can manage expropriation risk by forming conglomerates, i.e., bundling activities across sectors with different technology levels.

(b) **Publication:** Journal of Financial Economics, 2012 (Top-3 finance journal)

(c) **Citations** (Google, Web of Science, Top): 28, 5, 3
7. **Impatience vs. incentives** (with John Zhu)

(a) **Main insight:** We study the long-run dynamics of contracts in repeated principal-agent relationships with an impatient agent. Despite the absence of exogenous uncertainty, Pareto-optimal dynamic contracts generically oscillate between favoring the principal and favoring the agent.

(b) **Publication Status:** Econometrica, forthcoming (Top-5 general interest journal)

(c) **Citations** (Google, Web of Science, Top): 3, n/a, 0

(d) **Seminar and Conference Presentations:** 6


ii. By co-authors: n/a
8. **Markup cycles, dynamic misallocation, and amplification** (joint with Christine Parlour and Johan Walden)

(a) **Main insight:** Oligopolistic competition produces procyclical aggregate markups if valuations are governed by (sufficiently) risk-averse preferences. If industries are heterogeneous in terms of shock exposures or competitiveness, aggregate fluctuations originate purely from strategic behavior at the industry-level.

(b) **Publication:** Journal of Economic Theory, 2014 (Top journal for “economic theory”)

(c) **Citations** (Google, Web of Science, Top): 11, 0, 0

(d) **Seminar and Conference Presentations:** 11

9. **Tariff wars in a Ricardian model with a continuum of goods** (sole-authored)

(a) **Main insight:** I characterize optimum import tariffs in the Dornbusch-Fischer Samuelson model. Both higher absolute as well as higher comparative advantage increase equilibrium tariff rates. A sufficiently large economy prefers the Nash equilibrium of tariffs over free trade.

(b) **Publication:** Journal of International Economics, 2010 (Top journal for “international economics”)

(c) **Citations** (Google, Web of Science, Top): 18, 2, 0

(d) **Seminar and Conference Presentations:** [1] University of Chicago (Economics)

10. **Rybczynski’s theorem in the Heckscher-Ohlin world - anything goes** (with Hugo Sonnenschein and Christis Tombazos)

(a) **Main insight:** The predictions of the Rybczynski Theorem can be reversed in general equilibrium. This “reverse” outcome implies immiserizing factor growth.

(b) **Publication:** Journal of International Economics, 2009 (Top journal for “international economics”)
2 Teaching & mentoring

There are two things that I enjoy about teaching. First, I enjoy the in-classroom experience, which is generally shared by my students (see evaluations below). Second, I enjoy communicating the subject of finance and making students aware of pervasive misunderstandings of how financial markets are supposed to work. One of the key insights that I try to communicate in my course is that stock market prices are forward-looking, or why “good companies” are not necessarily “good investments.” I tend to start my course with a “true story:”

While cycling with a friend of mine, a biology postdoc at Stanford, I was asked whether Tesla stock was a “good investment.” Instead of immediately giving my opinion, I responded “Why do you think it should be a good investment?” He argued that he just read an article about the enormous growth prospects of ecologically-friendly cars, that oil resources would eventually be depleted, and that he knew of a lot of people who wanted to purchase a Tesla. In short, the company should be a strong buy! He looked at me with a convincing facial expression of a car salesman, but I did not seem all too impressed, and continued my questioning by asking him whether he was sure he did not have any better information. Somewhat annoyed by my lack of enthusiasm, he proceeded “Aren’t these enough reasons to buy Tesla?” I smiled and started to enter my teaching mode: “All the reasons that you just gave me are indeed reasons why Tesla is a great company, but they are also reasons why everyone else is willing to pay a high price for the stock, making it not necessarily a good investment. Why would anyone sell this stock to you for a price that is “too” low?” He paused, reflected on my comment, and then continued: “Finance is much more complicated than I thought… In contrast to our lab experiments (cancer research!), stock market prices already anticipate future outcomes.” I could not have said it in a better way. I felt an incredible amount of satisfaction teaching him, a distinguished researcher at our rival institution, this fundamental principle of my finance class, in such a short period of time! After 5 minutes of silence, he continued: “Marcus, do you think I should invest in the Chinese stock market? China’s economy is growing so much faster than that of the United States.”
This is usually the point when half of the class starts laughing, leaving me 10 weeks to explain this fundamental principle to the remaining half.

**Undergraduate teaching:** Most of my teaching since arriving at Haas in 2008 has been at the undergraduate level, where I have been teaching UGBA 103 “Introduction to Finance.” I tend to receive very encouraging feedback via emails from many of my students even years after I taught them (see also a representative sample online). Since some of this direct feedback comes from students expecting a recommendation letter at some point, I am particular fond of feedback from students communicated via third parties, such as David Robinson, lecturer at Haas.

