The unreliability of credit-to-GDP ratio gaps in real-time and the implications for countercyclical capital buffers*

Rochelle M. Edge
Office of Financial Stability Policy and Research, Federal Reserve Board
Ralf R. Meisenzahl
Division of Research and Statistics, Federal Reserve Board

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Abstract

Macroeconomists have long recognized the unreliability of activity gap measures and the acute difficulties that this can cause in formulating economic stabilization policies. The use of the nominal credit-to-GDP ratio gap as a reference point in taking decisions on accumulating countercyclical capital buffers—as has been put forward as a principle of the Basel Committee's countercyclcial capital buffer proposal—suffers potentially from the same problem of being unreliable in real-time and this could imply similar difficulties for bank supervisors in implementing macroprudential policies. This papers investigates the relevance of this issue for the credit-to-GDP ratio gap in the U.S. following the approach set out by Orphanides and Van Norden (2002) and finds that, similar to these authors' conclusions with regard to the output gap, *ex-post* revisions to the U.S. credit-to-GDP ratio gap are sizable and are on the same order of magnitude of the gap itself. Moreover, the main source of the revision stems not from revised estimates of the underlying data but rather from the unreliability of end-of-sample estimates of the trend credit-to-GDP ratio. Finally, the paper considers the potential costs of gap mismeasurement, in terms of the volume of lending that in real time may be incorrectly curtailed. On balance, we find this reduction in lending to be potentially large; although, loan interest-rates appear to increase only modestly.

*E-mail: rochelle.m.edge@frb.gov & ralf.r.meisenzahl@frb.gov. Mailing address: Federal Reserve Board, 20th and C Streets NW, Washington, DC 20551. We thank Jeremy Rudd useful comments on several of the filtering issues considered in this paper. The views expressed here are our own and do not necessarily reflect the views of the Board of Governors or the staff of the Federal Reserve System.

Macroeconomists have long recognized the unreliability of real-time output- and unemployment-rate gap measures and the acute difficulties that this can cause in formulating economic stabilization policies (see, Orphanides and Van Norden, 2002, and Staiger, Stock, and Watson, 1997). The use of the nominal credit-to-GDP ratio gap as a reference point in taking decisions on accumulating countercyclical capital buffers—as has been put forward as a principle of the countercyclcial capital buffer proposal of the Basel Committee's Macro Variables Task Force (MVTF)—suffers potentially from the same problem of being unreliable in real-time and this could imply similar difficulties for bank supervisors in implementing this type of macroprudential policy. This papers investigates the relevance of this issue for the credit-to-GDP ratio gap in the U.S. following the approach set out by Orphanides and Van Norden. Specifically, using U.S. data we calculate estimates of the credit-to-GDP ratio gap that would have been derived in real-time and would therefore have been used in practice by bank supervisors in setting countercyclical buffers and compare these with estimates that would be derived *ex-post* that tell us how—based on all the information to date—countercyclical buffers should have been set at some past point in time. Finally, we estimate potential costs of gap mismeasurement and find that the volume of lending that in real time may be incorrectly curtailed is potentially large.

As noted by Orphanides and Van Norden there are several reasons why real-time estimates of gap measures can differ from their final estimates. First, the underlying data used to calculate the gap measures—in our case, nominal private-sector credit and nominal GDP—can revise. Second, as data in later periods becomes available it may (even without any revisions to past data) alter our estimate of where the trend credit-to-GDP ratio was at some past point in time and therefore what the gap at that time was between the actual and trend ratio. Third, incoming data may cause us to revise our model of the timeseries of the credit-to-GDP ratio, which in turn will cause us to revise our past estimates of the trend and thereby the gap. We consider all three sources of revisions to the U.S. credit-to-GDP ratio gap, although we are only able to consider revisions that arise from changes to the underlying data for a limited period of time because we only have real-time vintages of credit data (in electronic form) dating back the 1995:Q2.[1] We would note that the MVTF's Countercyclical Capital Buffer Proposal Consultative Document certainly does indicate awareness of these issues.[2] So too do other key policy papers reviewing practical issues relating to the implementation of macroprudential policies.[3] However, these documents do not

[1]Hard copy vintages of the data are available for earlier periods. However, we decided against extending the real-time data analysis earlier because the main source of revisions to our gap estimates does not stem from data revisions but rather from the unreliability of end-of-sample estimates of the trend credit-to-GDP ratio.

[2]For example, while the analysis the Consultative Document does not use real-time data, the issue described above is noted in footnote 12 of the document. Likewise, although the analysis does not consider how gap estimates for any point in time change as data for subsequent periods becomes available, the credit-to-GDP ratio gap measures presented in the paper all extract the *one-sided* Hodrick-Prescott filter trend, for the clearly stated reason that this trend measure is constructed using only information that us available at the time of the observation period.

[3]See the discussion on signal extraction (on page 9) of the Committee on the Global Financial (CGFS) Report on

give any indication as to the magnitude of the uncertainty of real-time credit-to-GDP ratio gaps—a critical question if these measures are to be used to inform decisions on accumulating capital buffers—which is what this paper does.

The MVTF's Countercyclical Capital Buffer Proposal Consultative Document considers only one method—the Hodrick-Prescott (HP) filter—for extracting the trend credit-to-GDP ratio from the actual ratio. While this unobserved components detrending method is, as the Consultative Document states, "a standard mathematical tool used in macroeconomics to establish the trend of a variable over time" it is by no means the only one and indeed another source of uncertainty for bank supervisors in considering the credit-to-GDP ratio gap is that which arises from different methods of trend extraction. Thus, we consider a range of alternative detrending methods including approaches that assume a deterministic trend—specifically, a linear trend, a quadratic trend, a cubic trend, and a cubic spline—as well as an approach that considers a series' trend to be the component of the series that falls within a specified frequency range, which we extract using an approximate band-pass filter.

We find that revisions to the U.S. credit-to-GDP ratio gap are sizable and are on the same order of magnitude of the gap itself. Moreover, the main source of the revision stems not from revised estimates of the underlying data but rather from the unreliability of end-of-sample estimates of the trend credit-to-GDP ratio. Some of the episodes in which we find large revisions to estimates of the credit-to-GDP ratio gap correspond to periods in which the real-time estimate of the gap would have suggested a deployment of countercyclcical capital buffers but the final gap would not. We focus on three periods where this was the case—1999:Q3, 2001:Q4, and 2003:Q2—and calculate the potential cost to the economy of the credit-to-GDP ratio gap indicating in real-time that countercyclical capital buffers should be deployed. Using the capital ratios of U.S. banks in the Reports of Conditions and Income, we calculate the change in the system-wide capital shortfall/surplus implied by deploying the capital buffers. We then derive implications for the lending drawing on the literature on the effect of bank capital on lending. As noted by Kashyap, Stein, and Hanson (2010) the literature disagrees on the size of the effect. Therefore, we provide lower and upper bound estimates for the reduction in lending. If the proposed countercyclical capital buffer regime had been in effect in the quarter with the largest real-time mismeasurements, lending may have been reduced by up to 9 percent. It is worth noting that in the last two of these instances countercyclical capital buffers would have amplified an economic downturn or threatened a fragile recovery.

This paper is organized as follows. Section 1 outlines the detrending methods that we consider in this study while section 2 describes the data that we employ and the "real-time" and "final"

"Macroprudential instruments and frameworks: a stocktaking of issues and experiences".

trend-estimate concepts that we consider. These concepts—along with the taxonomy used—is the same as those laid out by Orphanides and Van Norden. Section 3 then reports our results, including our credit-to-GDP ratio gap estimates, the magnitudes of our gap revisions, and some of the implications for policy decisions—in real-time and *ex post*—resulting from these revisions. Section 4 gauges the potential costs of gap mismeasurement in terms of the volume of lending that could be incorrectly curtailed as a result of policy decisions based on misleading real-time estimates of the credit-to-GDP ratio gap, Finally, section 5 concludes.

1 Detrending methods

Before reviewing the different detrending methods that we will be considering in our study, it is useful to take a look at the series that we are attempting to model the trend for and in turn obtain the gap between the series and the trend. The black line in panel A of Figure 1 reports the timeseries of the nominal credit-to-GDP ratio using the definitions of variables given in the MVTF's Consultative Document and based on 2010:Q4 vintage data. For nominal credit (in the numerator) this is the volume of credit market debt outstanding of the non-financial corporate business sector and household and nonprofit organization sector as reported by the Federal Reserve Board (FRB) in the Flow of Funds Accounts (FOFA). For nominal GDP (in the denominator) this is the measure reported by the Bureau of Economic Analysis (BEA) in the National Income and Product Accounts (NIPA). As can be seen from panel A of Figure 1 there is a distinct upward drift in the credit-to-GDP ratio. Thus it is deviations from the upwardly trending path of the credit-to-GDP ratio that bank supervisors would want to use as their reference point in making decision on the accumulation of countercyclical capital buffers. The blue line in panel A also shows the credit-to-*potential*-GDP ratio. The series in the numerator of this ratio is the same as for the credit-to-GDP ratio. The series in the denominator of this ratio is the February 2011 Congressional Budget Office's estimate of potential output. This alternative ratio is used in the paper for performing some additional exercises; we include it here for future reference.

We use a number of different detrending methods to estimate the credit-to-GDP ratio gap. All detrending methods separate the movements in a series, c_t, into a trend component, μ_t, and a cyclical component z_t; that is,

$$c_t = \mu_t + z_t. \tag{1}$$

Some methods use data to estimate a time-dependent trend, μ_t, and then define the residual as the cyclical component, z_t. Such trends—called deterministic trends—include a linear trend, a quadratic trend, a cubic trend, and a cubic spline and these are described in subsection 1.1. Another method specifies a dynamic structure for both the trend and cycle components and estimates these components jointly. This method is described in subsection 1.2. The HP filter, which was the trend extraction method used by the MVTF in the Countercyclical Capital Buffer Consultative Document, falls into this class of detrending models and is the only unobserved components model that we use. The final approach that we consider is that of frequency detrending methods. These methods view economic time series as being the weighted sum of periodic functions (specifically, sine and cosine functions) and consider a series' trend to be that part of the series accounted for by functions that fall within a specified frequency range. We implement this method using an approximate band-pass filter.

Different trend extraction methods give different signals—even at the same point in time—as to the extent to which credit levels are excessive, and so add an additional complication to bank supervisors in deciding whether to deploy countercyclical buffers. This complication is, however, a different issue from what is our primary concern in this paper and so we do not dwell too much on it. A closely related issue in making credit-to-GDP ratio gaps operational in macroprudential policy is, however, that of the most appropriate filter to use for detrending. An augmented Dickey-Fuller test of the credit-to-GDP ratio over the full sample indicates that the series contains a unit root and, thus, has a stochastic trend. This suggests that some filtering methods—such as, the HP filter and an approximate band-pass filter, which are able to remove a unit root—are better suited to detrend the series than others—such as, any of the deterministic detrending methods, which are not able to remove a unit root. While this finding does not result in us dismissing entirely any consideration of deterministic detrending methods, it is a fact about the different detrending methods that we keep in mind, especially alongside the different methods' revision properties.

1.1 Deterministic detrending methods

Deterministic detrending methods assume that the trend credit-to-GDP ratio can be well approximated as a simple function of time. Such a trend can be linear, quadratic, or cubic; that is, modeled as following the process:

$$\hat{\mu_t} = \hat{\alpha} + \hat{\beta}_1 \cdot t, \tag{2}$$

$$\hat{\mu_t} = \hat{\alpha} + \hat{\beta}_1 \cdot t + \hat{\beta}_2 \cdot t^2, \text{ or} \tag{3}$$

$$\hat{\mu_t} = \hat{\alpha} + \hat{\beta}_1 \cdot t + \hat{\beta}_2 \cdot t^2 + \hat{\beta}_3 \cdot t^3. \tag{4}$$

The estimated coefficients for equations (2), (3), and (4) are obtained from estimating on actual data the following equations:

$$\begin{aligned}\mu_t &= \alpha + \beta_1 \cdot t + \nu_t, && (5)\\ \mu_t &= \alpha + \beta_1 \cdot t + \beta_2 \cdot t^2 + \nu_t, \text{ and} && (6)\\ \mu_t &= \alpha + \beta_1 \cdot t + \beta_2 \cdot t^2 + \beta_3 \cdot t^3 + \nu_t. && (7)\end{aligned}$$

Of course, a single deterministic process may not in practice be appropriate for modeling the trend of a series over the series' entire sample period. For example, it could be the case that in the early part of the sample a cubic process with one set of parameters represents the trend in the credit-to-GDP ratio whereas later in the sample the trend is represented by a cubic process with another set of parameters. In this case it is possible to model the trend process with a cubic spline, whereby in addition to allowing differently parametrized cubic processes to approximate the trend over different sample periods linear restrictions are placed on the parameters of the two cubic processes to ensure that at a specified period—called a knot-point—the level and first and second derivatives of the two cubic processes are equal. In considering this approach for modeling the trend in the credit-to-GDP ratio we assume two knot points in the sample period and we set these at even thirds. This means that our knot points will move when we undertake our real-time analysis.

1.2 Unobserved components detrending methods

Unobserved components methods specify a dynamic structure for the trend and cycle components of the credit-to-GDP ratio and estimate them jointly. The Hodrick-Prescott (HP) filter can be specified in terms of an unobserved components model; specifically, as the state-space model:

$$\begin{aligned}c_t &= \mu_t + z_t\\ \mu_t &= 2 \cdot \mu_{t-1} - \mu_{t-2} + \eta_t, && (8)\end{aligned}$$

where z_t and η_t denote mutually uncorrelated white noise shock processes with variances of σ_z^2 and σ_η^2, respectively. Setting σ_z^2/σ_η^2 to equal the parameter λ, the Kalman-smoother can be used to estimate the path of the trend, $\{\mu_t\}_{t=0}^T$. The path of the trend credit-to-GDP ratio derived from this method, is wholly equivalent to the trend that would be derived from the standard HP filter optimization problem:

$$\min_{\{\mu_t\}_{t=0}^T} \sum_{t=0}^{T} (c_t - \mu_t)^2 + \lambda \sum_{t=1}^{T-1} \left((\mu_{t+1} - \mu_t) - (\mu_t - \mu_{t-1})\right)^2.$$

The value of the parameter λ governs the smoothness of the trend. When output is the variable being filtered λ is typically set at $1,600$ since this implies a business-cycle frequency of around $7\frac{1}{2}$ years. The MVTF's Consultative Document considered a range of values for this parameter: $\lambda = 1,600 = 1^4 \cdot 1,600$; $\lambda = 25,000 = 2^4 \cdot 1,600$; $\lambda = 125,000 = 3^4 \cdot 1,600$; and, $\lambda = 400,000 = 4^4 \cdot 1,600$, which as pointed out in the document, are equivalent to the credit cycle having, the same, double, triple, and quadruple the length of the business cycle, respectively. It is worth noting that as $\lambda \to \infty$ the process for $\{\mu_t\}_{t=0}^T$ approaches a linear trend, like that given by equation (2). Thus, lower values of λ allow the trend of the credit-to-GDP ratio, $\{\mu_t\}_{t=0}^T$, to have more variability than would arise from a linear trend.

