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Bank Competition and Financial Stability: Much Ado about Nothing?

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Bank Competition and Financial Stability: Much Ado about Nothing?

Tomáš Havránek and Diana Žigraiová*

Abstract

The theoretical literature gives conflicting predictions on how bank competition should affect financial stability, and dozens of researchers have attempted to evaluate the relationship empirically. We collect 598 estimates of the competition-stability nexus reported in 31 studies and analyze the literature using meta-analysis methods. We control for 35 aspects of study design and employ Bayesian model averaging to tackle the resulting model uncertainty. Our findings suggest that the definition of financial stability and bank competition used by researchers influences their results in a systematic way. The choice of data, estimation methodology, and control variables also affects the reported coefficient. We find evidence for moderate publication bias. Taken together, the estimates reported in the literature suggest little interplay between competition and stability, even when corrected for publication bias and potential misspecifications.

Abstrakt

Teoretická literatura se neshoduje v názoru, jak míra soutěže mezi bankami ovlivňuje finanční stabilitu. Množství studií se pokouší tento efekt odhadnout empiricky, ale jejich výsledky se značně liší. V tomto článku analyzujeme 598 odhadů tohoto vztahu publikovaných v 31 studiích, k čemuž používáme metody meta-analýzy. Bereme v úvahu 35 faktorů, které mohou přispívat k tomu, že různé studie publikují různé výsledky, a používáme bayesovské modelové průměrování k ošetření modelové nejistoty. Naše výsledky naznačují, že definice finanční stability a soutěže mezi bankami použité v dané studii systematicky ovlivňují výsledky. Důležitá je také volba dat, metody odhadu a kontrolních proměnných. Nacházíme stopy mírné publikační selektivity. Literatura jako celek neukazuje na žádný významný vztah mezi soutěží na bankovním trhu a finanční stabilitou, i když odhady očistíme o publikační selektivitu a chyby v měření.

JEL Codes: C11, C83, G21, L16.

Keywords: Bank competition, Bayesian model averaging, financial stability, meta-analysis, publication selection bias.

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Nontechnical Summary

Dozens of researchers have estimated the effect of bank competition on financial stability, but their results vary significantly. Some studies support the competition-fragility hypothesis, which asserts that in a competitive environment banks are forced to take excessive risks, contributing to the fragility of the financial system. Other studies corroborate the competition-stability hypothesis, which suggests that banks in less competitive environments tend to rely on their too-big-to-fail position, which may contribute to instability. We collect all estimates of the competition-stability nexus from the literature and examine them quantitatively using meta-analysis methods. Our results suggest that the mean reported estimate of the relationship is close to zero, even after correcting for publication bias and potential mistakes in measurement. We find evidence for publication selection against positive results; that is, some authors of primary studies tend to discard estimates inconsistent with the competition-fragility hypothesis.

Our results indicate that data characteristics matter for the reported coefficients corresponding to the competition-stability nexus. Researchers who use heterogeneous samples of countries (including both developed and developing economies) tend to obtain smaller estimates. The effect of competition on stability is larger in developed countries, but even there the positive effects do not seem to be strong. Next, accounting for potential nonlinearities in the effect of competition on stability is important and typically yields smaller estimates of the competition-stability nexus. We also find that, in general, researchers who have more data at their disposal tend to report smaller estimates.

