

Cyclical implications of the Basel II capital standards

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Introduction and summary

One of the central changes proposed as part of the new Basel II regulatory framework is the concept of internal-rating-based (IRB) capital requirements.¹ Under the IRB approach, the amount of capital that a bank will have to hold against a given exposure will be a function of the estimated credit risk of that exposure. Estimated credit risk in turn is taken to be a predetermined function of four parameters: probability of default (PD), loss given default (LGD), exposure at default (EAD), and maturity (M). Banks operating under the “Advanced” variant of the IRB approach will be responsible for providing all four of these parameters themselves, based on their own internal models. Banks operating under the “Foundation” variant of the IRB approach will be responsible only for providing the PD parameter, with the other three parameters to be set externally by the Basel committee.²

It is clear that there are many potential benefits to further refining the existing risk-based capital requirements. As compared with the “one-size-fits-all” approach embodied in the original Basel I framework, IRB capital requirements should reduce pricing distortions across loan categories, as well as the accompanying incentives for banks to engage in various forms of regulatory capital arbitrage. At the same time, this new approach to capital regulation raises some concerns. One concern that has been voiced repeatedly—but has been subject to relatively little formal analysis—is that the new capital standards will exacerbate business cycle fluctuations. In brief, the idea is that in a downturn, when a bank’s capital base is likely being eroded by loan losses, its existing (non-defaulted) borrowers will be downgraded by the relevant credit-risk models, forcing the bank to hold *more* capital against its current loan portfolio. To the extent that it is difficult or costly for the bank to raise fresh external capital in bad times,

it will be forced to cut back on its lending activity, thereby contributing to a worsening of the initial downturn.

Our aim in this article is to take a closer look at this “cyclicality” aspect of the Basel II capital regulations. There are two primary components to our analysis. First, we start by developing a conceptual framework, which can be used to ask questions about the optimality of the proposed regulations. Our main conclusion here is that the Basel II approach of having a single time-invariant “risk curve”—that maps credit-risk measures (such as the PD) into capital charges—is, in general, suboptimal. From the perspective of a social planner who cares not just about bank defaults per se, but also about the efficiency of bank lending, it is more desirable to have a family of risk curves, with the capital charge for any given degree of credit-risk exposure being reduced when economy-wide bank capital is scarce relative to lending opportunities (as in, for example, a recession).³

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Of course, this is only a theoretical argument, and it leaves unanswered one key empirical question: How big might the costs associated with the imperfect Basel II approach plausibly be? Although this question is hard to answer fully, we attempt to make some progress on it in the second part of our analysis. We do so by simulating the degree of capital-charge cyclicality that would have taken place over the four-year interval 1998–2002 had the Basel II regulations been in force during this period.

Although several other recent papers have undertaken similar exercises, we make an effort to be relatively comprehensive, along several dimensions.⁴ First, recognizing that banks may use different types of credit-risk models to arrive at parameters such as the PD, we do all of our simulations with two distinct categories of models: 1) a model based on Standard and Poor’s (S&P) credit ratings; and 2) a model developed by the consulting firm KMV, which is based on a Merton (1974) option-pricing approach to estimating default probabilities.

Second, in light of the fact that different banks have very different loan portfolios, we check to see how our conclusions vary by region (for example, North America versus Europe) and by borrower risk type (for example, investment-grade versus non-investment-grade). Finally, across all of these simulations, we try to pay careful attention to a host of subtle methodological issues. The results can be quite sensitive to survivorship bias, as well as to how one treats firms that disappear from the datasets. We provide a detailed account of these issues and do our best to address them in a sensible and consistent fashion.

We should emphasize that our goal with these simulations is not so much to make a definitive case for a single “best” estimate of the degree of cyclicality in capital charges. Rather, we want to establish a plausible range of values and to explore in some detail how capital-charge cyclicality can vary both with the methodology used by the bank in question and with the composition of the bank’s portfolio. In some cases, our estimates imply a relatively large degree of cyclical variation. For example, applying the KMV model to samples of investment-grade borrowers yields increases in capital charges in the range of 70 percent to 90 percent over the period 1998–2002. In other cases, the magnitudes are more moderate, although they still appear to be economically significant. For example, the S&P-based simulations show capital-charge increases on the order of 30 percent to 45 percent over the same period.

Of course, even if one concludes that the single risk curve will exacerbate cyclical fluctuations, there is then the important question of what might be done

instead. We realize that there are a host of difficult implementation issues that would be involved in allowing the risk curve to vary. We discuss these issues in our concluding comments.

In the next section, we develop our conceptual framework. Then, we describe the results of the simulations. In the following section, we briefly survey the related literature in this area, making a particular effort to reconcile our empirical results with those reported in other work. Finally, we offer some suggestions for further study.

Conceptual framework

What are the goals of capital regulation?

In order to come to a view about the desirability of the proposed features of the Basel II accord, one first needs to have a clear understanding of the underlying economic goals of bank capital regulation. As with any form of regulation, the case for regulating bank capital presumably rests on some sort of market failure, or externality. In this particular case, the externality is that bank failures have systemic costs that are not fully borne by the bank in question. These systemic costs include losses absorbed by government deposit insurance, disruptions to other players in the financial system, and so forth. Thus, the regulator’s task is to somehow get the bank to internalize these systemic costs. In principle, there are a number of different ways to do so, and capital regulation can be thought of as one such method.

Of course, when one says that a goal of regulation is to get banks to internalize systemic costs associated with default, *this is not the same as saying that these default costs are the only thing that a well-intentioned social planner should care about.* Rather, the social planner should also continue to put weight on those objectives of banks that were properly internalized in the first place, for example, making positive net present value (NPV) loans; this is the sense in which we earlier described the planner as caring about the “efficiency” of bank lending. This implies a tradeoff. On the one hand, one can always reduce expected default costs by raising the capital requirement. On the other hand, if it is expensive for banks to raise and/or hold additional capital, a too-stringent capital requirement will lead to a reduction in bank lending, with the associated underinvestment on the part of those borrowers who are dependent on bank credit. The proper goal of regulation, therefore, is to balance two competing objectives: 1) protecting the system against costs of bank defaults versus 2) encouraging the creation of positive-NPV loans.