It is helpful to note here in the context of describing our unobserved components models that given an estimated state-space model we can generate two possible estimates of our state variables $\{\mu_t, z_t\}_{t=0}^T$. We can generate estimates of μ_t and z_t that depend only on data up to that point in time (*i.e.*, data from period 0 to period t) or we can generate estimates that depend only the whole timeseries of that we have (*i.e.*, data from period 0 to period T). The first approach generates what are called *filtered* estimates of μ_t and z_t, also known as one-sided estimates, while the second approach generates what are called *smoothed* estimates, also known as two-sided estimates. Clearly, for the last period of any sample, the filtered and smoothed estimates will be the same. Because the smoother uses all of the information available to estimate the paths of our models' state variables it provides our best estimate of what the credit-to-GDP ratio gap was some past specified point in time.

1.3 Frequency detrending methods

Frequency detrending methods view economic time series as being the weighted sum of periodic functions of the form $\cos(\omega t)$ and $\sin(\omega t)$, where ω denotes a particular frequency; that is,

$$c_t = \int_0^{\pi} \alpha(\omega) \cdot \cos(\omega \cdot t) d\omega + \int_0^{\pi} \delta(\omega) \cdot \sin(\omega \cdot t) d\omega.$$

Cycles are then fluctuations with a specified range of periodicities or frequencies, which are inversely related according to frequency $= 2 \cdot \pi/$periodicity.

We should note that the HP filter can be thought of as an approximation to a frequency-based filter that passes though the higher-frequency fluctuations in a series to the cycle while removing the series' lower frequency fluctuations; that is, its trend. These filters are typically called "high-pass" filters. For example, as documented by King and Rebelo (1993), the standard HP filter with $\lambda = 1600$ approximates a frequency-based filter that passes through to the cyclical component of the series periodicities that range up to 32 quarters in lenghth and, thus, frequencies that exceed

$2 \cdot \pi/32$. (HP filters with other values of λ can be similarly approximated.) Thus, the cyclical component of the HP filter with $\lambda = 1600$ is approximately the same as:

$$z_t = \int_{\pi/16}^{\pi} \alpha(\omega) \cdot \cos(\omega \cdot t) d\omega + \int_{\pi/16}^{\pi} \delta(\omega) \cdot \sin(\omega \cdot t) d\omega.$$

Another class of frequency-based filters are "band-pass" filters, which pass through higher-frequency fluctuations, albeit only up to some point. This means that very high frequency fluctuations—such as, fluctuations that might reflect residual seasonality in the data—are also extracted from the cyclical component along with the low frequency—that is, trend—movements. In considering band-pass filters, we follow the MVTF's approach of examining a range of values for the HP filter parameters and allow for a range of periodicities to represent the credit cycle. Specifically, we first considered periodicities in the band of 6 to 30 quarters ($7\frac{1}{2}$ years). This implies a credit cycle of about the same length as the business cycle and implies that z_t, the cyclical component of the credit-to-GDP ratio, is given by:

$$z_t = \int_{\pi/15}^{\pi/3} \alpha(\omega) \cdot \cos(\omega \cdot t) d\omega + \int_{\pi/15}^{\pi/3} \delta(\omega) \cdot \sin(\omega \cdot t) d\omega.$$

We also considered periodicities in the band of 6 to 60 quarters (15 years), which imply frequencies in the band of $\frac{\pi}{30}$ to $\frac{\pi}{3}$, and 6 to 90 quarters ($22\frac{1}{2}$ years), which imply frequencies in the band of $\frac{\pi}{45}$ to $\frac{\pi}{3}$. These result in credit cycles that are two and three times the length of the business cycle.

Extracting the exact values of these specified periodicities from the data requires a two-sided infinite-order moving-average of the data. Clearly, this is not implementable in practice such that an approximate filter is required. We use the Baxter-King filter where, following Staiger, Stock, and Watson (2001), we set the size of the two-sided moving-average to 80 quarters to reflect the considerable persistence in the series.

2 Data and real-time concepts

2.1 Real-time data

As was noted at the beginning of section 1 we follow the MVTF's Consultative Document in defining the nominal credit-to-GDP ratio. That is, nominal credit is the volume of credit market debt outstanding of the non-financial corporate business sector and household and nonprofit organization sector as reported by the FRB in the Flow of Funds Accounts (FOFA) while nominal GDP is the measure reported by the BEA in the National Income and Product Accounts (NIPA).

Statistical agencies, like the BEA, revise data regularly. Most frequently, these revisions are only to the most recent observation of data, although at least once a year annual re-benchmarking of the data are performed and seasonal-adjustment estimates are re-calculated and this typically results in revisions to the last two or three years of data. In addition, statistical agencies generally perform comprehensive revisions every five year or so. These revisions can change the entire history of the series, especially if new methodologies form part of the comprehensive revision. Revisions to the underlying data used to calculate credit-to-GDP ratios are one reason why resulting gap estimates can change. Studying this source of revision therefore requires that we obtain real-time *timeseries* for our nominal credit and nominal GDP series. Obtaining real-time timeseries for nominal GDP (or GNP) is straightforward with many electronic sources available. In our case we take the real-time GDP/GNP timeseries from "ALFRED," which is an Archive of Federal Reserve Economic Data maintained by the St Louis Fed. These real-time timeseries are available electronically for a long time—*i.e.*, timeseries vintages date back to the late 1940s. Real-time timeseries vintages for nominal credit are a little more difficult to come by and indeed is not available in any data archive like ALFRED. Electronically, vintages of these timeseries, which we obtained from our colleagues, are only available to 1995:Q2.

Both the credit and GDP series are quarterly, but because they are obtained from different sources the timing of their releases—relative to the data's reference quarter—will be different. In particular, with GDP (and the NIPA) for any quarter of data there are three estimates that are released: The first release, released one month after the reference period; the second release, released two months after; and, the third release, released three months after.[4] In this case, it is only the last quarter of data that changes with each release, and after the third release the quarter's data remains unchanged until the NIPA annual revisions. At this point the estimate—along with the last three years of data—is revised as a result of annual benchmarking and revisions to seasonal factors. The estimate then continues to be revised in the next two annual revisions, after which it is revised whenever comprehensive revisions takes place. With credit (and the FOFA) there is only one release of the data and this is usually about two and a half months after the reference period. Like the NIPA there are annual revisions to the FOFA as well as revisions that occur following NIPA revisions.

In calculating the nominal credit-to-GDP ratio we use for nominal GDP only the timeseries vintages corresponding to the third release of GDP. Given the different timings of the GDP and

[4]The differences between these releases stems from the availability of source data. In the first release a few key items GDP source data are unavailable and are therefore estimated by the BEA. In the second release almost all of the timely source data for GDP are available. In the third release all of the timely source data for GDP are available; the only unavailable data series are those which are annual and get folded in during annual revisions. Until recently the three releases of the NIPA were referred to as the advance release, preliminary release, and final release. We use the new terms because in the context of this paper the term "final" is a bit confusing.

credit data releases, this seems to be the most likely choice of data that bank supervisors would use. Consequently, in our analysis we do not use all of the timeseries vintages of GDP data contained in the ALFRED archive; rather, we just use the third releases.

To be clear a vintage of data corresponds to the whole timeseries of the data. Panel B of Figure 1 plots five different vintages of the nominal credit-to-GDP ratio; specifically, the 1995:Q2 vintage, the 1998:Q4 vintage, the 2002:Q2 vintage, the 2005:Q4 vintage, the 2009:Q2 vintage, and the 2010:Q4 vintage. The name assigned to each vintage corresponds to the last data observation in the series.

As can seen from the panel B of Figure 1 all of the vintages plotted are different—although only slightly so. Thus, based on observing the various vintages of the credit-to-GDP ratio one might be inclined to conclude that real-time measurement issues of the credit-to-GDP ratio gap are unlikely to be large. The results of this paper will demonstrate that this is not the case: Real-time measures of the credit-to-GDP ratio gap revise substantially, although—consistent with what is apparent from the figure—the major source of revision is not data.

2.2 "True" real-time estimates of the nominal credit-to-GDP ratio gap

Panel B of Figure 1 plots five of the different vintages of the the nominal credit-to-GDP ratio; in total there are sixty-three. To obtain the true real-time estimates of the nominal credit-to-GDP ratio gap we first apply our filtering methods (described in subsections 1.1, 1.2, and 1.3) to each of the sixty credit-to-GDP ratio series so as to calculate the timeseries of gaps for each vintage. We then take the last observation of each timeseries and combine it into a single series.[5] For the purposes of example, panel A of Figure 2 shows in blue the true real-time estimate of the credit-to-GDP ratio gap derived using the HP filter with $\lambda = 400,000$. The other lines then show the gap timeseries for each of the above five data vintages (1995:Q2, 1998:Q4, 2002:Q2, 2005:Q4, 2009:Q2, and 2010:Q4), which as can be seen have their last observations plucked out to form the true real-time gap series. Other data points in the true real-time gap series correspond to the last observations of other data vintages, which are not shown in the figure so as to not complicate it.

[5]To some researchers an estimate—such as, in this case the credit-to-GDP ratio gap—is not real-time unless it was actually constructed at the time of the reference period. This is the case even if real-time data is used. Thus, to some researchers calling the estimates derived in this section true real-time is inappropriate. We recognize this point but for the purposes of distinguishing the estimates of this section with those of subsequent sections we continue with this terminology.

2.3 "True" final estimates of the nominal credit-to-GDP ratio gap

The true final estimate of the gap uses the full sample of the most the recent vintage of data available, in our case 2010:Q4. In the case of the deterministic models described in subsection 1.1, equations (2), (3), and (4), and the cubic spline model are estimated using (entire) the most recent vintage of credit-to-GDP ratio data and based on these estimates (and the actual data) the gap series are then obtained. There are no parameters to be estimated in the unobserved components models described by equation (8) in subsection 1.2. In this case, the true final estimate of the gap is the smoother (or two-sided) estimate of the trend and the corresponding gap. There are also no parameters to be estimates when frequency detrending methods are employed. The true final estimate of the gap here uses—where available—subsequent periods' observations in the moving average calculations that yield the cyclical component of the credit-to-GDP ratio in any period.

2.4 "Quasi" real-time estimates of the nominal credit-to-GDP ratio gap

As noted in the introduction there are several reasons why the real-time estimates of the gap calculated in subsection 2.2 should differ from the final estimates calculated in subsection 2.3 and only one of these is revisions to real-time data. In order to gauge the role of data revisions alone we calculate quasi real-time estimates of the gap. These gap estimates are calculated in a similar way to those reported in subsection 2.2 but instead of applying our filtering methods to each of our sixty-three credit-to-GDP ratio timeseries vintages as we did there, we apply our filtering methods to credit-to-GDP ratio timeseries taken from our final data vintage with a rolling endpoint set equal to the period for which the gap is being calculated. We then take the last observation of each timeseries, as we did in subsection 2.2, and combine it into a single series. Thus for the estimate of the gap in any period we are only using data up to that point in time, although we are using currently data available (not the data actually available at that time).[6]

[6] Orphanides and Van Norden consider a fourth real-time/final gap-estimate concept called the quasi final estimate. This type of gap measure is only really relevant for gaps that are derived from unobserved components models. This gap measure differs from the true final gap estimate (described in subsection 2.3) in that while it also uses the final vintage of data and estimates the model using the entire sample period it is generated as the filtered (that is, one-sided) estimate of $\{z_t\}_{t=0}^{T}$ rather than the smoothed (that is, two-sided) estimate. Note, however, that for the HP filter there is no difference between quasi final and quasi real-time gap estimates since all parameters in this model are calibrated. Consequently, we do not include an additional chart for this gap concept. The only circumstance under which the quasi final and quasi real-time gap estimates will differ is if the unobserved components model includes parameters that require estimation.

3 Results

Panel B of Figure 2 plots the true real-time estimates for our credit-to-GDP ratio gap measured using all of the detrending methods described in section 1. As noted, we can only do this for a limited timeseries (since 1995:Q2) because we do not currently have real-time FOFA data vintages for earlier periods. Panel C plots the true final estimates for our credit-to-GDP ratio gap measured using all of the detrending methods described in section 1. We show this for the sample period 1980:Q1 to 2010:Q4. Panel D plots the quasi real-time estimate, which we can also derive over the 1980:Q1 to 2010:Q4 sample period. With the exception of the cubic spline, the different methods for estimating the trend of the credit-to-GDP ratio yield gaps that display broadly similar contours. The magnitudes of the gaps are, however, very different, although that is to be expected given that in some cases the parameters of different filtering methods were set to allow more-or-less of the credit-to-GDP ratio's fluctuations through to the cyclical component.

We begin this section by first considering the sources of revision to the real-time credit-to-GDP ratio gap and then examining the magnitudes of the revisions. We then investigate the extent to which revisions could result in different policy actions being taken. After this we look into how quickly revisions tend to be realized; a question that could be a concern given that banks have a year in which to build their capital buffers once supervisors call for their deployment. Finally, we discuss reasons for the revision results we obtain from different filtering methods.

3.1 Revision sources

The six panels in Figure 3 plot revisions to the credit-to-GDP ratio gap. The three panels in the upper row of the figure plot the total revision, that is, the difference between the real-time estimate—described in subsection 2.2—and the final estimate—described in subsection 2.3. The three panels in the lower row plot the difference between the quasi real-time estimate—described in subsection 2.4—and the final estimate—described in subsection 2.3. The different columns in the figure represent different filtering methods.

Because we only have real-time vintages for the FOFA extending back to 1995, we can only plot the real-time to final estimate revisions extending back to that date. For the quasi real-time to final estimates we can, however, plot a much longer timeseries. A comparison of the real-time to final and quasi real-time to final revisions over the sixteen-year sample period over which a comparison can be made indicates fairly similar looking revisions both in terms of size and contour. Recall from subsection 2.4 that the reason why we constructed the quasi real-time estimate was that revisions to the underlying data are only one of the reasons why estimates of the gap might change. Even without revisions to past data, the availability of data for later periods alters our estimates of where the trend credit-to-GDP ratio was at some past point in time and therefore what the gap was. Observing the revisions between the quasi real-time and the final estimates—and comparing them to the revisions between the true real-time and final estimates—allows us to separate out the portion of the overall revision to the gap that stems from *revisions* to the data and the portion that stems from the *availability* of data for later periods revising our estimates of the trend credit-to-GDP ratio (at any point in time). In this case we can see from Figure 3 that the main source of revisions to estimates of the credit-to-GDP ratio gap is not due to revisions to the underlying data but rather is due to revisions stemming from the unreliability of end-of-sample estimates of the trend. This is an important result for understanding the reasons for the unreliability of credit-to-GDP ratio gaps in real-time and, in particular, appreciating that simply looking at various vintages of the credit-to-GDP ratio and observing that they do not revise that much does not imply that the gap will not revise substantially. We would note, however, that the spirit of this result has been documented for macroeconomic series; see, Orphanides and Van Norden, 2002.