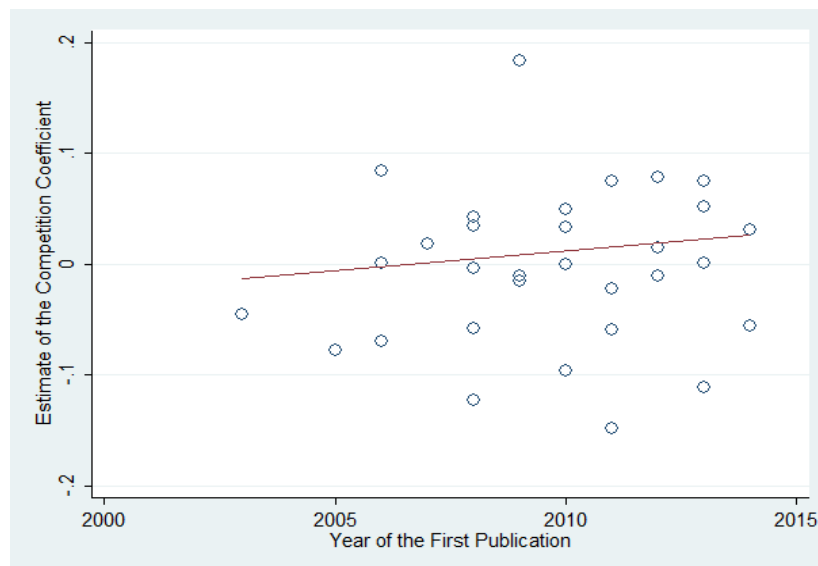
Furthermore, we show that the definition of the proxy for financial stability is important for the results of primary studies. For example, if dummy variables (usually indicating financial crises) are used as a proxy for stability, the authors tend to report much larger estimates than when a continuous measure of financial stability is used. In a similar vein, the results of primary studies are systematically affected by the choice of the proxy for bank competition. Studies using the H-statistic (a measure that uses the elasticities of banks' revenue with respect to the banks' input prices) tend to report larger estimates of the competition-stability nexus, while studies that employ the Boone indicator (based on the relation between bank performance, measured by profits, and efficiency, measured by marginal costs) usually show smaller estimates. Next, if the researchers ignore the endogeneity problem in regressing financial stability on bank competition (i.e., that stability may also influence competition), they tend to underestimate the effect.

We also find that controlling for supervisory and regulatory conditions in regressions usually decreases the reported estimates, which supports the notion that banking systems with more activity restrictions and greater barriers to entry are more likely to suffer from systemic financial distress (Beck et al., 2006 a,b). In the last step of our analysis we construct a weighted average of all the estimates and give more weight to the ones whose authors avoid potential mistakes in measurement (such as ignoring endogeneity) and that have better publication characteristics (for example, more citations). The resulting estimate still points to a very weak or non-existent link between bank competition and financial stability.

1. Introduction

The theory does not provide clear guidance on the expected sign of the relationship between bank competition and financial stability. On the one hand, the competition-fragility hypothesis (represented, for example, by Keeley, 1990) argues that competition hampers stability. Strong competition in the banking sector forces banks to take on excessive risks in the search for yield, which leads to overall fragility of the financial system. On the other hand, under the competition-stability hypothesis (for instance, Boyd and De Nicolo, 2005), increased competition makes the financial system more resilient. A competitive banking sector results in lower lending rates, which support firms' profitability, leading to lower credit risk for banks. Moreover, in uncompetitive environments banks are more likely to rely on their too-big-to-fail position and engage in moral hazard (Mishkin, 1999). Since the early 2000s, dozens of researchers have reported estimates of the competition-stability nexus, but their results vary. As Figure 1 shows, the reported results do not converge to a consensus number, complicating our inference from the literature.

Figure 1: The Reported Estimates of the Competition-stability Nexus Do Not Converge



Notes: The figure depicts the median partial correlation coefficients corresponding to the effects of banking competition on financial stability reported in individual studies. The horizontal axis measures the year when the first drafts of the studies appeared in Google Scholar. The line shows the linear fit (the slight upward trend is not statistically significant).