At one level, the existence of this tradeoff is obvious and well understood—the fact that capital charges are not 100 percent for all loans is testament to the fact that regulators care about something other than simply driving the probability of bank default to zero. At the same time, there is another implication of the tradeoff that generally receives less attention: When banks' lending activities are more severely capital constrained (that is, when underinvestment problems are worse), it is socially desirable to accept a higher probability of bank default, all else equal. In other words, when times are worse, there needs to be some adjustment on *both* the default-cost and lending margins. It cannot make sense for bank lending to bear the entire brunt of the adjustment, while the expected costs of bank defaults remain constant.

In contrast to this tradeoff type of logic, most discussions of bank capital regulation start from the premise that the goal is to hold the probability of bank default *below some fixed target level*. For example, it is common to speak in terms of, say, a 99.90 percent confidence level, which means that the bank has enough capital such that there is only a 0.10 percent probability of default over the next year. Once this target confidence level is set, one can use information on the nature of the bank's portfolio—along with various other assumptions—to figure out how much capital it will take to achieve the target. Gordy (2003) is a well-known paper in this vein. Indeed, Gordy is able to show that, under certain special circumstances, an approach to risk-based capital charges very much like that set out in the Basel II accord is the most efficient way to achieve the target confidence level. This Basel II approach can be summarized in terms of a single "risk curve," which relates the capital charge for any given loan to the risk attributes of that loan, such as its probability of default (PD).

The problem with targeting a single once-and-for-all confidence level is that this essentially amounts to treating default costs as the only legitimate item in the social planner's objective function and completely ignoring the importance of the bank lending function. This has the potential to lead to capital requirements that inefficiently exacerbate cyclical fluctuations in lending, as we describe next.

Potential cyclical problems with a fixed risk curve

Consider the effects of a recession under a regime of capital regulation that targets a fixed confidence level. The recession will have two effects. First, it will naturally lead to loan losses, thereby eroding banks' capital positions. Second, existing non-defaulted loans are likely to become significantly riskier—that is, to

have higher PDs. Thus, if there is a single fixed risk curve, as is contemplated under Basel II, the capital charges for banks' existing portfolios will go up. This will further tighten the overall capital constraint, putting additional downward pressure on lending activity.

This is not to say that any cuts in bank lending during a recession are undesirable. To the contrary—it is to be expected that there are to be fewer positive-NPV lending opportunities in bad times, so it is efficient for there to be some scaling back of bank loan portfolios.⁵ In terms of economic efficiency, the key item of interest is the *shadow value of bank capital*, which measures the scarcity of bank capital *relative* to positive-NPV lending opportunities. A higher shadow value of bank capital indicates a greater relative scarcity and, hence, more severe problems of underinvestment in terms of lending and, ultimately, in terms of physical investment. If a capital-regulation regime targets a fixed confidence level and at the same time leads to a shadow value of bank capital that rises markedly in recessions (that is, it leads to lending that is *excessively* procyclical), this would be suboptimal from the perspective of the tradeoff that we have described above.

It is not *a priori* obvious that the shadow value of bank capital must necessarily go up in bad times. There are two competing effects: On the one hand, loan losses and reduced operating income tend to lower the stock of bank capital, which pushes its shadow value up. On the other hand, a slowdown in aggregate economic activity means that there are fewer positive-NPV lending opportunities, which works in the opposite direction.

Determining which of the two effects dominates has been the subject of a number of empirical investigations.⁶ Peek and Rosengren (1995), in one of the most informative of these studies, examine the lending behavior of banks in New England during the 1990–91 recession. The point of focusing on this period is to isolate banks that faced a large common shock. They show that the banks that suffered the largest declines in capital cut lending the most—as would be predicted if capital constraints were binding. Most of the paper is devoted to showing that this finding is not plausibly interpreted as the result of the capital-impaired banks having bigger reductions in the demand for loans.

A recent paper by van den Heuvel (2002) argues that the Peek and Rosengren results apply more broadly to generic downturns that are induced by a tightening of monetary policy. He reports regressions showing that states in which the banking system is less well capitalized prior to a monetary tightening show larger subsequent output declines. This finding would be predicted if bank capital was scarce and bank borrowers could not offset declines in bank loans from other sources.

Thus, we read the literature on bank capital crunches as generally supporting the notion that bank capital is scarcer (that is, has a higher shadow value) during recessions.

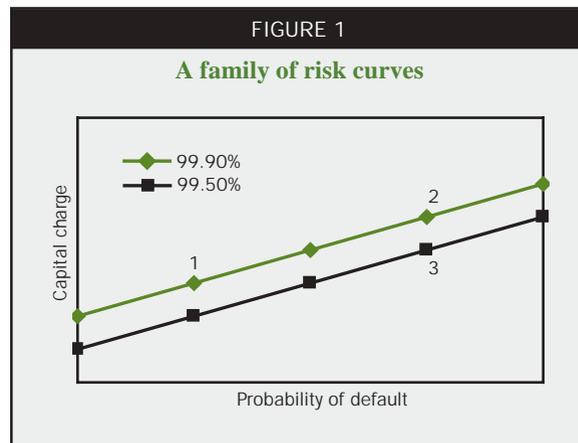
What do socially optimal capital requirements look like?

In the longer working paper version of this article (Kashyap and Stein, 2003), we present a model of the regulator’s problem that starts from first principles. Instead of just assuming that the goal of capital regulation is to target a fixed confidence level, we postulate that the regulator has an objective function that explicitly incorporates two considerations: 1) like the banks themselves, the regulator cares about the creation of positive-NPV loans (that is, loans on which the return exceeds the appropriate discount rate), but 2) the regulator also puts weight on the additional social costs of bank defaults. Using this model, we ask how the regulator can best maximize the objective function using capital regulation.