For the analysis that follows, the result that almost all of the revisions to the credit-to-GDP ratio gap stems from problems with end-of-sample trend estimation (and not from data revisions) means that we can concentrate our discussion on the quasi real-time to final revisions—for which we have a longer sample period—without concern that we are missing a sizable source of revision. We consider now the magnitude of the quasi real-time to final revisions shown in lower panels of Figure 3.

3.2 Revision magnitudes

A visual comparison of the lower panels of Figure 3 and the credit-to-GDP ratio gaps shown in Figure 2 indicates that with the exception of the linear trend model, which has extremely large gaps towards the end of the sample, the revision magnitudes are on about the same order as the magnitudes of the credit-to-GDP ratio gap themselves. This fact is also evident in Table 1, and the upper halves of Tables 2 and 3, which report key summary statistics for the gap estimates and for their quasi real-time to final revisions.

Table 1 reports the mean, standard deviation, and minimum and maximum values for the final and quasi real-time gaps implied by all of the filtering methods over the period 1980:Q1 to 2010:Q4. Note that the sample period for the data begins about 25 years earlier, in 1954:Q1, such that the means shown in the table (calculated over a shorter sample period) need not equal zero. The upper half of Table 2 reports similar statistics for the revision between the quasi real-time and final gap estimates. Also included are estimates for the persistence of the revisions, which as can be seen are very high.

The upper half of Table 3 considers the reliability of the quasi real-time gap estimates. The first two columns follow Orphanides and Van Norden's approach to quantify the earlier visual comparison concerning the magnitudes of the revisions—*i.e.*, the "noise" of the estimates—relative to the magnitude of the (final) measure itself—*i.e.*, the "signal" from the estimates. The first column reports the ratio of the standard deviation of the revision to the standard deviation of the credit-to-GDP ratio gap. The second column reports the ratio of the RMSE of the revision to the standard deviation of the credit-to-GDP ratio gap. The difference between these two measures is that the latter reflects biases in the quasi real-time estimate—that is, persistent revisions in one direction or the other—which the other measure excludes. For the most part, however, there is only a notable difference between these two noise-to-signal ratios for the gaps for which the trend follows either a linear or a quadratic process.

Consistent with the visual comparison of Figures 2 and 3, the noise-to-signal ratios of the gap estimates are high. That is, with the exception of the gap estimates implied by the linear trend and by the quadratic trend, the noise for the gap estimate in real-time is about 75 percent to 150 percent the size of the signal. And for the gap estimates for which the noise-to-signal ratio appears to be relatively low, it is not primarily the standard deviations of the revisions that are smaller but rather it is the fact that the gap is so large that drives this outcome.

The remaining columns in the upper half of Table 3 gauge how different the signals implied by the quasi real-time and the final gap measure are. The third column reports the correlations between the quasi real-time and final gap measures and the fourth column reports the proportion of the time that the gap estimates take on different signs. With the exception of the gap implied by the linear trend, the quadratic trend, and perhaps also the HP filtered trend with $\lambda = 400,000$ the correlations between the quasi real-time and final gap are relatively low; that is, on the order of 0.35 to 0.7, where this excludes the cubic spline for which the correlation is negative. The proportion of the time that the quasi real-time and final gaps have opposite signs is also quite high—on the order of 25 to 40 percent of the time for most gap estimates. Gap estimates have the opposite sign relatively less frequently for the linear trend—on the order 10 percent of the time—but have the opposite sign relatively more frequently for the cubic-spline trend—on the order of two-thirds of the time.

3.3 Revisions and different *ex-post* policy actions

For the purposes of using the credit-to-GDP ratio gap to guide macroprudential policy, an important question is that of how accurately the gap signals to supervisors in real-time that they should be requiring banks to accumulate countercyclical capital buffers. To look at this question we examine how frequently it is the case that quarters in which the credit-to-GDP ratio gap is found in quasi real-time to lie in the upper parts of the series' distribution correspond to the quarters in which the final credit-to-GDP ratio gap is found to lie in the equivalent upper part of final gap series' distribution.

Explaining how we answer this question is perhaps best described by considering Figure 4, which shows our approach graphically for four of our detrending methods—specifically, the quadratic trend and the HP filtered trends with λ values set equal to 25,000, 125,000, and 400,000. The thick-solid and the thin-solid black lines shown in each panel of Figure 4 are the quasi real-time and final gap, respectively, implied by each filtering method. (These are the same gaps that were shown as a group in panels C and D of Figure 2.) The thick-dashed black line shows for each detrending method what in quasi real-time would have been considered the 90th percentile credit-to-GDP ratio gap. We calculate the 90th percentile credit-to-GDP ratio gap iteratively and in the same way that the quasi real-time gaps were calculated. Recall that these are calculated by estimating the gap timeseries over iteratively longer samples of the final data and then combining into a separate timeseries the very last period of each iterative timeseries. In the same way that we take the last observation of each gap timeseries to obtain the quasi real-time gap series, we can also take the 90th percentile (or any percentile) of each gap timeseries and this is what is plotted by the thick-dashed line in each of the panels of Figure 4. Because the associated timeseries of gaps changes with each additional period added to the sample, this 90th percentile series does change somewhat over time. The thin-dashed line is the 90th percentile of the final credit-to-GDP ratio gap. This is the estimate of the 90th percentile credit-to-GDP ratio gap based on all information available up to the end of 2010.

If we take the 90th percentile of the credit-to-GDP ratio gap to be the level at which supervisors would deploy countercyclical capital buffers—which is consistent with the more frequent extreme of the 10- to 20-year incidence described in the MVTF's Consultative Document—we can ask in which periods in quasi real-time would countercyclical capital buffers have been in place.[7] In Figure 4 these periods would be the quarters in which the thick-solid line—representing the gap measure—exceeds the thick-dashed line—representing the 90th percentile. We can then ask for what proportion of these times do we also find using the final data that the gap exceeds the 90th percentile. The second last column of Table 3 shows this percentage, which in some cases in near zero and never gets above 40 percent of the time. The last column of Table 3 ask the question in the opposite direction; that is, for what proportion of the times that the final gap is found to exceed the 90th percentile was the quasi real time gap also found to be in the 90th percentile. Here we find higher numbers for all but the gap measures generated by the band-pass filter approach. This

[7]We should note that using a percentile of the gap series as a threshold for deployment was not suggested by the MVTF's Consultative Document. Rather the Consultative Document indicated a credit-to-GDP ratio gap threshold of around 2 percent; albeit adjusted in some way depending on the filter being used. We viewed a set percentile to be a convenient way to define the threshold at which countercyclical capital buffers might be deployed given the different ranges of the various ratio-gap estimates. We think of this approach as being somewhat akin to the adjustment the MVTF suggested for different filters, although the threshold implied by the 90th percentile is generally several percentage points higher than 2 percent.

indicates that for the deterministic and unobserved components detrending methods, reacting to apparently excessive levels of credit that later turn out to be "false positives" is a greater problem than missing—and thereby failing to react when—credit levels are excessive. With the band-pass filter, however, the problem of failing to react in quasi real-time to excessive credit levels appears to be a greater concern.

3.4 Timing of the different *ex-post* policy actions

As is evident from Table 3 and Figure 4 there are a few occasions when in quasi real-time it would appear that the credit-to-GDP ratio gap is very high—in our case defined to be in the upper 90th percentile of the series distribution to date—but when the entire data sample is used to calculate the gap this no longer appears to be the case. For the four detrending methods that we consider in Figure 4 this appears to occur around the 2001 to 2003 period. Figure 5 summarizes the results for all of the filtering methods that we consider. To do this, in the upper panel we assign a value of one to all periods in which the final gap estimate has a value that puts it above the 90th percentile of the historical distribution of gaps. Then for each period, we sum (or stack) up the different filtering methods that place the final gap in the 90th percentile. In the lower panel we do the same thing for the quasi real-time gap. In a sense the information contained in Figure 5 reflects that contained in the last two columns of Table 3, although the information in Table 3 is harder to infer from Figure 5 and Figure 5 also gives an indication of at what points in time the credit-to-GDP ratio gap lies in the upper part of the gap series' distribution. Clearly, the 2001 to 2003 period is a point at which many filtering methods indicate that countercyclical capital buffers should have been deployed when the analysis is conducted in quasi real-time (the lower panel) but not when the analysis is undertaken using the full sample of data (the upper panel).

One problem that has been noted with regard to using elevated credit-to-GDP ratio gaps to signal the deployment of countercyclical capital buffers is that this ratio can rise in economic downturns purely due to the fact that GDP typically declines sooner than credit (if, indeed, credit declines at all).[8] Given that this is a well documented way in which the credit-to-GDP gap can be misleading, supervisors would likely, before deploying a countercyclical capital buffer, check that it was not the case that the credit-to-GDP ratio gap was high for this reason. In Figure 6 we use the Congressional Budget Office's February 2011 estimate of the path of potential GDP to calculate quasi real-time and final credit-to-*potential*-GDP ratio gaps, similar to in Figure 4, and also calculate the 90th percentiles of the resulting gap series. In Figure 7 we generate plots like

[8] The MVTF's Consultative Documents, for example, discusses this as a concern as do Repullo and Saurina (2011) who document that for a number of countries the credit-to-GDP ratio tends to be high when GDP growth is low and vice versa.

those of Figure 5 for credit-to-potential-GDP ratio gaps.[9]

We see from Figures 6 and 7 that in quasi real-time fewer filtering methods suggest a deployment of countercyclical capital buffers in 2001 to 2003 when the credit-to-potential-GDP ratio gap is considered instead of the credit-to-GDP ratio gap. This suggests that some of the increase in the credit-to-GDP ratio in this period may indeed reflect slower GDP growth. That said, several filtering methods do still suggest the deployment of buffers in 2001 and 2003 indicating that using the credit-to-potential-GDP ratio does not fully resolve the issue. Note, also, that when the credit-to-potential-GDP ratio is considered, gaps generated by some filtering methods start to call for countercyclical capital buffers earlier; for example, in 1999 and 2000. This reflects the fact that GDP in the U.S. was about 2 to 4 percentage points above potential at this time thereby implying an equivalently higher credit-to-potential-GDP ratio over these years.

In section 4 we will consider the implications for lending of supervisors deploying countercyclical capital buffers at various points in the 1999 to 2003 period based on real-time estimates of the credit-to-GDP ratio gap. Because we know *ex post* that these were not times when credit-to-GDP ratios were excessive and that countercyclical capital buffers should not have been deployed then, this exercise allows to gauge how costly to the economy the unreliability of these gap measure can be.

3.5 Revisions realized within the year

The MVTF's countercyclical capital buffer proposal gives banks one year to accumulate capital and thereby meet additional requirements. Given this timing along with the magnitude of the revisions of quasi real-time to final estimates of the credit-to-GDP ratio gap, policymakers may be concerned by the extent to which these revisions are realized within the one-year timeframe. If the estimates of the credit-to-GDP ratio gap in a given period revise substantially within a year, it may weaken supervisors' influence for deploying countercyclical capital buffers. The lower parts of Tables 2 and 3 report statistics that look at this question; we focus our discussion on the results in Table 3.

The first two columns of the lower part of Table 3 indicate that revisions to the quasi real-time gap over the first year after the estimate is made are moderate for most filtering methods. That is, for most filtering methods it would seem that the revision over the first year is about one-third of the ultimate revision. The exceptions, however, are the HP filter trend with a λ value set equal to $1,600$ and the band pass filters that pass through periodicities in the range 6 to 30 periods and 6 to 60 periods. For these filtering methods about two-thirds of the ultimate revision occurs within

[9]We should note that there is something a bit perverse about trying to find the trend of the credit-to-potential GDP ratio series given that potential GDP is itself a trend series that suffers from large revision problems. We do not dwell on this issue, however, given that we are using potential GDP here mainly to check that a decline in output is not the reason the gap calls for a deployment of countercyclical capital buffers.

the first year. Unsurprisingly, as shown in the third column, the correlation between the quasi real-time gap estimated in any quarter and its estimate one year later is also quite a bit lower for these three filtering methods—*i.e.*, equal to 0.7 or lower—relative to the other methods for which the correlation is reasonably high—*i.e.*, 0.9 or higher. In addition, the frequency of opposite-signed gap estimates, shown in the fourth column, does not drop-off as dramatically with these three filtering methods in moving from the full to the one-year revision horizon.

The second last column of this part of Table 3 addresses the issue of whether quarters in which the credit-to-GDP ratio gap estimated in quasi real-time appears to call for a deployment of countercyclical capital buffers, continues to indicate this, for the same quarter, one year later. Again we take the gap exceeding the 90th percentile to correspond to periods in which supervisors would want to deploy countercyclical capital buffers. As reported in this column, we find that for the majority of the filters considered, when the credit-to-GDP ratio gap is found in any quarter to lie in the 90th percentile of the gap distribution, less than 50 percent of the time does this continue to be case one year later. The last column considers how the credit-to-GDP ratio gap when measured with a one-year lag and found to have lied above (the current estimate of) the 90th percentile one-year earlier compares with where in the distribution the gap is found to lie in quasi real-time. Here we find that for the deterministic and unobserved components detrending methods "false positives" are a larger problem, while missed responses appear to be a greater concern for the band-pass filtering method.

3.6 Revisions and different filtering methods

The relationship between the choice of filtering method and the size and timing of revisions can be summarized as follows. First, the credit-to-GDP ratio gaps obtained from the linear and—to a lesser extent—quadratic detrending methods yield the smallest revisions. Second, deterministic detrending methods with higher-order polynomial trends imply larger gap-estimate revisions while unobserved component and frequency detrending methods that allow for longer credit cycles imply smaller gap-estimate revisions. Finally, gap estimates implied by the frequency detrending methods exhibit a much larger portion of their revisions quite quickly—that is, within the course of a year—relative to other filtering methods.

The finding that credit-to-GDP ratio gaps implied by linear and—to a lesser extent—quadratic detrending methods have the smallest revisions reflects the problems that other detrending methods have at the endpoints of a series. These end-point problems arise because both unobserved-components and band-pass filtering methods estimate the trend and cycle of a series using past and current values of a series, as well as whatever future observations are available. Toward the end of the sample period fewer future observations are available, which can result in large revisions when data do become available.