My teaching evaluations in the category “instructor effectiveness” demonstrate teaching excellence, i.e., medians of 6 in every single section that I taught after 2009, but I had hoped for even better scores. To understand this discrepancy, I have assembled a list of all student evaluations in the category “instructor effectiveness” for UGBA 103 since the Fall of 2006 for all sections taught by ladder faculty, a total of 23 sections. Comparing myself against other teachers of the same course allows me to control for various features of the course that an instructor should not be held responsible for, i.e., the large section size (up to 300 students), the inconvenient Andersen auditorium classroom (with ample cheating opportunities in exams), and the diversity of student backgrounds in terms of quantitative abilities.

The results suggest that the course is indeed difficult for an instructor: Despite the fact that multiple sections were taught by the most successful Finance teachers at Haas (winning various Cheit teaching awards), the average “instructor effectiveness” score over 23 sections is just 4.98. Even in my first two years of teaching after graduate school (Fall 2008, and Fall 2009), I thus achieved relatively solid means of 5.04 and 5.05 (and medians of 5). In my final two years of undergraduate teaching (Fall 2010, and 2 sections in Spring 2012) I did, relatively speaking, very well with scores ranging between 5.56 and 5.84 (medians of 6), making these sections rank #2, #3, and #4 over all 23 sections.

I truly enjoy teaching undergraduates, as I have been blessed with remarkable students.

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15 Dear Prof. Opp, because I teach the Intro course, all Haas Ugrads have had me as their teacher at one time or another, so I get to know a lot of students. Idle chatter often leads to comments on how the current Haas Ugrads are liking their required courses. Of course, this is sometimes whines and complaints. However, I'm compelled to write and say that more than half-a-dozen students have told me how much they are enjoying Finance this semester, how much they are learning in a few weeks and above all, how well organized your approach to teaching is. It just seems that things are going very well. In admiration, Dave Robinson.

16 Only one instructor (Richard Stanton) achieved a mean above 6 dating back to the Fall of 2006.
Table 1. Ladder faculty ratings in the category “Instructor Effectiveness” for “Introduction to Finance” UGBA 103 in all 23 sections taught since Fall 2006. Sections taught by me are highlighted in bold. The mean score for instructor effectiveness is 4.98 (across all 23 sections).

that I am still in contact with, such as Chris Hammond (now LLM student at Berkeley). I have also been able to place two of my students successfully into world-class Ph.D. programs. Jae Hyen Chung got admitted into the University of Chicago, Econometrics program in 2013 and Yang Liu got admitted into the University of Pennsylvania program, Economics program in 2011.

MBA teaching: One of the suggestions in the mid-career review was to demonstrate teaching excellence in the self-supporting degree programs. In the spring semester of 2014, I taught MBA students for the first time. Despite noticing some drastic differences between teaching MBAs and undergraduates, my teaching performance was solid with medians of 6 in both sections and means of 5.2 and 5.63. Based on the learning experience from teaching MBA students for the first time, I am cautiously confident I will be able to improve my
scores in the coming years.

**Ph.D. teaching:** In spring 2011, I co-taught an interesting Ph.D. topics course together with William Fuchs in which we analyzed the “Economics of crisis.” We built this course from scratch (see syllabus Ph.D.297T) and tried to give students an overview of classical and recent papers on banking crises, currency crises, debt crises, and liquidity crises. The course went well in many respects. The students seemed very happy with the content and the instructors (my median: 6, mean: 6.1), I learned a lot from digesting and teaching this material, and, I became acquainted with a student from the math department, John Zhu (now assistant professor of Finance at Wharton), a subsequent co-author on the paper “Impatience vs. Incentives.”

Going forward, I would like to be exposed more to Ph.D. teaching. In fact, I will teach the introductory Ph.D. class “Corporate finance theory” in the spring semester of 2015. I am already excited to build a new course to ensure that more Ph.D. students from Haas and the Economics department are fascinated by corporate finance theory. I hope that teaching this course will also help me to become a more natural “sparring partner” and regular advisor for Ph.D. students. As of now, I have only advised two graduate students from the Economics department as an outside committee member, Mu-Jeung Yang (Ph.D., 2012, placement: University of Washington, Seattle), and Yury Yatsynovich (ongoing, also qualifying exam outside member). On the finance side, I collaborated with Javed Ahmed (now at Federal Reserve, Board of Governors) on an earlier version of the paper “Target Revaluation after Failed Takeover Attempts – Cash versus Stock” before I successfully teamed up with Ulrike Malmendier and Farzad Saidi (Ph.D. student at NYU at the time). I hope to be more productive as a Ph.D. advisor and co-author with Ph.D. students in the future.

**References on financial regulation**


Baghai, R., H. Servaes, and A. Tamayo, Forthcoming, Have rating agencies become more conservative?: Implications for capital structure and debt pricing, *Journal of Finance*.


References on corporate control


References on dynamic relationships


**References on international trade**