In the interests of brevity, and because the results are quite intuitive, we simply summarize the predictions from the model. The main finding is that instead of there being a single once-and-for-all risk curve that maps risk measures (such as the PD) into capital charges, optimality requires a *family* of point-in-time risk curves, with each curve corresponding to a different shadow value of bank capital—that is, to different macroeconomic conditions. In other words, one would target a high confidence level (for example, 99.90 percent) in good times, when bank capital is relatively plentiful and hence has a low shadow value. When there is a recession, and the shadow value of bank capital goes up, one would shift to a lower curve, corresponding to a reduced confidence level (for example, 99.50 percent) and reduced capital charges. Figure 1 provides an illustration of this approach.

There are two equivalent ways to accomplish the curve-shifting envisioned in figure 1. First, and most intuitively, the required ratio of capital to risk-weighted assets can adjust to offset movements in the shadow value of bank capital. Thus, for example, the capital requirement might be lowered from 8 percent to 6 percent in a recession. Alternatively, if the required ratio of capital to risk-weighted assets is to be kept fixed at some constant value (say 8 percent), then an increase in the shadow value of bank capital (for example, a recession) needs to be accompanied by a downward shift in the set of risk weights assigned to loans of varying degrees of riskiness.

The approach that we are outlining makes a sharp distinction between 1) the cross-sectional sensitivity



of capital charges to risk at a point in time; and 2) the time-series sensitivity of capital charges to risk over the business cycle. Consider first the cross-sectional question. For a fixed point in time, suppose we compare the optimal capital charges for two different types of loans. This corresponds to moving along a single curve in figure 1, say from point 1 to point 2. Thus, the relative capital charges for the two types of loans should depend only on their relative riskiness. Or, said differently, capital charges should be fully sensitive to risk in the cross-section.

Now consider the business cycle question. Suppose we have a fixed loan type, whose risk increases as we move from an expansionary environment to a recession (think of a borrower who has been downgraded). This corresponds to a movement *across* the two risk curves in figure 1, say from point 1 to point 3. In this case, the capital charge does not rise as sharply for a given increase in risk as it does in the cross-sectional case.

Limitations of the Basel II framework

This analysis highlights the problems inherent in the current Basel II framework, which envisions a single once-and-for-all risk curve, rather than a family of risk curves. Basel II would force the cross-sectional slope of the curve—for example, the ratio of capital charges for an AA credit and a BBB credit at a fixed point in time—to be the same as the time-series slope—for example, the ratio of capital charges for an AA credit that gets downgraded to BBB during a recession. As we have framed it, this amounts to trying to solve two problems with a single instrument.

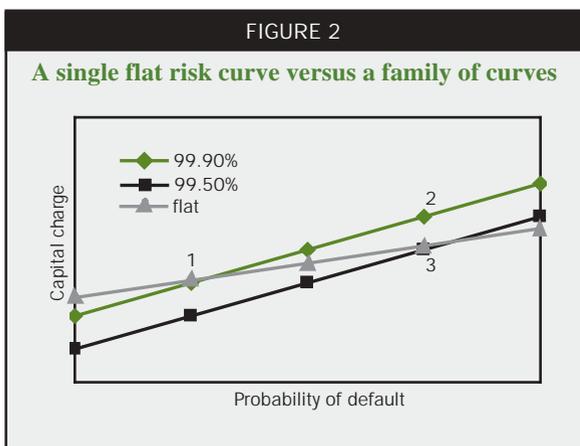
Of course, one can always do better on the time-series problem, say by flattening the curve, as is illustrated in figure 2. And indeed, recent revisions to the Basel II formulas have moved in the direction of such flattening.⁷ But with a single curve, this necessarily

comes at the expense of doing worse on the cross-sectional problem: a too-flat curve distorts banks' pricing of risk at a given point in time and is likely to be at odds with the methods that banks (appropriately) use for their internal decision-making.

Will banks offset cyclical risk by holding excess capital?

One argument that is sometimes made is that banks will naturally tend to offset any potential cyclical problems by holding buffer stocks of excess capital during good times. While this argument contains a kernel of truth, it is fundamentally unsound. A rational farsighted bank may indeed engage in some buffer-stocking. But essentially, the buffer stock will be set today so that if things turn out *as expected* at some future date, the bank will be no more capital-constrained then than it is today. Of course, the problem is that a recession is, almost by definition, an outcome that is worse than anticipated.⁸ So the *realized* shadow value of capital is still likely to go up significantly in a recession (that is, there is still likely to be something of a credit crunch), in which case our previous analysis continues to apply. By way of analogy, this is exactly like saying that even if an individual holds an optimal level of precautionary savings, he will nevertheless see his consumption fall if he is hit with a sufficiently adverse unexpected shock—for example, a serious illness or long-term layoff.

There is a simple reason why a family of risk curves is still preferred to a single once-and-for-all risk curve, even when banks are farsighted and hold optimal buffer stocks. When a bank chooses how much capital to hold at some initial point in time, it cannot know exactly what the shadow value of bank capital will be in the future. In contrast, with a family of risk curves, the regulator effectively gets to pick the right curve *after the fact*, once this shadow value is known.



Simulating the cyclical risk of risk-based capital requirements

Overview

We now turn to our empirical analysis.⁹ As noted in the introduction, our aim here is to simulate the effects that the Basel II framework could potentially have on the cyclical risk of bank capital requirements. To do so, we study the period from December 1998 to December 2002, an interval that was marked by pronounced economic slowdowns in both the U.S. and Europe. We also consider two different types of models that banks might conceivably use to estimate PDs under the new rules: 1) a model based on S&P counterparty ratings; and 2) a model developed by KMV that implements a Merton (1974) approach to estimating default probabilities. In each case, once we have the model-based PD for a given borrower, we apply “typical” values for the other three parameters (LGD, EAD, and M), and crank all four of these numbers through the Basel Committee’s formula to arrive at the required capital charge for that borrower.¹⁰ In other words, irrespective of the model, we always apply the same mapping to get from a borrower’s PD to its capital charge.