The Baxter-King filter, which is implemented as a symmetric two-sided moving average of the actual data uses an estimates AR process to "pad" the sample period with backcasts and forecasts of the series to be filtered. As additional quarters of actual data become available, the future-period observations and forecasts used in the filter also change, potentially resulting in large revisions to the gap and trend estimates. Of course, the degree to which these estimates of the trend and cycle will change depends on the weights in the moving average assigned to the quarters for which new data and revised forecasts become available.

The HP filter, which can also be expressed as a two-sided moving average of the actual data, does not pad the sample period with backcasts and forecasts but instead applied different weights at different points in the sample period. That is, if we are calculating the cyclical component of a series for the first period of the sample, the coefficients in the moving average applied to the actual data will be different from the coefficients applied for the second period in the sample, the third period in the sample, and so on. The amount by which the moving average coefficients change from one observation to the next depends on where in the sample the observation lies: At the beginning and end of a sample the moving average weights differ greatly from one observation to the next, while in the middle of a large sample the moving average weights change little. As Baxter and King (2001) discuss, the moving-average weights for an HP filter with $\lambda = 1,600$ only settle down after about three years.[10] This yields significant instability in our real-time estimates of the credit-to-GDP ratio's trend and associated gap measure.

Deterministic detrending methods model the trend as the fitted value of an estimated polynomial function of time, with the cyclical component defined as the residual between the series and the trend. Here, additional quarters of data result in revisions to the trend and the cycle through their effects on the estimated coefficients of the polynomial. In principle, additional quarters of data should alter the trend and cycle only very modestly in large samples because additional observations should yield only slight changes in parameter values. The results in Tables 2 and 3 indicate that this is in fact the case for credit-to-GDP ratio when we use linear or quadratic detrending. However, when we model the trend as a cubic function of time, the real-time reliability of the resulting gap measure is among the poorest of all the filtering methods we consider. This in turn reflects overfitting at the endpoint: As can be seen from the plot of the credit-to-GDP ratio in Figure 1, there is a large run-up and subsequent decline in the series over the early 1980s to early 1990s period. A higher-order polynomial initially attempts to fit this bulge with a small increase in the trend at the end of the sample. As more data become available and the run-up starts to

[10]Baxter and King also document that in the early and late parts of the sample the HP filter is not a good approximation to the high-pass filter (see subsection 1.3, above). Three years into the sample and three years from the sample's end, however, the HP filter is a better approximation.

reverse itself, the cubic polynomial calls for a flatter trend, which in turn implies large revisions to the gap. The reason this does not happen for the linear and quadratic trends is that since they never attempt to fit the bulge in the credit-to-GDP ratio; hence their parameter estimates change relatively little around this episode and smaller revisions obtain. This underscores the sensitivity of even relatively low-order polynomial detrending procedures in real-time when the actual series exhibits persistent but ultimately transitory movements.

The cubic spline has the largest revisions of all the methods we consider. This reflects both the problems faced by cubic polynomial detrending, as well as the fact that the estimation intervals for the segments of the spline can be quite small despite a large available time-series of data. Specifically, our time-series of 27 years at the start of our real-time analysis and 57 years over the complete sample translate into spline segments that initially span 9 years of data and eventually span 19 years. Consequently, additional quarters of data can have significant effects on the trend and result in large revisions to the estimated gap.

We would note that although the linear and quadratic trends exhibit the smallest revisions, they are not necessarily the best techniques to use for estimating the credit-to-GDP ratio gap. If there is a stochastic trend in the data (which seems likely given the results of the unit root tests from section one), then deterministic detrending methods are not defensible on economic grounds and will generate spurious cycles. Although the focus of this paper is on revisions to the various gap estimates, any practical attempt to use a credit-to-GDP ratio gap to guide countercyclical capital policy would require some consideration of issues such as the nature of the trend (deterministic or stochastic) and thus the most appropriate filter to use.

Another feature of the estimated revisions implied by the various detrending procedures is that the unobserved-component and frequency-detrending methods that allow for longer credit cycles imply smaller revisions to the implied gap measures. For the HP filter this arises because longer assumed credit cycles imply a smoother path of the trend (this is associated with a larger penalty on changing the slope of the trend in the HP filter optimization problem—see subsection 1.2). This also means that additional observations will have a smaller effect on the estimated cyclical component with correspondingly smaller revisions to the gap measure. The intuition for the Baxter-King filter is similar: Allowing for longer credit cycles implies that a smaller range of low-frequency fluctuations will be extracted in constructing the cycle; additional observations will therefore have smaller effects on the low-frequency component of the series, in turn implying smaller revisions to the cyclical component.

Finally, the result that a larger share of revisions occur within a year is we use filtering methods with shorter credit cycles (*e.g.*, the HP filter trend with a λ of $1,600$ and the bandpass filters that pass through periodicities in of range 6 to 30 quarters) reflects the fact that smaller amounts of data are needed to change the trend estimates for these filtering methods. Similarly, the different ways that the HP and Baxter-King filters deal with end-point problems determines the rapidity with which the bulk of the revisions will occur. As noted earlier, it takes about three years before the moving average weights associated with the HP filter settle down; hence, a reasonable fraction of revisions to the gap occur more than one year after the reference quarter. Revisions to the Baxter-King gaps occur more quickly because the largest moving average weights are on observations that are just a couple of quarters before and after the current observation. Consequently, the largest revisions for the Baxter-King gaps occur within a year.

4 Real implications for the use of countercyclical capital buffers

The results of section 3 underscored a key practical difficulty associated with countercyclical capital buffers; specifically, the tendancy for credit-to-GDP ratio gaps to yield "false positives" in terms of indicating in real-time excessively high levels of credit. We now turn to consider the likely real economic costs of incorrectly deploying countercyclical capital buffers based on unreliable real-time estimates of the credit-to-GDP ratio. We focus on the reduction in lending that would have obtained were countercyclical capital buffers to have been deployed in 1999:Q3, 2001:Q4, and 2003:Q2—the dates highlighted in section 3 as being those for which a number of detrending methods yielded false positives. The effect of countercyclical capital requirements on lending in these quarters depends on a number of considerations including: (i) the extent that countercyclical capital requirements would be binding in these quarters; (ii) the capital shortfalls that countercyclical buffers would imply, (iii) the effect of increased capital requirements on the level of lending, and (iv) on the implications of risk weights for lending when banks increase their capital ratios.

4.1 The extent that countercyclical capital requirements bind

We begin this subsection with a brief overview of the Federal Deposit Insurance Corporation's (FDIC) definition of what constitutes a well capitalized, adequately capitalized, and under capitalized bank and the supervisory implications of a bank being in one of these categories. Banks are well capitalized if their total risk-based capital ratio is at least 10 percent, their Tier 1 risk-based capital ratio is at least 6 percent, and their leverage ratio is at least 5 percent and they are adequately capitalized if they are not well capitalized and their total risk-based capital ratio is at least 8 percent, their Tier 1 risk-based capital ratio is at least 4 percent, and their leverage ratio is at least 4 percent.[11] Banks that fail to meet the adequately capitalized criteria are undercapitalized.

[11] A leverage ratio of at least 3 percent suffices if a bank's CAMELS rating is 1 and the bank does not experiences significant growth.

These three categories affect the banks' assessment by the FDIC; that is, their contribution to the deposit insurance fund. After the implementation of the Basel Accord banks moved quickly to improve their capital ratio to lower their FDIC assessment and signal markets that they have solid balance sheets.

As noted, our results in section 3 indicate that countercyclical capital buffers would likely have been in effect in 1999:Q3, 2001:Q4, and 2003:Q2. To consider what this would have meant for whether countercyclical capital buffers would have been binding, Figure 8 shows the distribution of risk-based capital ratios across all US banks for these three quarters. The left side of the figure shows the distribution unweighted across institutions and the right side of the page shows the distribution weighted by asset size. As can be seen from the left panel of Figure 8, nearly all banks meet the FDIC's definition of well capitalized in 1999:Q3, 2001:Q4, and 2003:Q2. Only a few banks have ratios smaller than 10 percent and many banks have significantly higher ratios. The right side of the figure shows the distribution of capital ratios weighted by the total assets of the bank, which is heavily skewed to the left. Two key observations follow. First, regulatory capital ratios are binding as banks sort themselves into the well capitalized bank category.[12] Second, large banks have capital ratios that are only slightly above the regulatory requirements.[13] In sum, changes in capital ratio requirements as would have been implied by credit-to-GDP ratio measures in 1999:Q3, 2001:Q4, and 2003:Q2 would have affected a *significant share* of the economy's bank capital, even if the changes would have affected only a *small number* of institutions.

4.2 The capital shortfalls that countercyclical capital requirements imply

With the data underlying Figure 8 we can also calculate how much capital the banking sector would have needed to have raised in 1999:Q3, 2001:Q4, and 2003:Q2 in order to have met the capital requirements imposed by countercyclical capital buffers. This calculation is done by simply determining for each quarter which banks would have had a shortfall of capital relative to the new countercyclical capital buffer requirements, calculating the dollar value of the shortfall for each bank, and summing these individual bank shortfalls together to obtain the shortfall for the entire

[12]Hanson, Kashyap, and Stein (2011) point out that during recessions regulatory capital requirements may not be binding constraints. In particular, they argue that markets, fearing a deterioration of banks' balance sheets, may be willing to fund strongly capitalized banks only. Figure 9 illustrates this point. The capital ratios started to increase with the decline of the stock market in 1999. During the 2001 recession aggregate regulatory capital ratios shot up. Therefore, capital requirements during good times need to exceed the capital ratio requirements of the market in a recessions to achieve the desired countercyclical effect. Hanson, Kashyap, and Stein (2011) suggest a range of 12 to 15 percent, well above the maximum capital requirements in the MVTF's Countercyclical Capital Buffer Proposal Consultative Document.

[13]These two facts are consistent with the view of Hanson, Kashyap, and Stein (2011), who argue that competition puts pressure on banks to reduce capital ratios. Smaller banks tend to have higher capital ratios but also appear to utilize a different lending technology such as relationship lending. See, for instance, Petersen and Rajan (1994) and Berger and Black (2010).

banking sector. The upper part of Table 4 shows the outcome of these calculations for a range of countercyclical capital buffer add-ons; specifically, for a $\frac{1}{2}$ percentage point, 1 percentage point, $1\frac{1}{2}$ percentage point, and 2 percentage point add-on. (We used a 2 percentage point upper range for the buffer because it was also used as the assumed maximum add-on in the MVTF's Consultative Document.)

It is well documented that banks operate with higher capital ratios than are required by regulation, with many factors—including the capital ratio that credit markets demand, managers' own views and preferences on risk management, and the regulatory and supervisory environment—accounting for this difference between the actual and regulatory-required capital ratios. (See Alfon, Argimon, and Bascunan-Ambros, 2004, for a clear discussion of this issue.) Thus, we also considered the amount of the capital shortfall in the banking system were banks to want to hold a pre-cautionary buffer in excess of the higher regulatory minimum. Specifically, we considered the capital shortfall were supervisors to deploy a 2 percentage point countercyclical capital buffers add-on and banks were to want to hold an additional $\frac{1}{2}$ percent or 1 percent of capital. These estimates are also shown in the top part of Table 4.

Our calculations (reported in Table 4) suggest the following range of estimates for the capital shortfalls implied by countercyclical capital buffers being deployed in 1999:Q3, 2001:Q4, and 2003:Q2. In 1999:Q3 the total capital shortfall of U.S. banks would have been between $1.8 billion for a capital ratio requirement of 10.5 percent and $67.7 billion for a capital ratio requirement of 12 percent plus a 1 percent pre-cautionary buffer. For 2001:Q4 and 2003:Q2 the respective numbers are $2.1 billion and $1.1 billion for a capital ratio requirement of 10.5 percent and $58.8 billion and $67.3 billion for a capital ratio requirement of 12 percent plus a 1 percent pre-cautionary buffer.

4.3 The impact of capital shortfalls on lending and interest rates

We now address the question of how the capital shortfalls reported in the upper part of Table 4 affect bank lending. There is a wide range of theoretically possible values for the effect. For example, one possible extreme is for a zero effect. This would be the case for a well capitalized bank—that is, banks with a capital ratio well above its regulatory requirements—or a bank with access to additional sources of capital, because, in principal, such a bank can withstand additional regulatory capital buffers without reducing lending. The other extreme of a bank that targets regulatory requirements and actively manages its assets. Without raising any new equity, such a bank would need to reduce assets (lending) quite substantially in response to an increase in the countercyclical capital buffer. For illustration, consider a bank that initially has a required capital ratio of 10 percent and a leverage rate of 10. If regulatory requirements were to increase by one percentage point to 11 percent, to achieve this new ratio the bank would need to reduce assets by

9.1 percent or $9.10 per dollar of equity, where 9.1 is the leverage rate, which equals the inverse of the new capital ratio.

The view that a capital shortfall of some dollar amount would result in a dollar reduction in assets equal to the product of the leverage rate and the shortfall was strongly advanced by Hatzius (2007, 2008) in the early years of the recent crisis: first in a note about the reduction in loan volumes that would result from banks' mortgage-related portfolio losses and later in a Brookings paper on the same issue. Hatzius' view was based on a scatterplot for aggregate leverage and commercial-bank asset growth from 1963 to 2006 in Adrian and Shin (2007), which (was reported for a different motivation in the paper but nonetheless) showed an almost constant leverage ratio. Clearly, adopting this view would lead to very substantial reductions in assets and loan volumes from the capital shortfall estimates that we obtain in Table 4.