When the literature lacks patterns visible at first sight, narrative surveys are useful in discussing the reasons for the heterogeneity observed in the results, but they cannot provide policy makers and other researchers with clear guidelines concerning the relationship in question. Our aim in this paper is to collect all available estimates of the relation between bank competition and financial stability, and examine them using up-to-date meta-analysis methods. Meta-analysis is most commonly applied in medical research to synthesize the results of clinical trials, and the use of

this method dates back at least to Pearson (1904). Meta-analysis later spread to the social sciences, including economics and finance, and examples of early applications are summarized by Stanley (2001). Recent applications of meta-analysis include Chetty et al. (2011), who explore the intertemporal elasticity of substitution in labor supply, Doucouliagos et al. (2012), who investigate the link between chief executives' pay and corporate performance, and Babecky and Havranek (2014), who evaluate the impact of structural reforms on economic growth.

We collect 598 estimates of the competition-stability nexus from 31 studies published between 2003 and 2014, and present, to our knowledge, the first meta-analysis on the topic. We do not find evidence for any robust relationship between bank competition and financial stability: either the positive and negative effects of competition offset each other, or current data and methods do not allow researchers to identify the relationship. This conclusion holds even when we account for publication selection bias and potential misspecifications in the literature.

The studies estimating the effect of bank competition on financial stability differ greatly in terms of the data and methodology used. We account for 35 aspects of studies and estimates, including the length of the sample, regional coverage, the definitions of key variables, the inclusion of controls, the estimation methodology, and publication characteristics (such as the number of citations of the study and the impact factor of the journal). We explore how these aspects affect the reported estimates, and use Bayesian model averaging (BMA; Raftery et al., 1997) to address model uncertainty. BMA is especially useful in meta-analysis, because for many study aspects there is no theory telling us how they should influence the results. Our findings indicate that researchers' choices concerning the data used, the definitions of key variables, and the estimation methodology affect the reported estimates systematically. We also find that highly cited studies published in good journals tend to report larger estimates of the competition-stability nexus. Finally, using all the estimates we construct a synthetic study, for which we select the methodology and publication aspects that we prefer (such as control for endogeneity and the maximum number of citations). The resulting estimate of the competition-stability nexus is very small.

The paper is organized as follows. Section 2 briefly discusses the related literature on the topic and explains how the effect of bank competition on financial stability is estimated. Section 3 explains how we collect the estimates and re-compute them to a common metric (partial correlation coefficients). Section 4 tests for the presence of publication bias. Section 5 describes the sources of heterogeneity in the literature and provides estimates of the competition-stability nexus conditional on our definition of best practice. In Section 6 we perform robustness checks using, among other things, alternative priors for BMA and alternative weights. Section 7 concludes. Appendix A presents diagnostics of the BMA exercise. The online appendix at <http://meta-analysis.cz/competition> includes an extensive robustness check using a more homogeneous subsample of estimates, provides additional results, and also lists the studies included in the meta-analysis.

2. Estimating the Effect of Bank Competition on Financial Stability

The impact of bank competition on financial stability remains a controversial issue in the theoretical literature. Two opposing theories – the competition-stability hypothesis and the

competition-fragility hypothesis – can be used to justify the conflicting results often found in empirical studies.

The competition-fragility hypothesis asserts that more competition among banks leads to instability of the financial system. Marcus (1984) and Keeley (1990) model theoretically the “charter value” proposition, where banks choose the risk level of their asset portfolios. In the setting of limited liability, bank owners, who are often given incentives to shift risks to depositors, tend to engage only in the upside part of the risk-taking process. In more competitive systems, this behavior places substantial emphasis on profits: banks have higher incentives to take on excessive risks, which leads to higher instability of the system in general. In addition, in competitive systems the incentives of banks to properly screen borrowers are reduced, which again contributes to system fragility (Allen and Gale, 2000; Allen and Gale, 2004; Boot and Thakor, 1993). Conversely, when entry barriers are in place and competition in the sector is limited, banks have better profit opportunities and larger capital cushions and, therefore, are not prone to taking aggressive risks. In this framework highly concentrated banking systems contribute to overall financial stability (Boot and Greenbaum, 1993; Hellman, Murdoch, and Stiglitz, 2000; Matutes and Vives, 2000).