Wherever possible, we also disaggregate our universe of borrowers along two dimensions: 1) regional (for example, North America versus Europe); and 2) credit quality (that is, investment-grade versus non-investment-grade). We do so to get an idea of how the degree of cyclical risk might plausibly vary across banks with different loan portfolios. We will not take a stand on which particular subsample is most informative, since we expect different banks to have different customer mixes.

Likewise, we view the larger KMV sample and smaller S&P sample as two proxies for the experience of a diversified sample of bank customers. Ideally, one would like to know what happens to the typical small firm that is not publicly traded or rated as well. Unfortunately, tracking small firms over time is challenging, as they are more likely to fail and to have transitory relationships with banks, so finding consistent data on them is difficult. The longer working paper version of this article shows some calculations with a sample of Deutsche Bank borrowers, and for the borrowers that could be tracked the results were qualitatively similar to those for the KMV sample.

Before getting to the estimates, we need to confront a host of tricky measurement questions.

Methodological issues

In all of our simulations, the basic goal is to ask how the capital requirements for a *fixed loan portfolio* might evolve over the course of a business cycle

downturn. This is to be contrasted with the question of how an *actively managed portfolio* might behave. In the latter case, active management muddles together the direct effect of a tightened capital constraint with the bank's endogenous response. For example, suppose we look at the evolution of a bank's actively managed portfolio during a recession and find that average credit quality (and hence the mean capital charge) is roughly unchanged. Should we conclude from this that there is no cyclical problem deserving of policymaker attention? Probably not—it may just be that the bank has reacted to a tightening capital constraint by cutting off credit to its riskier borrowers, which is precisely the policy problem that concerns us.

Although the fixed-loan-portfolio question sounds straightforward, answering it with available data requires us to confront several methodological problems.

Survivorship bias

One problem that arises across all variants of our simulations is survivorship bias. This problem can be illustrated by reference to our S&P sample, which we describe in more detail shortly. The total number of non-defaulted firms that are present in this dataset as of December 1998 is 3,599. However, even holding aside firms that eventually default, another 542 of these original firms—about 15 percent—simply disappear from the sample by December 2002. These disappearances could reflect mergers, delistings, or unrecorded defaults.

If one draws the sample for our simulations by imposing the criterion that we must have data on a firm for all four years 1998–2002 in order for it to be included, this will create a potentially severe survivorship bias. In the S&P case, this would mean excluding the 542 firms that disappeared from the dataset, and one suspects that these firms probably had worse-than-average performance over the period even if they did not default. Thus, excluding them would lead us to understate the degree of cyclical in capital charges.

Therefore, in all of our simulations we begin with a sample that includes *every non-defaulted firm present in a given dataset in 1998*. This is in principle the right way to address the survivorship-bias issue. Of course, it raises another question, which is how we fill in the missing values for those firms that disappear later on.

Filling in missing values for firms that disappear from the sample

Suppose that a given firm is in the S&P dataset in 1998, with a rating of A–, and that it reappears in 1999, with a rating of BB+. After that, it disappears, so we have no further information on it for 2000, 2001, or 2002. How should we handle this observation?

In the full version of the paper we experiment with two different approaches. The first, which we call “freezing,” sets the rating (or, equivalently, the PD) in all the missing years to the last observed value. Thus in this example, the firm would be assigned a rating of BB+ for each year in the 2000–2002 interval. This method is simple enough, but almost certainly biased toward generating too little cyclical in our particular sample period. We know that on average firms were being downgraded over this period, so freezing the missing observations at stale ratings levels prevents us from capturing this tendency.

An alternative approach, which we call “imputation,” works as follows. If firm *i* is last observed in year *t*, we take all firms in the same geographic region and the same rating class as firm *i* that survive until *t* + 1. We then impute to firm *i* in years *t* + 1 the average rating (and hence average PD) of these surviving peers. In our previous example, the firm that disappears as a BB+ in 1999 would be attributed a rating in 2000 equal to the average year 2000 rating of all firms in its region that were also BB+ in 1999. We then follow an analogous procedure for years *t* + 2, etc.

This imputation approach still has its rough edges, but it gets around the basic problem of assuming that disappearing firms never would have experienced further downgrades had they remained in the sample. In the interests of brevity, we only report the results based on the imputation procedure, but in the longer version of the article we compare the two procedures and show that imputation generally leads to estimates of cyclical in that are greater than those that come out of the freezing method.

Handling firms that default

A distinct question has to do with how we handle a firm that remains in the dataset, but is known to be in default. Here again, we experiment with two different approaches. In the first, we keep defaulted firms in the simulations in all years. Thus, if a firm defaults in 1999, we keep it in as a defaulted firm (with a PD set to 1) in all subsequent years. With this approach, we always have exactly the same number of observations in our simulations in each year—that is, simply the number of firms that were not in default and were present in the data in 1998, 3,599 in the S&P dataset.

Alternatively, we discard defaulted firms in the year after they default. So if a firm defaults in 1999, we keep it in for 1999, but throw it out thereafter. In our S&P dataset, this leads us to reduce the number of observations by 304, to 3,295, as of 2002.

Unlike the freezing versus imputation question, here it is not obvious to us that one method is inherently

more attractive than another. Rather, we see them as answering two somewhat different questions. When we keep all the defaults in the sample, this tells us something about the *total* capital shortfall that a bank would have faced as a result of deteriorating economic conditions over the period 1998–2002, due to both loan losses and increased capital charges on remaining non-defaulted loans. Of course, some of this shortfall would have occurred even under the Basel I rules, as the bank would have had to eventually recognize losses on defaulted loans in either case. When we remove defaulted firms from the sample, this is essentially like asking how much *additional* (as compared to Basel I) cyclical capital charges Basel II creates as a result of the variation in capital charges that it imposes on firms that are downgraded, but that remain out of default.