We resist adopting this view of very substantial effects of capital shortfalls on lending (and thereby obtaining substantial costs from real-time gap mismeasurement) because, given the results from various estimated models of bank lending, we believe they are too large. Specifically, the empirical literature starting with Bernanke and Lown (1991) and Hancock and Wilcox (1993, 1994) finds only modest effects of bank capital on lending. Bernanke and Lown, using data for a cross-section of 111 New Jersey banks for the year-ended 1991:Q1, estimate a 2 to 2.5 percentage point increase loan growth for a 1 percentage point increase in capital ratios. Hancock and Wilcox, who estimate their model on dollar amounts over the year-ended 1991:Q1, find that a $1 capital surplus increases lending by $1.50. These numbers are significantly smaller than those that active asset management implies. More recently, Berrospide and Edge (2010) estimate the effect of bank capital on lending starting in 1996 and extending up to the 2008 crisis using bank holding companies data and confirm the modest effects of Bernanke and Lown (1991) and Hancock and Wilcox (1993, 1994). In particular, in estimating Hancock and Wilcox style regressions, Berrospide and Edge found that a $1 shortfall/surplus in bank capital resulted in a $1.86 reduction/increase in loans.[14]

The estimates of Berrospide and Edge (2010) results reported above focus on bank capital shortfalls/surpluses in general and not to capital shortfall after a change in regulatory capital requirements. Hancock and Wilcox (1993) do distinguish capital shortfalls relative to bank specific targets and capital shortfalls relative to regulatory requirements, which came about after the implementation of the Basel Accord. The latter have significantly larger effects on lending—

[14]Berrospide and Edge (2010) reconcile their findings with the scatterplots of Adrian and Shin (2007) scatterplot by differences in sample period. Splitting the sample at 1990, Berrospide and Edge show that the aggregate leverage ratio is almost constant before implementation of the Basel Accord but after the implementation, which is the sample they consider, the aggregate leverage ratio fluctuates significantly. Thus, the active asset management hypothesis—while evident in the pre-1990 period and strong enough to show through clearly in a scatterplot for the 1963 to 2006 sample—cannot be confirmed for the post-1990 period.

specifically a $1 regulatory capital shortfall leads to a $3.16 reduction in total lending.[15]

Given the broad range of effects of bank capital on lending, we construct a lower and upper bound for the implications of capital shortfalls—implied by countercyclical capital buffers being deployed in 1999:Q3, 2001:Q4, and 2003:Q2—on lending. The lower bound is simply the system-wide capital shortfall, excluding any pre-cautionary buffers, multiplied by the (low) estimate—as reported in Berrospide and Edge (2010)—that a $1 capital shortfall results in a $1.86 reduction in lending. Thus, the number $3.3 billion shown in Table 4 as the lower-bound lending decrease for a 0.5 percentage point countercyclical capital buffer add-on, is the product of the capital shortfall of $1.8 billion implied by regulation—shown in the top part of the table—and the Berrospide-Edge parameter estimate of $1.86. The upper bound assumes that in addition to the increase in the regulatory requirement, banks build-up a pre-cautionary buffer of one percentage point in excess of the new, higher regulatory minimum. Here, however, we apply different estimates of the effects of capital shortfalls on lending depending on whether the shortfall is a regulatory shortfall or is a shortfall relative to the banks' own desired capital target. That is, for regulatory shortfalls we use the estimates of a $3.16 reduction in lending for a $1 capital shortfall, as estimated by Hancock and Wilcox specifically for this type of shortfall. For the shortfall relative to the banks' own desired capital ratio we use the estimate of a $1.86 reduction in lending for a $1 capital shortfall, as estimated by Berrospide and Edge. Thus, the number $36.2 billion shown as the upper-bound lending decrease for a 0.5 percentage point countercyclical capital buffer add-on is the sum of two terms. The first is the product of the capital shortfall of $1.8 billion implied by regulation and the Hancock-Wilcox parameter estimate, $3.16. The second is the product of the capital shortfall implied by banks' own desired capital ratio—equal to $18.2 billion less $1.8 billion—and the Berrospide-Edge parameter estimate $1.86.

The results for all of the various capital-ratio changes we consider are shown in Table 4. As is evident, the effects of countercyclical capital buffers on lending can be substantial. For example, for an additional capital requirement of 2 percent points, the reduction in lending following 1999:Q3 could have been up to $167.1 billion. To put this in some context, in this quarter new mortgage originations to consumers were about $400.0 billion, new auto loan originations were about $75.0 billion, and C&I loan originations were some $144.6 billion in the first week of August 1999. Similarly, the range of $3.9 billion to $141.1 billion for 2001:Q4 can be compared to new mortgage originations of about $700.0 billion, new auto loan originations of about $100.0 billion,

[15] Francis and Osborne (2009) estimate Hancock and Wilcox style regressions for the U.K. and find estimates for the effect of capital shortfalls/surpluses on lending that lie between the estimate of Berrospide and Edge and the estimate from Hancock and Wilcox for shortfalls/surpluses from regulatory capital targets. Interestingly, because the U.K. has bank-specific capital ratio requirements, the capital shortfall/surplus measure that Francis and Osborne use in their model reflects regulatory requirements. Likely, this accounts for some of the difference between the effects found by Berrospide and Edge and Francis and Osborne; although, different country samples are likely also to be important.

and C&I loan originations were some \$87.5 billion in the first week of November 2001. Finally, the range of \$2.0 billion to \$162.1 billion for 2003:Q2 can be compared to new mortgage originations of almost \$1000.0 billion, new auto loan originations of about \$75.0 billion, and C&I loan originations were some \$61.8 billion in the first week of May 2003.[16] The literature discussed above suggests that the reduction in lending occurs in the bank-dependent loan categories. Thus if only mortgages, auto and C&I loans are affected, lending could have reduced by up 7.56 percent of total lending in the third quarter of 1999, by up to 7.63 percent of total lending in the last quarter of 2001, and by up to 8.92 percent of total lending in the second quarter of 2003.

Figure 10 provides an additional way to put in context the decline in lending that would be implied by countercyclical capital buffers being deployed based on a false positive. Here we focus on 2001:Q4, which is the quarter that the NBER dated as the trough of the 2001 recession. The solid line in panel A of Figure 10 shows the first difference of the volume of depository institutions' (DIs') loans at an annualized rate over the period 1990 to 2010, as reported in the Flow of Funds Accounts. The dotted line subtracts from the flow of DI loan volumes \$141.1 billion from all four quarters of 2002, where this amount is the upper bound of the reduction in lending that would have been implied by a 2 percentage point countercyclcial buffer add-on having been deployed in 2001:Q4. We subtract this amount from the flow of DI loan volumes in all four quarters of the following year because the proposal put forward in the MVTF's Consultative Document gave banks a year to increase their capital ratios; note also that we are working with annual rates.

The solid black line in panel B shows the path of loan volumes for DIs (deflated by GDP prices) in the quarters around 2001:Q4, rescaled to a value of 100 in that quarter. (This is the real volumes counterpart to the solid black line in panel A.) The dashed black line shows the path of loan volumes in the quarters around 2001:Q4 assuming the deployment of a 2 percentage point countercyclical capital buffer. (This line is the real volumes counterpart to the dashed black line in panel A.) As can be seen from the figure, the deployment of the buffer implies that over the first year following the 2001 recession (real) DI loan volumes would have contracted rather than remaining about flat as it did in history.

To see whether such a contraction would have been significant, we overlay on panel B the paths of real DI loan volumes following the 1990-91 and 2007-09 recession. To make these lines comparable we rescale them so that their value at the trough of the recession is 100; that is for the 1990-91 recession line (shown in blue) we set real loan volumes equal to 100 in 1991:Q1 and for the 2007-09 recession line (shown in red) we set real loan volumes equal to 100 in 2009:Q2. As can be seen from the figure, the contraction in real DI loan volumes in 2002 would have been similar to the paths of loan volume in the years following 1991:Q1 and 2009:Q2. Beyond the year, however, the

[16] See the results of the NYFRB Consumer Credit Panel (Federal Reserve Bank of New York, 2010), and the Survey of Terms of Business Lending, http://www.federalreserve.gov/releases/e2/.

paths diverge. Thus had a 2 percentage point countercyclical capital add-on been put into effect in 2001:Q4 and had banks also chosen to hold an additional 1 percentage point buffer—which is the upper bound of our estimates—loan volumes in 2002 would have declined similarly to how they contracted during the credit crunches following the 1990-91 and 2007-09 recessions.

Our discussion so far has focused solely on quantities. However, higher capital requirements can also potentially affect banks' funding cost and translate into higher spreads for borrowers. That said, there appears little empirical evidence to support this possibility. For example, Meisenzahl (2011) using small business loan data, finds no significant effect of funding cost or capital ratios on business loan interest rates when using all banks. Specifically, for banks with more than $50 billion in assets in the 2003 Survey of Small Business Finances survey sample, Meisenzahl finds that a 10 percentage point increase in the capital ratio increases the business loan interest rate only 23 basis points (and this effect is moreover insignificant). Hanson, Kashyap, and Stein (2011), doing a back-of-the-envelope calculation, report a modest loan interest rate increase of up to 35 basis points for a 10 percent point increase in the capital ratio.[17] In sum, additional countercyclical capital buffers of 1 or 2 percent points appear unlikely to increase loan interest rates significantly.

4.4 The role of risk weights

Since the reduction in lending accounts only for a part of the necessary increase of capital ratios, we now discuss how banks increased their capital ratios during the 2001 recession. Figure 9 shows that capital ratios increase during the 2001 recession. This increase was concentrated in the risk-based capital ratios and driven by the largest banks (see figure 9 and table 5). Admati, DeMarzo, Hellwig, and Pfleiderer (2010) argue that the risk-weights assigned to conventional bank loans relative to securities and trading assets may in themselves contribute to a credit crunch. In times of higher capital standards, banks have an incentive to reduce lending and increase holding in assets with low risk weights.

The sharp increase in the capital ratios can be attributed to an increase in the liquid assets.[18] The 25 largest banks increased their holding of liquid assets, which usually have low risk weights, by over 14 percent during the fourth quarter of 2001 while for the whole banking sector the increase was only 1.4 percent. In contrast, the 25 largest banks increased their holding of illiquid assets, which usually have high risk weights, by only 1.6 percent in the same quarter while for the whole banking sector the increase was 0.4 percent.

The Call Reports show that the largest increase occurred in fact in the assets classes with zero

[17] In a companion paper, Kashyap, Stein, and Hanson (2010) report a range of 25 to 45 basis points for a 10 percent point increase in the capital ratio.

[18] The classification of liquid and illiquid assets follows Berger and Bouwman (2009).

or low risk weights. The banking sector as whole increased holdings of assets with a risk weight of 0 percent (Call Report Item RCFDb696) by 3.0 percent in 2001:Q1 and by another 7.5 percent in 2002:Q4. Holdings in the 20 percent risk-weight category (Item RCFDb697) increased by 4.0 percent in 2001:Q1 and declined by 1.8 percent in 2002:Q4. Similarly, holdings in the 50 percent and 100 percent risk-weight categories (Items RCFDb698 and RCFDb699) increased by mere 1.4 percent and 0.5 percent in 2001:Q1 and declined by 3.9 percent and 0.9 percent in 2002:Q4. Hence, these patterns support the view of Admati, DeMarzo, Hellwig, and Pfleiderer (2010) that the risk-weights may in themselves contribute to a credit crunch, and the patterns also support our assumptions about where the reduction in lending is likely to occur that we made in the previous section. The increase in liquid assets and the decrease in illiquid assets are also consistent with the findings of Hancock and Wilcox (1983) and Peek and Rosengren (1995) that the reduction in lending occurs in in the bank-dependent loan category such as small business lending.

5 Conclusions

The motivation of this paper was to assess potential cost of using the credit-to-GDP ratio gap as reference point for countercyclical capital buffers as proposed by the MVTF. Specifically we show that the credit-to-GDP gap measures are very unreliable in real time: Revisions to real-time estimates are large and are, moreover, on the same order as fluctuations in the gap itself. In addition, correlations between quasi real-time and final estimates of the gap are low. Importantly, also, it is not revisions to the underlying data that is the main reason for the unreliability of the credit-to-GDP ratio gap in real-time but rather it is end-point problems associated with the filters that we use to isolate the cyclical component of the credit-to-GDP ratio that drive essentially all of the revisions.

The unreliability of the credit-to-GDP ratio gap documented in this paper suggests considerable practicable difficulties in tying the deployment of countercyclical capital buffer to this measure. Specifically, there is a tendency for credit-to-GDP ratio gaps to yield "false positives" in terms of indicating in real-time excessively high levels of credit that later—based on longer timeseries of data—do not appear so extreme. Since excessively high levels of credit bring about the deployment of countercyclcial capital buffers, false positives can result in banking-sector capital shortfalls and unnecessary restraint being placed on lending. We investigate a few instances in which the credit-to-GDP ratio gap yields false positives and find that in these episodes the impact on loan volumes can be notable.

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Table 1: Credit-to-GDP Ratio Gap Summary Statistics

Method	Mean	Std. Dev.	Minimum	Maximum	Corr. w/ final
Linear					
Final	-1.08	9.09	-14.20	21.95	
Quasi real-time	2.89	8.71	-10.32	23.51	0.97
Quadratic					
Final	-0.8	7.65	-13.60	16.21	
Quasi real-time	4.99	6.86	-8.52	17.90	0.81
Cubic					
Final	0.46	6.27	-12.97	11.89	
Quasi real-time	-0.15	6.15	-13.07	8.00	0.37
Cubic spline					
Final	0.10	5.90	-18.74	9.46	
Quasi real-time	0.15	5.95	-18.74	10.42	-0.11
HP: 1600					
Final	-0.01	2.30	-8.47	6.98	
Quasi real-time	-0.02	2.69	-9.33	3.46	0.41
HP: 25,000					
Final	-0.01	4.20	-12.55	7.81	
Quasi real-time	0.92	4.74	-12.55	7.75	0.45
HP: 125,000					
Final	0.04	5.52	-10.35	9.89	
Quasi real-time	1.92	5.77	-10.35	9.83	0.61
HP: 400,000					
Final	-0.10	6.35	-10.21	12.39	
Quasi real-time	2.45	6.42	-9.99	13.53	0.73
BK: 6 to 30					
Final	0.04	1.87	-3.86	5.8	
Quasi real-time	0.07	1.14	-3.86	2.27	0.56
BK: 6 to 60					
Final	0.05	3.22	-6.41	9.52	
Quasi real-time	0.35	2.34	-5.06	4.17	0.41
BK: 6 to 90					
Final	1.03	5.93	-7.41	13.8	
Quasi real-time	1.13	3.25	-5.69	5.97	0.68

Table 2: Revision Summary Statistics

Method	Mean	Std. Dev.	RMSE	Minimum	Maximum	AR
	Entire quasi real-time to final revision					
Linear	-3.97	2.38	4.63	-7.96	0.00	1.000
Quadratic	-5.79	4.49	7.32	-13.03	1.79	1.002
Cubic	0.61	6.97	6.97	-8.21	15.09	0.995
Cubic spline	-0.05	8.84	8.80	-15.85	13.34	0.994
HP: 1600	0.02	2.74	2.73	-4.02	7.05	0.981
HP: 25,000	-0.93	4.72	4.79	-7.21	9.02	0.994
HP: 125,000	-1.87	5.01	5.33	-9.14	8.19	0.997
HP: 400,000	-2.56	4.73	5.36	-10.02	5.92	0.997
BK: 6 to 30	-0.03	1.55	1.54	-2.94	5.48	0.932
BK: 6 to 60	-0.30	3.11	3.11	-5.85	8.76	0.970
BK: 6 to 90	-0.10	4.40	4.39	-6.12	9.82	0.985
	Revisions within one year					
Linear	-0.20	0.75	0.78	-1.60	0.99	0.989
Quadratic	-1.02	1.35	1.69	-3.04	1.76	0.992
Cubic	0.09	2.13	2.12	-2.40	4.90	0.997
Cubic spline	-0.33	3.44	3.44	-6.33	8.86	1.022
HP: 1600	-0.05	1.75	1.74	-2.34	6.43	1.034
HP: 25,000	-0.44	1.83	1.87	-3.01	4.44	1.016
HP: 125,000	-0.59	1.59	1.69	-2.63	2.73	1.000
HP: 400,000	-0.57	1.36	1.47	-2.74	2.08	0.992
BK: 6 to 30	-0.04	1.29	1.29	-1.69	5.16	0.884
BK: 6 to 60	-0.05	1.87	1.86	-2.56	7.70	0.882
BK: 6 to 90	-0.05	2.01	2.00	-2.79	8.41	0.881