The competition-stability hypothesis, on the other hand, proposes that more competitive banking systems imply less fragility of the financial system. Specifically, Boyd and De Nicolo (2005) show that lower client rates facilitate lending as they reduce entrepreneurs’ cost of borrowing. Lower costs of borrowing raise the chance of investment success, which, in turn, lowers banks’ credit portfolio risk and leads to increased stability within the sector. Some theoretical studies reveal that banks in uncompetitive systems are more likely to originate risky loans, which pave the way to systemic vulnerabilities (Caminal and Matutes, 2002). Similarly, Mishkin (1999) stresses that, in concentrated systems, regulators are prone to implement too-big-to-fail policies that encourage risk-taking behavior by banks.

Overall, it appears that empirical studies conducted for individual countries do not find conclusive evidence for either the stability-enhancing or the stability-deteriorating view of competition (Fungacova and Weill, 2009; Fernandez and Garza-Garciab, 2012; Liu and Wilson, 2013). Some of the cross-country literature shows that more competitive banking systems are less likely to experience a systemic banking crisis (Beck et al., 2006a; Schaeck et al., 2009). In contrast, other studies (Yeyati and Micco, 2007; Uhde and Heimeshoff, 2009; Boyd et al., 2006) reveal that in more competitive systems bank failures tend to be more frequent. Further research also provides evidence that in more concentrated systems banks have higher capital ratios, which offsets the possibly stronger risk-taking behavior on their part (Berger et al., 2009; Schaeck and Cihak, 2012).

Throughout the study we use the term “financial stability” to refer to both bank-level stability and banking sector stability. This simplification is justified in the literature by Vives (2010), Beck (2008), and Carletti and Hartmann (2002), for example. Beck (2008) observes that individual bank fragility puts a country’s financial safety under pressure, as several systemic banking crises started as crises in individual banks. In addition, increased cross-border dependencies in the financial sector proved to be of importance, especially during the recent crisis, when financial distress in US subprime markets manifested in German banks. Carletti and Hartmann (2002) explain that banks are heavily linked to each other due to their involvement in interbank lending

markets. Coupled with information asymmetries about their relative health there is a risk of distress propagating from one bank to another, consequently creating systemic risk, i.e., a risk of interbank contagion. Vives (2010) acknowledges a potentially systemic impact of banks on stability owing to a mix of their features, such as high short-term leverage, dispersed debtholders, opacity of bank assets with long maturities, and susceptibility to contagion. Furthermore, banks' implications for financial stability and economic development are even more profound in emerging economies, where information asymmetry problems are aggravated and financial markets are less developed (Vives, 2010).

In this meta-analysis we focus on variants of the following model used in the literature to examine the effect of bank competition on stability:

$$Stability_{it} = \alpha + \beta \cdot Competition Measure_{it} + \sum_{k=1}^N \gamma_{kit} X_{kit} + e_{it}, \quad (1)$$

where i is a bank index and t a time index and X is a set of control variables, both bank-specific and country-specific. Measures of stability and competition tend to vary across individual studies, as we will discuss later in this section (the various estimation methods used by researchers will be discussed in Section 5). We are interested in the coefficient β ; positive (negative) estimates of the coefficient imply a positive (negative) effect of bank competition on financial stability.