An example may help to illustrate this distinction. Consider a bank with 100 in loans as of 1998, each with a capital charge of 8 percent. The bank is thus required to hold 8 in capital; assume that it just meets this constraint with equality in 1998. By 2002, ten of the original loans have defaulted, with losses given default of 45 percent (that is, the recovery rate is 55 percent). The remaining 90 in loans that are still on the books have been downgraded, and each now has a Basel II capital charge of 10 percent. Of course, under Basel I, the capital charge on these remaining loans would stay at 8 percent.

If no further capital is raised, the bank's total stock of capital in 2002 is 3.5—the original 8, less 4.5 in loan losses from defaults. Under Basel II, the capital charge for the remaining loans in 2002 is 9 ($9 = 90 \times 10$ percent). So the bank has a total shortfall relative to its capital requirement of 5.5 ($5.5 = 9 - 3.5$). Under Basel I, the capital charge in 2002 is 7.2 ($7.2 = 90 \times 8$ percent), and the bank has a total shortfall of 3.7 ($3.7 = 7.2 - 3.5$). The *extra* shortfall created as a result of switching from Basel I to Basel II is thus 1.8.

If we do not remove defaulted firms from our sample, our methodology will generate a total “capital charge” for 2002 of 13.5 under Basel II ($13.5 = 90 \times 10$ percent + 10×45 percent). So we would say that there has been an increase of 5.5 from the initial 1998 level of 8. That is, this approach captures the entire capital shortfall of 5.5 that arises under Basel II, due to both downgrades (1.8 of the shortfall) and defaults (3.7 of the shortfall). In contrast, if we do remove defaulted firms, we only look at the non-defaulted 90 of loans in both 1998 and 2002. For these loans, the capital requirement goes up by 1.8, from 7.2 to 9.0. Note that this figure of 1.8 corresponds exactly to the *increase* in the shortfall that arises as a result of the move from Basel I to Basel II.

To summarize, we experiment with two different methodologies:

Method 1: Keep all firms (including defaults) in the sample at all times, and use the imputation technique to deal with any missing firms.

Method 2: Remove firms in the year after they default, and use the imputation technique to deal with any missing firms.

Whether method 1 or 2 is preferred depends on the question at hand: that is, total cyclical versus extra cyclical capital charges created by Basel II.¹¹ The method 2 numbers are by their nature less dramatic, so if one is looking for a conservative set of estimates, these are probably the ones to look at first.

Results from S&P counterparty ratings

As noted above, our S&P universe starts with 3,599 non-defaulted firms for which we have ratings information as of December 1998. We convert each firm's rating into a PD, using a fixed correspondence across all regions and time periods.¹² The PDs in turn can be mapped into capital charges, using the Basel Committee's formula. We then compute mean capital charges in each year, dealing with defaulted and missing firms according to the protocols spelled out in methods 1 and 2.

In table 1, we show the percent change in capital charges over the 1998–2002 period, both for the full sample and for a variety of subsamples. (When we split the sample into investment-grade and non-investment-grade subsamples, we do so once and for all based on a firm's rating as of 1998.) If one looks at the results corresponding to our preferred method 2, they tell a reasonably consistent story across Europe and North America, as well as across investment-grade and non-investment-grade firms: Increases in capital charges are generally in the range of 30 percent to 45 percent over the period 1998–2002. The “rest of world” subsample yields considerably higher increases, in the 80 percent range, but one should probably not make too much of this particular finding, as the sample size here is very small—only 320 borrowers in total.

Not surprisingly, method 1, which keeps defaulted firms in the sample, generates substantially higher estimates, with numbers in many cases approaching 100 percent. Again, however, these numbers are best thought of not as a measure of the incremental degree of cyclical capital charges associated with the transition from Basel I to Basel II, but rather as a measure of the total capital shortfall that banks experience in a recession, due to a combination of downgrades and loan losses.

TABLE 1

Capital-charge cyclicity, 1998–2002, using S&P ratings

Method	Region	Rating class	Initial capital, 1998	Percent change capital 1998–2002
1	All	All	5.85	86.36
1	All	Investment-grade	2.40	59.28
1	All	Non-investment-grade	11.59	95.66
1	Europe	All	3.05	66.21
1	Europe	Investment-grade	1.84	41.49
1	Europe	Non-investment-grade	12.19	94.65
1	North America	All	6.28	84.36
1	North America	Investment-grade	2.52	56.92
1	North America	Non-investment-grade	11.56	92.73
1	Rest of world	All	6.07	118.99
1	Rest of world	Investment-grade	2.55	105.10
1	Rest of world	Non-investment-grade	11.70	123.84
2	All	All	5.85	31.45
2	All	Investment-grade	2.40	43.67
2	All	Non-investment-grade	11.59	42.90
2	Europe	All	3.05	33.40
2	Europe	Investment-grade	1.84	41.49
2	Europe	Non-investment-grade	12.19	47.86
2	North America	All	6.28	26.81
2	North America	Investment-grade	2.52	37.64
2	North America	Non-investment-grade	11.56	37.66
2	Rest of world	All	6.07	77.89
2	Rest of world	Investment-grade	2.55	97.04
2	Rest of world	Non-investment-grade	11.70	87.92

Notes: Investment-grade (IG) refers to all firms with a rating of BBB– or better in December 1998; non-investment-grade (non-IG) refers to those with a rating of BB+ or worse. There are 3,599 observations in the full sample (2,247 IG and 1,352 non-IG); 456 in the Europe subsample (403 IG and 53 non-IG); 2,823 in the North America subsample (1,647 IG and 1,176 non-IG); and 320 in the rest-of-world subsample (197 IG and 123 non-IG).

Although the method 2 estimates are not huge, they still would appear to be economically significant. One useful benchmark is that the method 2 estimates are in most cases at least 40 percent of the corresponding method 1 numbers, and sometimes (typically with investment-grade borrowers) quite a bit more. This suggests that the *added* cyclical pressure on bank capital positions associated with Basel II is of almost the same order of magnitude as the preexisting baseline effect under Basel I.¹³ In other words, to the extent that there have previously been concerns expressed about capital crunches during recessions, the change from Basel I to Basel II might be expected to—very loosely speaking—almost double the impetus behind these episodes.