Table 3: Summary Reliability Indicators

Method	Noise-to-Signal (1)	Noise-to-Signal (2)	Corr.	Opposite Sign	In latter 90 pctl. if in QRT 90 pctl.	In QRT 90 pctl. if in latter 90 pctl.
	Entire quasi real-time to final revision					
Linear	0.26	0.51	0.97	0.11	0.39	1.00
Quadratic	0.59	0.96	0.81	0.37	0.32	1.00
Cubic	1.11	1.11	0.37	0.39	0.21	0.30
Cubic spline	1.50	1.49	-0.11	0.66	0.03	0.04
HP:1600	1.19	1.19	0.41	0.38	0.15	0.33
HP:25,000	1.12	1.14	0.45	0.40	0.19	0.39
HP:125,000	0.91	0.97	0.61	0.31	0.33	0.70
HP:400,000	0.74	0.84	0.73	0.27	0.40	0.91
BK:6 to 30	0.83	0.82	0.56	0.30	0.10	0.05
BK: 6 to 60	0.97	0.97	0.41	0.32	0.00	0.00
BK: 6 to 90	0.74	0.74	0.68	0.23	0.31	0.22
	Revisions within one year					
Linear	0.08	0.09	1.00	0.01	0.92	1.00
Quadratic	0.18	0.22	0.99	0.03	0.88	1.00
Cubic	0.34	0.34	0.98	0.03	0.38	1.00
Cubic spline	0.58	0.58	0.87	0.17	0.05	0.67
HP:1600	0.76	0.76	0.60	0.21	0.12	0.45
HP:25,000	0.44	0.45	0.96	0.08	0.43	0.95
HP:125,000	0.29	0.31	0.99	0.03	0.46	0.96
HP:400,000	0.21	0.23	1.00	0.03	0.62	1.00
BK:6 to 30	0.69	0.69	0.52	0.27	0.40	0.24
BK: 6 to 60	0.58	0.58	0.73	0.25	0.55	0.41
BK: 6 to 90	0.34	0.34	0.85	0.12	0.75	0.48

Table 4: Counterfactual Capital Shortfall and Reduction in Lending

	Capital Shortfall (in billion)		
Required Total Risk-based Capital Ratio	1999:Q3	2001:Q4	2003:Q2
10.5%	1.8	2.1	1.1
11.0%	6.9	5.5	6.3
11.5%	18.2	12.9	15.7
12.0%	32.2	24.4	28.4
12.0% + 0.5% pre-cautionary buffer	49.3	39.9	46.4
12.0% + 1.0% pre-cautionary buffer	67.7	58.8	67.3

	Reduction in Lending (in billion)					
	1999:Q3		2001:Q4		2003:Q2	
Required Total Risk-based Capital Ratio	Lower Bound	Upper Bound	Lower Bound	Upper Bound	Lower Bound	Upper Bound
10.5%	3.3	36.2	3.9	26.7	2.0	30.6
11.0%	12.8	68.9	10.2	52.6	11.7	61.0
11.5%	33.9	115.3	24.0	90.9	29.2	106.7
12.0%	59.9	167.1	45.4	141.1	52.8	162.1

Source: Call Reports.
The capital shortfall is defined as the total amount of capital need by banks that have capital ratios below the requirements holding assets constant. The lower bound is constructed using the estimates for bank-specific target ratio of \$1.86 of lending for \$1 of capital as reported in Berrospide and Edge (2010). The upper bound is constructed using the estimates for regulatory capital shortfalls of \$3.16 of lending for \$1 of capital as reported in Hancock and Wilcox (1993) for each \$1 of capital shortfall plus a 1% pre-cautionary bank-specific target buffer with \$1.86 of lending for \$1 of capital.

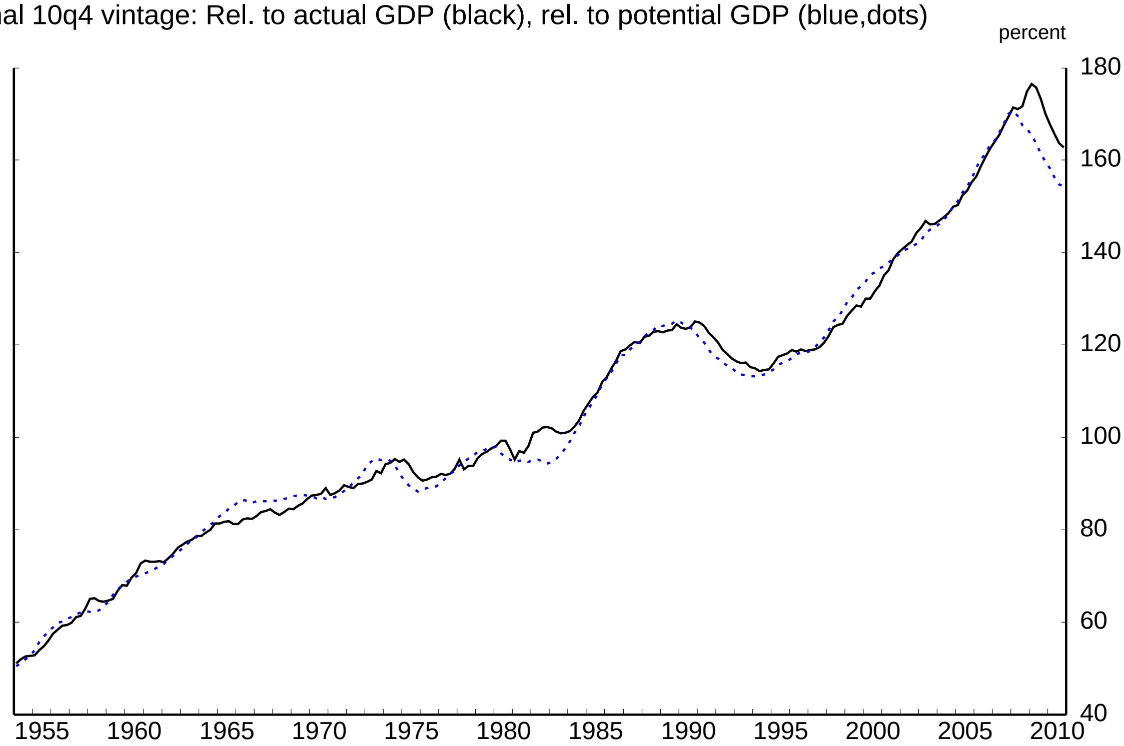
Table 5: Unweighted Capital Ratio Growth Rates in 2001

	Full Sample			25 Largest Banks by Assets		
Quarter	Tier 1 Leverage Capital Ratio	Total Risk-Based Capital Ratio	Tier 1 Risk-Based Capital Ratio	Tier 1 Leverage Capital Ratio	Total Risk-Based Capital Ratio	Tier 1 Risk-Based Capital Ratio
2001:Q2	-1.31%	-2.10%	-2.21%	1.49%	1.16%	1.01%
2001:Q3	-1.22%	-1.22%	-1.20%	-1.08%[a]	0.03%	-0.55%[a]
2001:Q4	-2.66%	-1.69%	-1.86%	-0.12%[a]	4.00%	4.23%
2002:Q1	-0.46%	0.15%	0.11%	2.16%	2.48%	2.88%

Source: Call Reports.
[a] for the 10 largest banks by assets these growth rates are also positive.

Fig. 1: Credit-to-GDP Ratios

A: Final 10q4 vintage: Rel. to actual GDP (black), rel. to potential GDP (blue,dots)



B: Vintages: 95q2 (dots), 98q4 (dashes), 02q2 (magenta), 05q4 (red), 09q2 (blue), 10q4 (black)

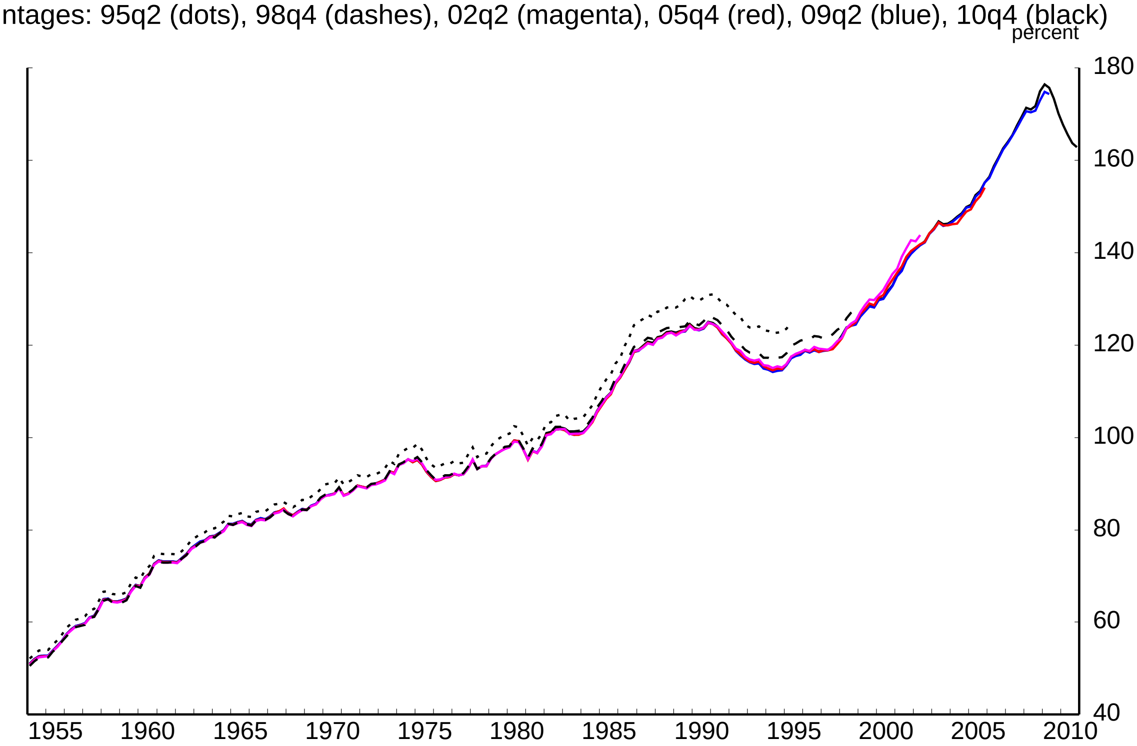
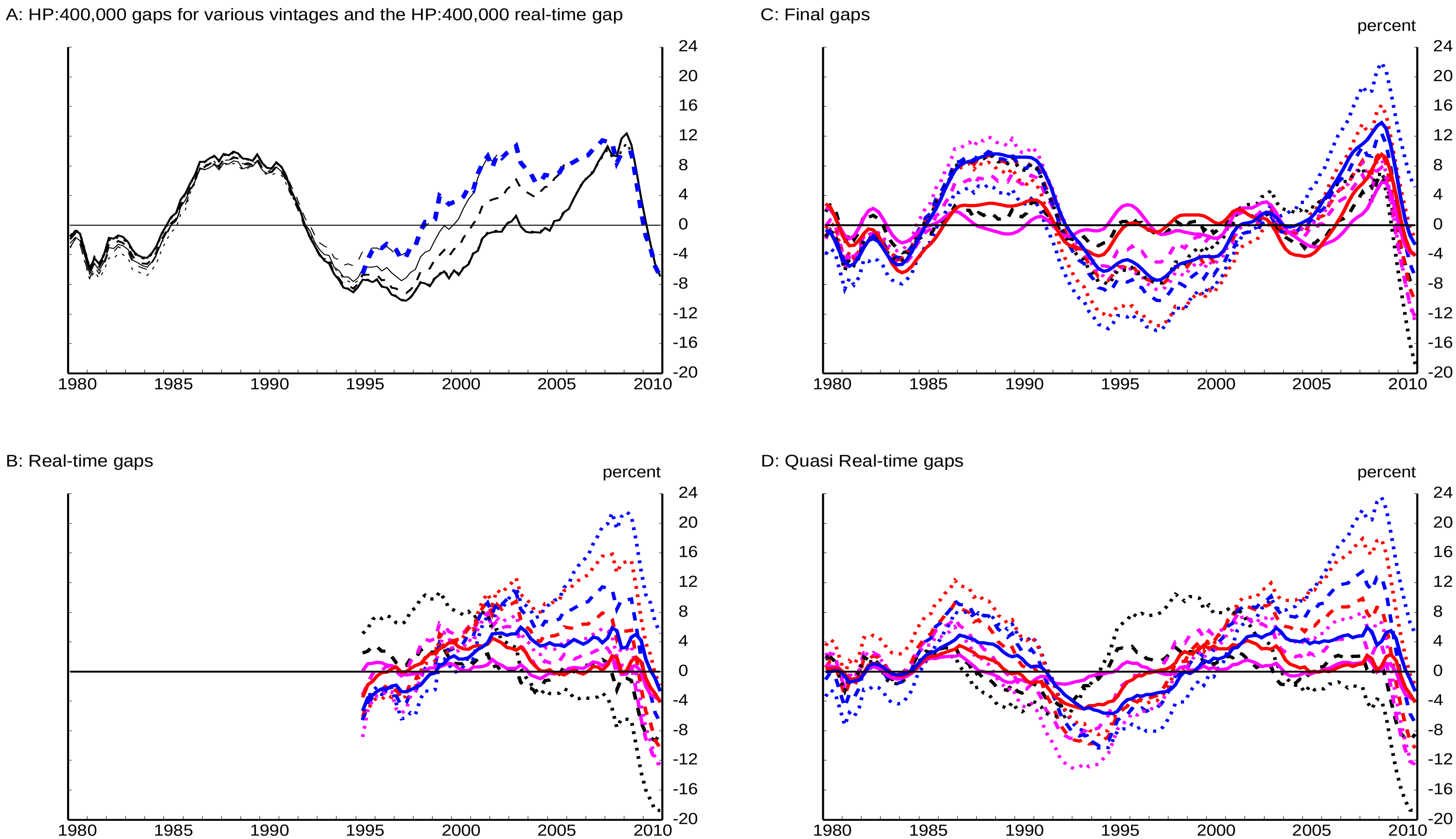