The method 2 estimates are also noteworthy in the context of rating agencies' stated goal of rating borrowers "through the cycle." Under this approach, a borrower's rating is supposed to be based not on its current likelihood of default per se, but instead on its probability of default in a fixed hypothetical downside

scenario. This is clearly intended to smooth out ratings over the course of a business cycle. But as we can see in the table, this smoothing is far from total—there remains a good deal of cyclicity.

Results from KMV model

Table 2 presents our results from the KMV model simulations. Before comparing these results to those in table 1, it is important to note that the samples are quite different.¹⁴ Our KMV sample starts with a much larger universe of firms (17,253 versus 3,599) and a larger fraction of these firms are non-investment-grade than in the S&P data (58 percent versus 38 percent). One way this shows up is in the mean initial capital charge in 1998: It is 10.72 percent for the entire KMV sample, versus 5.85 percent for the S&P sample.

These differences imply that it may not be too meaningful to directly compare the cyclicity estimates for the two full samples; these comparisons will be confounded by the fact that we are not holding the composition of firms constant. It probably makes a bit more sense to compare the investment-grade subsamples,

where the compositional differences are likely to be less of an issue.

When we do so, it appears that the KMV methodology leads to substantially more cyclical capital charges. For example, using method 2 and focusing on all investment-grade borrowers, the mean S&P-based change in capital from 1998 to 2002 is 44 percent, while the corresponding KMV number is 83 percent. This is consistent with previous research, which has come to the same basic conclusion. And it also fits with the idea that the KMV approach is meant to deliver a “point-in-time” estimate of default risk, as opposed to the smoothed, “over-the-cycle” construct used by the rating agencies.¹⁵

Interestingly, however, one does not see the same comparative patterns when focusing on the entire samples or on the non-investment-grade subsamples.

Indeed, for all non-investment-grade borrowers, the mean KMV-based change in the capital charge under method 2 is only 3 percent. While this may at first seem counterintuitive, there is in fact a simple explanation. The initial 1998 capital charge for this non-investment-grade subsample in the KMV data is so high—at 15.64 percent—that, if we exclude firms that go into default, there is just not much room for the remaining non-defaulted firms to see their capital charges get much higher. Simply put, if one looks at a group of firms that start out with very low credit ratings, these ratings cannot get much lower outside of default, and so there can never be more than a modest effect on capital charges as a result of downgrades.

Conversely, highly rated firms have much further to fall (again, outside of default), and so the banks that lend to them are potentially more vulnerable to the sort

TABLE 2

Capital-charge cyclical, 1998–2002, using KMV model

Method	Region	Rating class	Initial capital, 1998	Percent change capital 1998–2002
1	All	All	10.72	35.91
1	All	Investment-grade	4.01	111.47
1	All	Non-investment-grade	15.64	21.61
1	Germany	All	5.78	82.24
1	Germany	Investment-grade	3.40	161.91
1	Germany	Non-investment-grade	12.16	24.56
1	Rest of Europe	All	7.26	62.53
1	Rest of Europe	Investment-grade	3.66	139.07
1	Rest of Europe	Non-investment-grade	13.12	27.90
1	North America	All	12.17	44.45
1	North America	Investment-grade	4.34	107.60
1	North America	Non-investment-grade	16.56	35.14
1	Rest of world	All	11.81	11.09
1	Rest of world	Investment-grade	4.12	77.91
1	Rest of world	Non-investment-grade	15.69	2.17
2	All	All	10.72	13.81
2	All	Investment-grade	4.01	82.54
2	All	Non-investment-grade	15.64	3.20
2	Germany	All	5.78	59.11
2	Germany	Investment-grade	3.40	113.57
2	Germany	Non-investment-grade	12.16	18.50
2	Rest of Europe	All	7.26	31.27
2	Rest of Europe	Investment-grade	3.66	92.62
2	Rest of Europe	Non-investment-grade	13.12	5.64
2	North America	All	12.17	9.86
2	North America	Investment-grade	4.34	73.27
2	North America	Non-investment-grade	16.56	4.23
2	Rest of world	All	11.81	10.33
2	Rest of world	Investment-grade	4.12	80.34
2	Rest of world	Non-investment-grade	15.69	1.72

Notes: Investment-grade (IG) refers to all firms with a KMV EDFTM of 0.94 percent or better in December 1998; non-investment-grade (non-IG) refers to the remainder. (This breakpoint is obtained by calculating the mean values of EDFTM for BBB-rated and BB-rated firms, and then taking the midpoint.) There are 17,253 observations in the full sample (7,292 IG and 9,961 non-IG); 378 in the Germany subsample (275 IG and 103 non-IG); 4,183 in the rest-of-Europe subsample (2,593 IG and 1,590 non-IG); 7,051 in the North America subsample (2,532 IG and 4,519 non-IG); and 5,641 in the rest-of-world subsample (1,892 IG and 3,749 non-IG).

of cyclicity in capital charges induced by the Basel II framework. This observation brings us back to our earlier point, about the importance of heterogeneity across banks. Even if the Basel II capital requirements do not create a large amount of cyclical variation for all banks, they may well have large cyclical effects on banks with particular kinds of portfolios, in this case, banks that lend to relatively high-credit-quality firms.