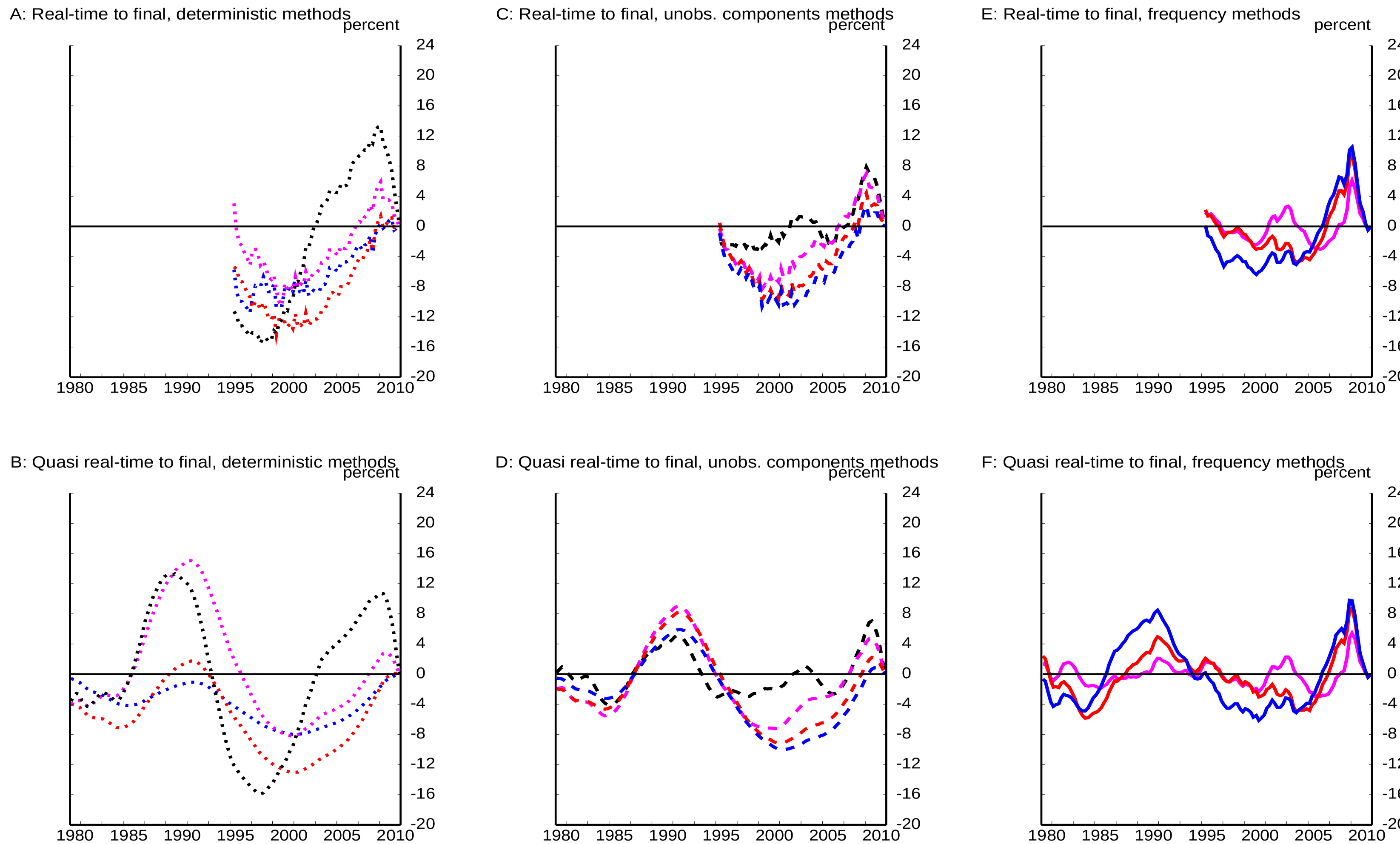
Fig. 2: Credit-to-GDP Ratio Gaps



Legend for Panel A: 95q2 (thin, dots), 98q4 (thin, dashes), 02q2 (thin, solid), 05q4 (dots), 09q2 (dashes), 10q4 (solid), HP-400,000(blue)

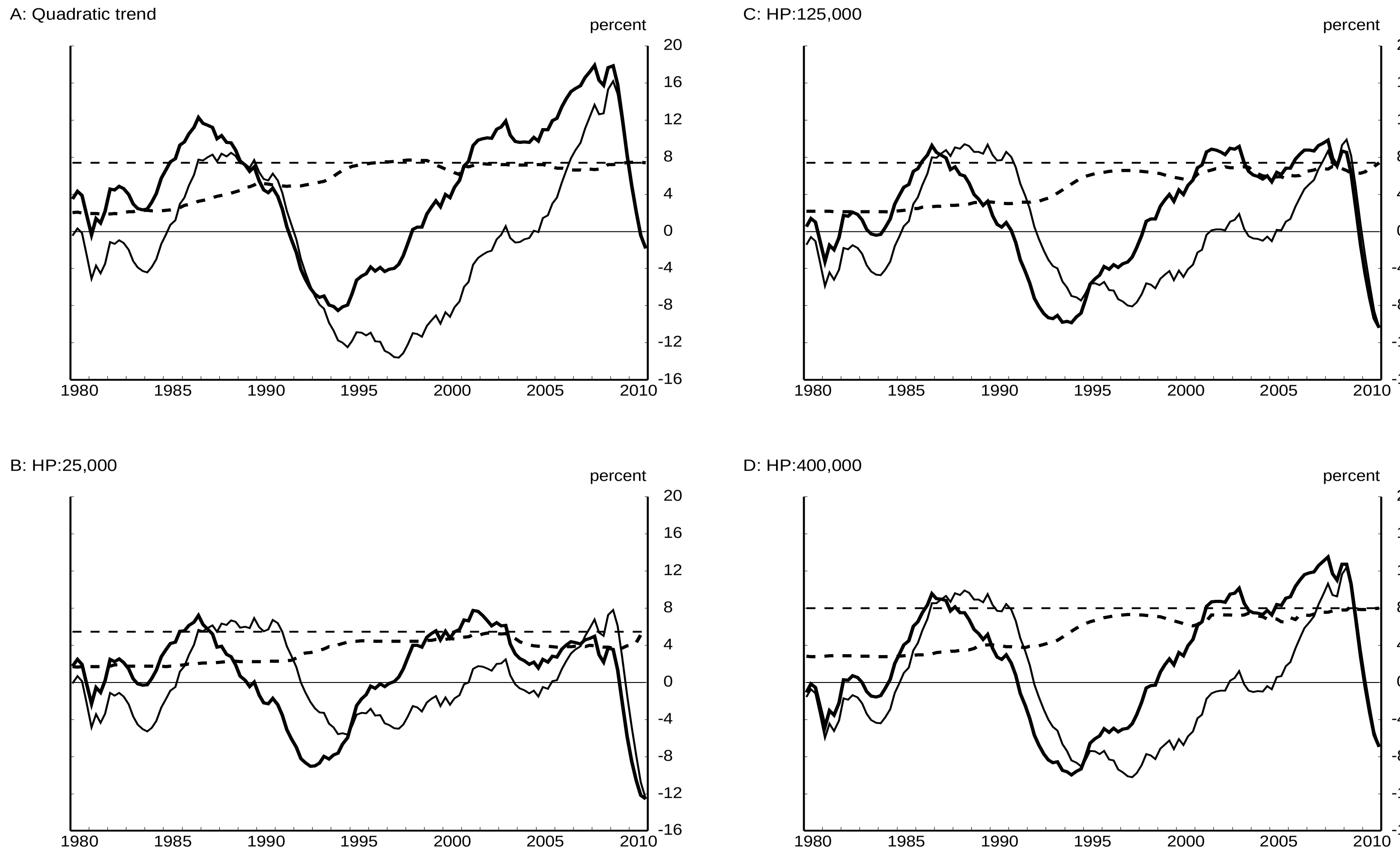
Legend for Panels B, C, and D: Lin. (blue, dots), Quad. (red, dots), Cub. (magenta, dots), Cub. sp. (black, dots), HP:400,000 (blue, dashes), HP:125,000 (red, dashes), HP:25,000 (magenta, dashes), HP:1,600 (black, dashes), BK:90 (blue, solid), BK:60 (red, solid), BK:30 (magenata, solid)

Fig. 3: Credit-to-GDP Ratio Gap Revisions



Legend: Lin. (blue, dots), Quad. (red, dots), Cub. (magenta, dots), Cub. sp. (black, dots), HP:400,000 (blue, dashes), HP:125,000 (red, dashes), HP:25,000 (magenta, dashes), HP:1,600 (black, dashes), BK:90 (blue, solid), BK:60 (red, solid), BK:30 (magenata, solid)

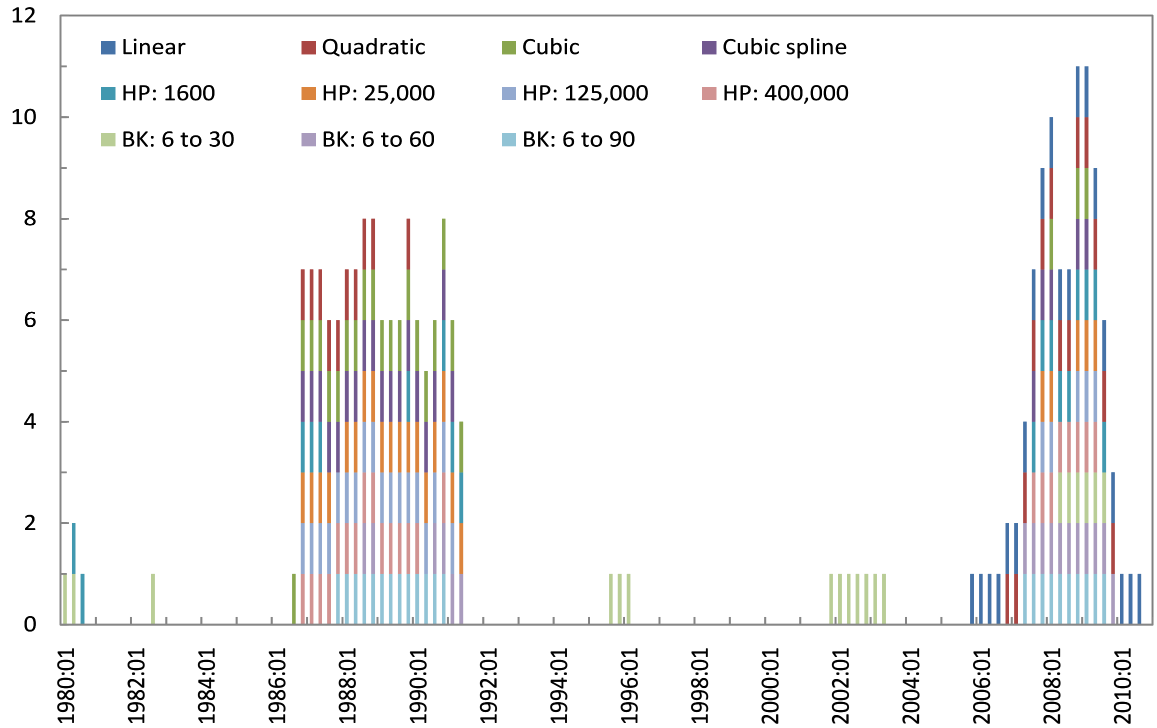
Fig. 4: Credit-to-GDP Ratio Gaps and Series 90th Percentiles



Legend: Quasi real-time gap (thick, solid), Quasi real-time 90th percentile (thick, dashed),
Final gap (thin, solid), Final 90th percentile (thin, dashed),

Fig. 5: Quarters in which the Credit-to-GDP Ratio Gap is in the 90th Percentile

A. Final



B. Quasi Real-time

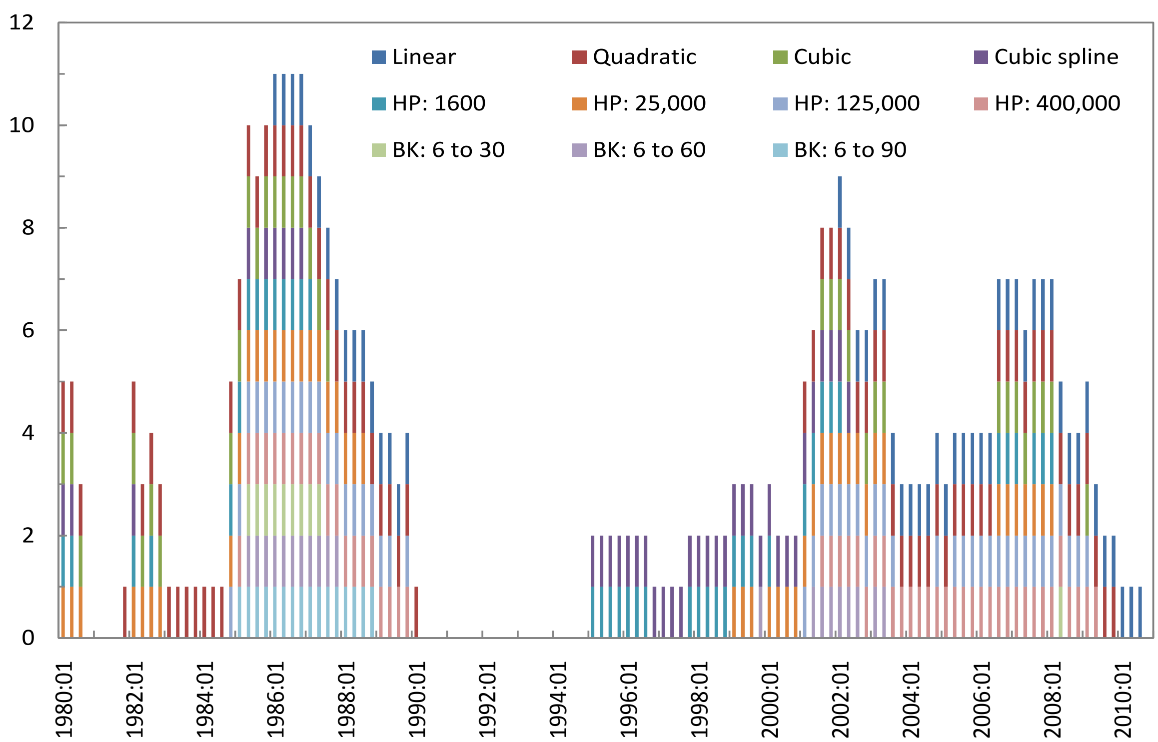
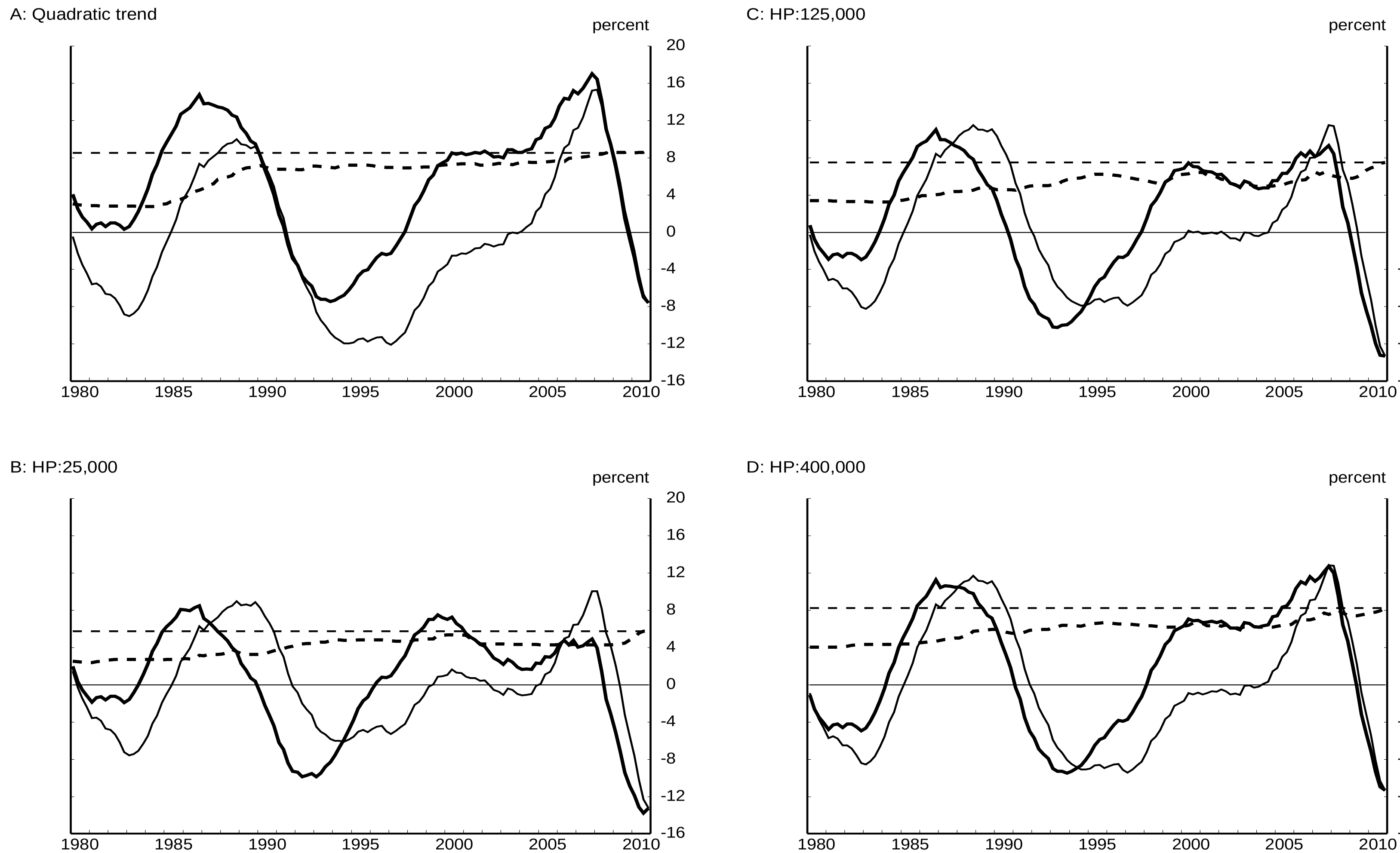