Related literature

A handful of other recent papers have attempted to perform simulations more or less like those in the previous section. These include Carling, Jacobson, Linde, and Roszbach (2002), Catarineu-Rabell, Jackson, and Tsomocos (2003), Corcostegui, Gonzalez-Mosquera, Marcelo, and Trucharte (2002), Heid (2003), Jordan, Peek, and Rosengren (2003), Rosch, (2002), and

TABLE 3

Summary of other research on capital-charge cyclicity

Study	Country	Period	Capital charge basis	Maximum change in capital (% unless stated otherwise)	Notes
Ervin and Wilde (2001)	U.S.	1990–92	Not specified	20	All BBB borrowers
Segoviano and Lowe (2002)	Mexico (Customers of main large banks)	3/95–	Foundations, Nov. 2001	69.8	Includes E-rated loans; peak losses are in 12/96; capital changes inferred from their table 2.
		12/99	Standardized	57.1	
Segoviano and Lowe (2002)	Mexico (Customers of main large banks)	3/95–	Foundations, Nov. 2001	56.7	Excludes E-rated loans; peak losses are in 12/96; capital changes inferred from their table 2.
		12/99	Standardized	15.7	
Catarina-Rabell, Jackson, and Tsomocos (2003)	U.S. high-quality banks' customers U.S. average-quality banks' customers Same quality customers as Deutsche Bank	1990–92	QIS 3, 10/02	15.2	Based on Moody's ratings transitions of 5,022 non-defaulting corporate borrowers, using different initial borrower distributions described in column 2.
				17.9	
				15.3	
Catarina-Rabell, Jackson, and Tsomocos (2003)	U.S. high-quality banks' customers U.S. average-quality banks' customers Same quality customers as Deutsche Bank	1990–92	QIS 3, 10/02	53.2	Based on Merton model PD transitions of 282 borrowers, using different initial borrower distributions described in column 2.
				8.8	
				47.1	
Jordan, Peek, and Rosengren (2003)	U.S.	1996–2001	11/01	S&P: ≈20% KMV: ≈280%	Shared National Credit borrowers, all loans exceed \$20 million.
Rosch (2002)	U.S.	1982–2000	11/01	+15	Multiple <i>one-year</i> swings of this size, based on S&P transitions.
Corcostegui, Gonzalez-Mosquera, Marcelo, and Trucharte (2002)	Spain	1993–2000	QIS 3, 10/02	–6.1 percentage points	No base level given, + 3.1 p.p. the year before this swing.
Carling, Jacobson, Linde, and Roszbach (2002)	Sweden	1994–2000	1/01	–11.23 percentage points	No base level of capital given, two methods of gauging PDs, either historical default experience (top) or based on one model (bottom).
				–20.37 percentage points	

Segoviano and Lowe (2002). It is hard to directly compare the numbers across all the studies, because they vary along a number of dimensions. These include: 1) the sample period; 2) the universe of firms under consideration; 3) the model used to derive PDs; and 4) the fact that different papers use different iterations of the Basel Committee formula that maps the PD and other credit-risk parameters into a capital charge. (Recall that this formula has been updated more than once, most recently in October 2002.)

In table 3, we take a rough cut at summarizing the key assumptions and results of several of these other papers; in some cases we have had to read a bit between the lines to do so. Here we will focus on a couple that seem closest to what we have done: Catarineu-Rabell, Jackson, and Tsomocos (2003), hereafter CJT, and Jordan, Peek, and Rosengren (2003), hereafter JPR. As we do, these papers perform simulations using both credit ratings and a KMV-based approach.

With respect to the former methodology, CJT examine a sample of Moody's-rated firms over 1990–1992, and estimate an increase in Basel II capital charges of between 15 percent and 18 percent for this period, depending on the sample of firms considered. These numbers are somewhat lower than the 30 percent–45 percent range that we obtained with the S&P data, using our preferred method 2. Some of this difference may well be due to the different sample period and to the different universe of firms that they consider. But we speculate that some of it is also likely to be due to CJT's not handling survivorship bias in the same way we do; as best as we can tell, they only consider firms for which data was available in *every year* from 1990 to 1992, which raises the sorts of problems we discussed above.

One clue that survivorship bias may in fact help to reconcile these results is that when we redo the analysis using the freezing (rather than imputation) approach to missing firms—which can be thought of as only a partial fix for survivorship bias—we get an estimate of 23 percent for the sample of all firms, quite close to the CJT numbers. Going further, if we totally disregard survivorship issues altogether, and simply draw our sample from all firms that were present in the S&P database throughout the period 1998–2002, our estimate falls to 20 percent.

Essentially the same observations apply to JPR. Using S&P credit-rating data from 1996 to 2001, they estimate an increase in capital charges of about 20 percent. This is closely in line with the results of CJT, and again, somewhat lower than the numbers we obtain. As with CJT, we suspect that JPR do not take

account of survivorship bias in the same way we do and that this helps to explain why their estimates of cyclicity are smaller than ours.

With respect to the KMV-based analyses, the qualitative conclusions of CJT run closely parallel to ours. In particular, they find that the KMV model can induce a much greater degree of cyclicity in capital charges (they get numbers on the order of 50 percent, even without addressing survivorship-bias issues), but that this tends to happen only when one looks at a portfolio of high-credit-quality firms. For firms of lower credit-quality, the cyclical effects generated by KMV appear to be much more modest in their simulations, as in ours.¹⁶

Conclusion

We take two broad messages away from the work reported here. On the empirical front, our simulations suggest that the new Basel II capital requirements have the potential to create an amount of additional cyclicity in capital charges that is, at a minimum, economically significant, and that may be—depending on a bank's customer mix and the credit-risk models that it uses—quite large. Our empirical analysis also underscores the importance of dealing with various methodological issues that crop up in this context, most notably survivorship bias. As we have shown, failure to do so can lead to estimates of cyclicity that are substantially downward-biased.

On the theory side, our main point is that the Basel II approach of having a single time-invariant risk curve is, in principle, suboptimal. Instead, it is more desirable to have a family of risk curves—that is, to tolerate a greater probability of default when economy-wide bank capital is scarce relative to lending opportunities.

One obvious objection to this conclusion is that it is naïve with respect to political-economy considerations. In particular, one might argue that any theory that suggests reducing capital requirements in bad times will simply give regulators an excuse to engage in after-the-fact forbearance, with all the accompanying potential for various forms of regulatory moral hazard.