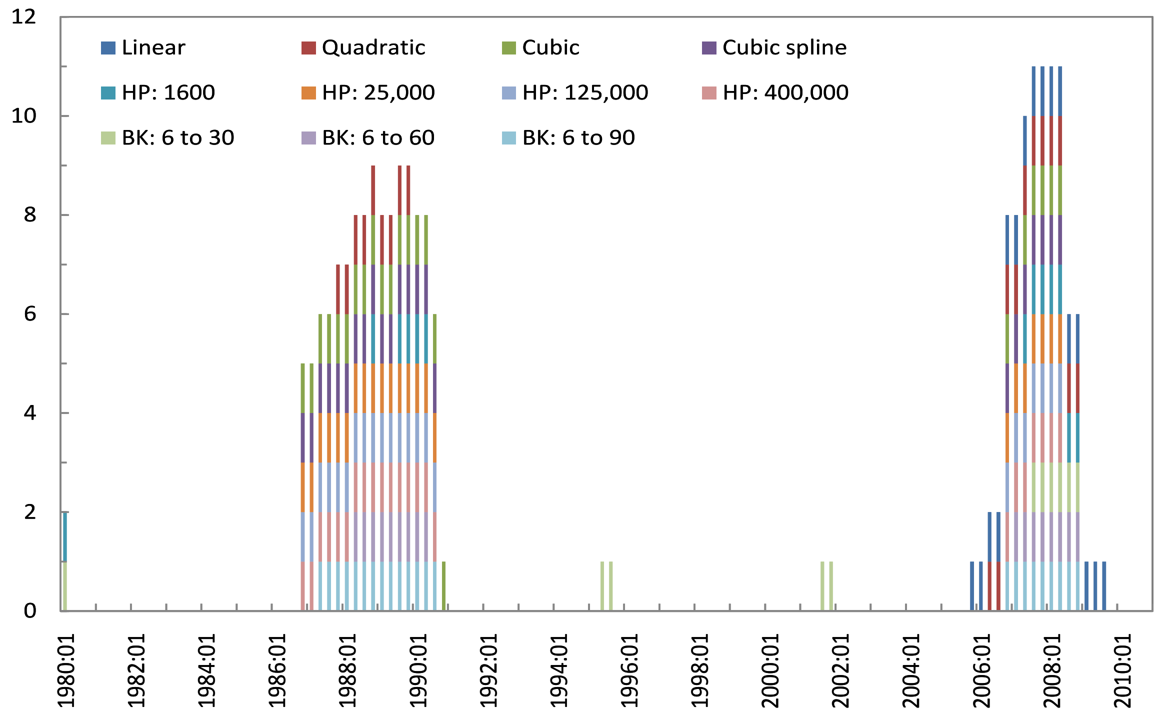
Fig. 6: Credit-to-Potential GDP Ratio Gaps and Series 90th Percentiles



Legend: Quasi real-time gap (thick, solid), Quasi real-time 90th percentile (thick, dashed),
Final gap (thin, solid), Final 90th percentile (thin, dashed),

Fig. 7: Quarters in which the
Credit-to-Potential GDP Ratio Gap is in the 90th Percentile

A. Final



B. Quasi Real-time

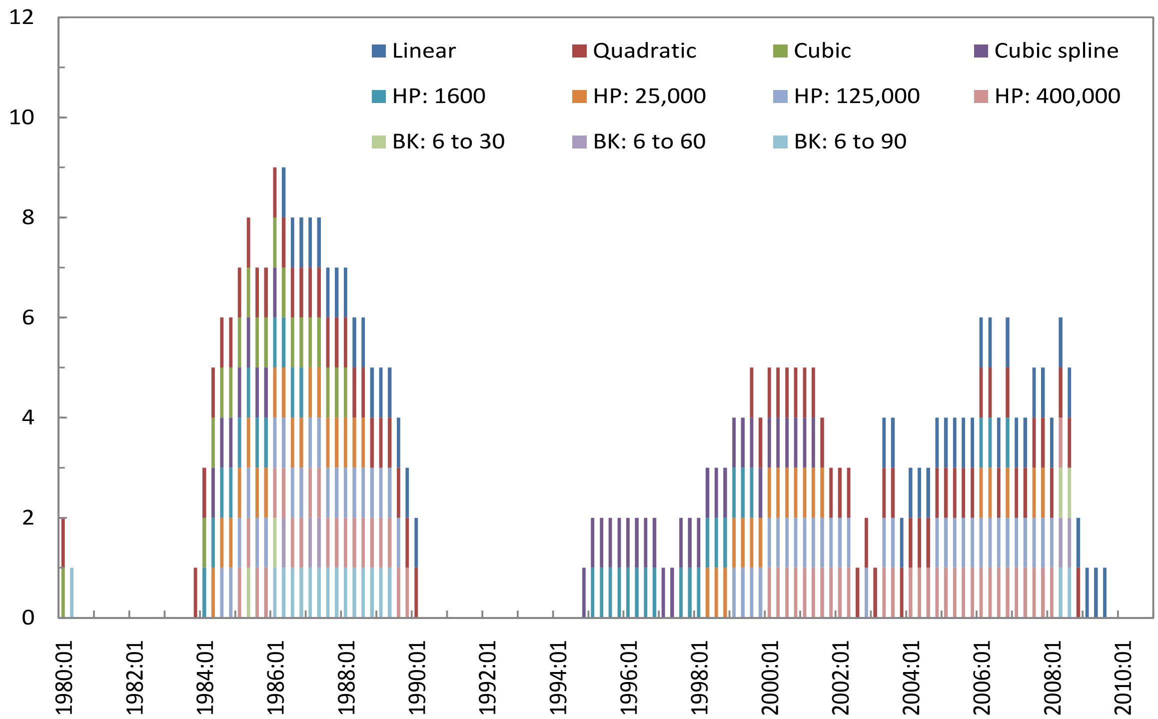


Fig. 8: Distribution of Unweighted and Weighted Capital Ratios

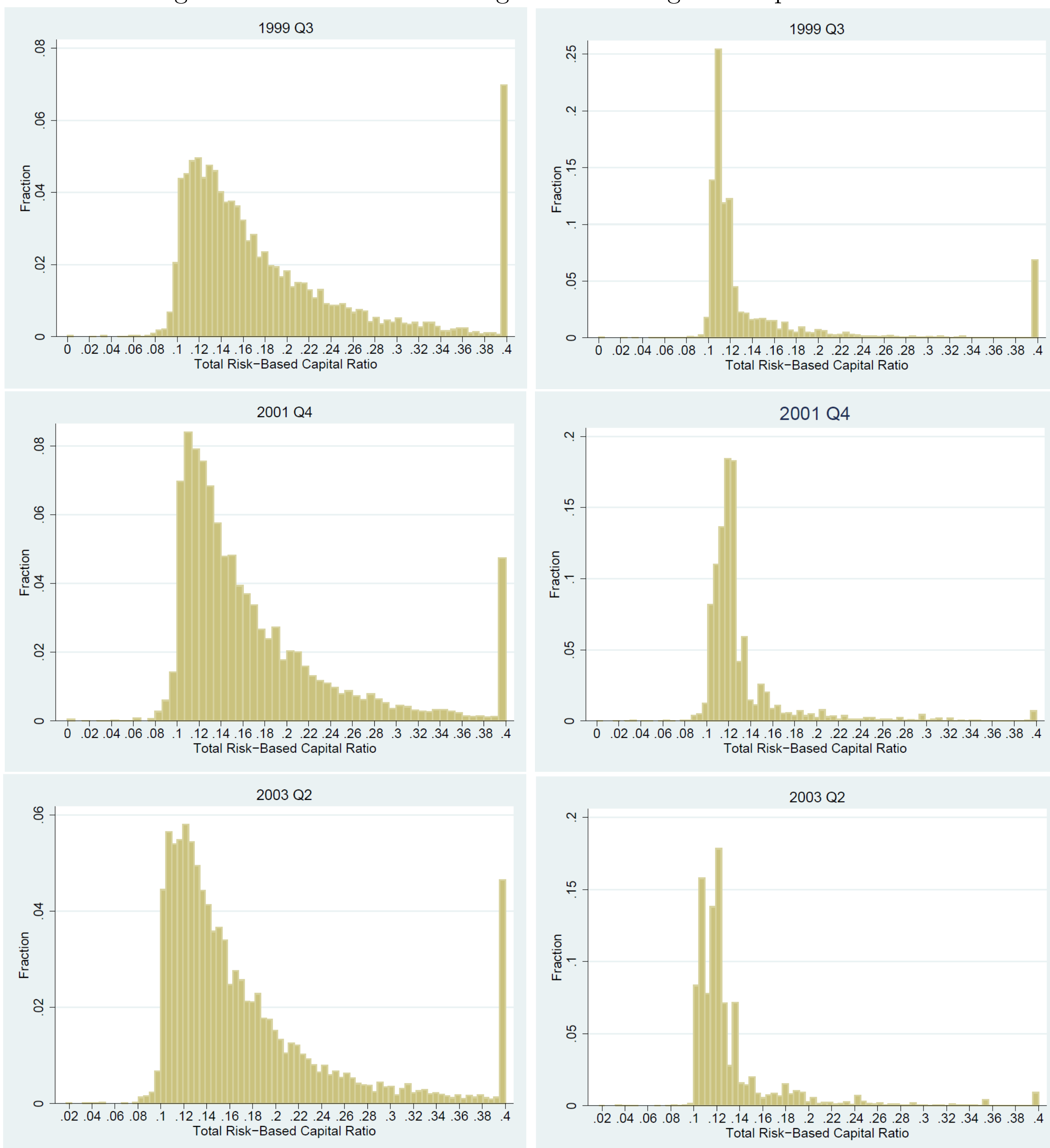
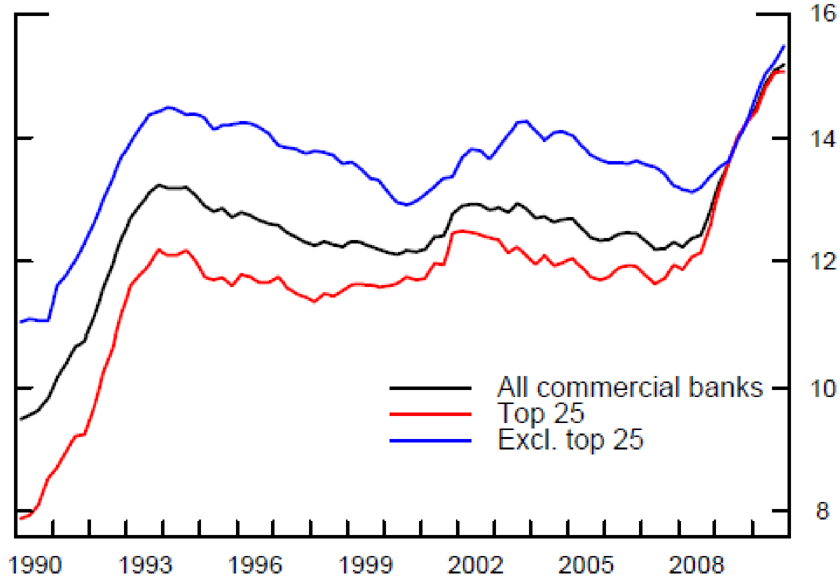


Fig. 9: Capital Ratios



Note. Rank is by total assets.
Source. Call report.

Fig. 10: Depository Institutions’ Loan Volumes

A: Annualized First Difference of DI Loan Volumes: Actual (solid); Alternative (dashed)

B: DI Loan Volumes: 2001 Actual (solid); 2001 Alt. (dashed), Shifted 1991 (blue), Shifted 2009 (red)