While we agree that such moral hazard concerns are of great importance, we believe the above argument can be turned on its head. If it really is the case that capital requirements need to come down, say, in a severe recession, it is probably better to acknowledge this fact of life up front and to explicitly codify the magnitude of the adjustment. Such *ex ante* codification will, if anything, tend to reduce regulators' *ex post* discretion, thereby tempering moral hazard problems. In contrast, with an unrealistically rigid *ex ante* rule that never contemplates the need to reduce capital

requirements, the risk is that in a sufficiently bad scenario, the rule will become de facto untenable. At this point, it will be left to regulators to relax the rule as they see fit—perhaps on a highly subjective case-by-case basis—without any previously imposed constraints.

Of course, this discussion raises the question of how one might design a credible, transparent formula that links capital requirements to some measure of aggregate economic conditions. This is a difficult question and one that we are not prepared to answer fully. But we venture a few thoughts. At one extreme, it is easy to imagine crude rules that are based on aggregate business cycle indicators. For example, one might drop the required ratio of capital to risk-weighted assets from 8 percent to 6 percent whenever GDP growth falls below some threshold.¹⁷

While formulas of this sort seem like they would be easy enough to implement, there may be a good deal of slippage between something like GDP growth and the construct that the theory tells us should be relevant, namely the shadow value of bank capital. Alternatively, one can try to come up with other, more sophisticated indicators that track this shadow value more closely.¹⁸ The tradeoff is that an overly complicated, hard-to-verify measure is probably not much better than no measure at all.

One way to get around these problems might be to create a market for regulatory capital relief. In particular, suppose that the regulator periodically auctions off a small supply of tradable certificates, each of which entitles the holder to breach the standard 8 percent capital requirement by, for example, \$1 million for one year. The market price of these certificates would then be a direct and transparent measure of the shadow value of bank capital—that is, a high price would indicate a relative shortage of bank capital. Moreover, by allowing the regulator to increase the supply of certificates in the face of rising prices, it would become possible to tie the effective capital requirement to this shadow value, just as the theory suggests should be done.

We should stress that this last suggestion is intended in the spirit of preliminary brainstorming, and nothing more; we have not even begun to come to grips with the many practical complexities that it would surely entail. However, our aim here is not to push any one particular proposal for linking capital requirements to economic conditions. Rather, we just hope to call attention to the general issue, and to illustrate that there is a lot of room for further thought about it.

NOTES

¹For an overview of the Basel II accord, see Basel Committee on Banking Supervision (2003). Basel II is currently scheduled to take effect in 2007.

²See Jackson (2002) for an explicit description of the Basel Committee's capital requirement formula and its evolution over time.

³Several others have made similar suggestions. See, for example, Gordy and Howells (2003), Rosch (2002), Ervin and Wilde (2001), Purhonen (2002), and Cosandey and Wolf (2002).

⁴See, for example, Lowe (2002) for references. We discuss this work in some detail below.

⁵In fact, many would argue that the lending opportunities dry up in advance of a downturn and that the loans that go bad in the recession are the product of earlier bad decisions. In this case, tying the capital increase to the recognition of the loan losses is inappropriate.

⁶Some well-known papers in this literature include Bernanke (1983), Bernanke and Lown (1991), and Peek and Rosengren (1997).

⁷This is not to say that the motivation for these revisions was necessarily to address the particular time-series problem that we have identified here.

⁸A similar argument can be made with respect to loan-loss provisions, which can be thought of as an attempt to set aside reserves to deal with *expected* losses.

⁹All the simulations reported below were carried out with the assistance of Michael Luxenburger of Deutsche Bank, whose work we gratefully acknowledge.

¹⁰We set LGD = 0.45; M = 2.5 years; and EAD = 1, as is done in the foundation approach. In all cases, we use the formula for firms in the largest size class by revenues. Note that by keeping LGD fixed over the business cycle, we may be ignoring a source of variability in capital charges, since in reality, it is likely that recovery rates on defaulted loans are lower in recessions. If this is so, and if a given bank's internal model takes this variation in LGD into account, it will tend to have more cyclicalities in (IRB advanced) capital charges than our results suggest.

¹¹One might argue that method 2 should remove defaulted firms from the data *immediately*—rather than in the year after they default—if the goal is to get a sense of the purely incremental effect

created by the Basel II downgrading mechanism. The problem with this approach (as applied to annual data) is that it leads us to ignore any within-year downgrades that occur. For example, suppose a firm is rated BB at year-end 1999, slips to CCC by mid-year 2000, and then defaults right before the end of 2000. If we were to delete this firm from the data after 1999, this would amount to ignoring its final and most significant out-of-default downgrade.

¹²The mapping is as follows: AAA corresponds to a PD of 0.01 percent; AA to 0.03 percent; A to 0.07 percent; BBB to 0.23 percent; BB to 1.07 percent; B to 4.82 percent; CCC to 22 percent; and D to 100 percent. Because these PDs probably rise during downturns, this mapping presumably leads us to understate the degree of cyclicalities in the capital charges.

¹³If the ratio of the method 2 to method 1 estimate is 40 percent, this suggests that the *extra* cyclicity effect due to Basel II is 67 percent (40/60) of the preexisting effect due to Basel I alone.

¹⁴In the KMV data, we are not told explicitly when a firm is in default. So we make the following approximation: We regard a firm as defaulted if the KMV model yields an EDFTM (that is, an estimated default probability) of 20 percent, and the firm disappears from the data in the following year. We then apply our various sample-selection protocols as before.

¹⁵A paper published by KMV (1998) shows that their ratings are in fact considerably more volatile on a year-to-year basis than those from S&P.

¹⁶Further confirming evidence on this point comes from Purhonen (2002).

¹⁷Here we are also ignoring all the problems associated with the timing of data releases and the revisions to macro data.

¹⁸For example, following Kashyap, Stein, and Wilcox (1993), one might look at the ratio of commercial paper issuance to bank loan growth as a measure of tightness in bank loan supply, and hence as a proxy for the relative scarcity of bank capital. But doing this kind of indexation properly will likely require a number of adjustments to account for trends in the data, other sources of shocks, and so on.

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