Financial stability is often measured in an indirect way: that is, by considering individual or systemic banking distress, effectively the negative of stability. In this spirit, the non-performing loan (NPL) ratio is often used as a fragility indicator. Nevertheless, the NPL ratio only covers credit risk and cannot be directly linked to the likelihood of bank failure (Beck, 2008). Another measure of individual bank distress extensively used in the literature is the Z-score (e.g. Boyd and Runkle, 1993; Lepetit et al., 2008; Laeven and Levine, 2009). This measure indicates how many standard deviations in return on assets a bank is away from insolvency and, by extension, from the likelihood of failure. The Z-score is calculated as follows:

$$Z_{it} = \frac{ROA_{it} + E_{it}/TA_{it}}{\sigma_{ROA_{it}}}, \quad (2)$$

where ROA is the rate of return on assets, E/TA is the ratio of equity to total assets, and σ_{ROA} is the standard deviation of the return on assets. However, the Z-score has also been subject to criticism for several reasons (Beck and Laeven, 2006). First, as it measures risk in a single period and does not reflect the probability of a series of negative profits the Z-score might underestimate banking risk. Second, the Z-score's reliance on accounting data is another caveat, especially given banks' tendency to smooth income over time. Bank profitability, measured by ROA and ROE, profit volatility, approximated by ROA and ROE volatility, and bank capitalization, expressed by the capital adequacy ratio (CAR) or the ratio of equity to total bank assets, are additional measures of individual bank distress frequently used in the literature. Moreover, some studies (e.g. Beck, Demircuc-Kunt, and Levine, 2006a,b) model fragility in the banking sector by means of systemic banking crisis dummies. Other studies (such as Fungacova and Weill, 2009) apply individual bank failure dummies or measures of a bank's distance-to-default to proxy financial

stability. A bank's distance-to-default is a market-based measure, calculated as the difference between the asset value of the bank and the face value of its debt, scaled by the standard deviation of the its asset value. In general, it has been shown to predict defaults well, even outperforming accounting-based measures (Anginer et al., 2014). The fact that the measure is market-based allows for more frequent updates than in the case of accounting measures. Moreover, it tends to be more forward-looking, capturing market perceptions of the bank's soundness in the future (Anginer et al., 2014). On the other hand, the measure's dependence on stock market data is its greatest pitfall. The fact that many banks, especially in developing and transition countries, are not listed on stock exchanges makes the applicability of the distance-to default measure limited.

Concerning the proxies for competition, the Lerner index is one of the indicators frequently employed in the literature. This index quantifies the price power capacity of a bank by expressing the difference between price and marginal cost as a percentage of the price:

$$Lerner_{it} = \frac{(P_{TAit} - MC_{TAit})}{P_{TAit}}, \quad (3)$$

where P_{TAit} is the price of total assets, expressed in practice by total revenues to total bank assets, and MC_{TAit} is the marginal cost of total assets for bank i . The index thus takes values between 0 and 1, with the values of 0 and 1 reached only in the case of perfect competition and under pure monopoly, respectively. As the Lerner index is calculated as the difference between the profits on bank assets and the costs of bank operations, it captures both bank asset and funding information (Beck et al., 2013). Another advantage of the Lerner index is its independence of the definition of geographic product markets, which is quite difficult for banking sector to construe (Anginer et al., 2014). In contrast, the Lerner index does not account for product substitutability and is heavily dependent on proper price adjustment for lending risk, as the ratio of price to marginal cost should decrease in the degree of competitiveness (Beck, 2008). Alternatively, the degree of competition in the banking sector can be measured by the H-statistic, introduced by Panzar and Rosse (1987). The H-statistic measures competition by summing the elasticities of a bank's revenue with respect to its input prices. However, the H-statistic relies on the assumption that the market is in equilibrium, i.e., it assumes that increases in input prices cause total revenue and marginal cost to move together, which holds only under perfect competition (Beck, 2008). Another competition measure, the Boone (2008) indicator, applied by Schaeck and Cihak (2012), for example, expresses the effect of competition on the performance of efficient banks and offers an organization-based explanation for how competition can improve stability. The Boone indicator draws on the efficient structure hypothesis and reflects how aggressively more efficient banks make use of their cost advantage to reallocate profits from the least efficient banks in the market (Schaeck and Cihak, 2012).

In addition, concentration ratios were originally used as bank competition proxies: for instance, the Herfindahl-Hirschman index and the C3 concentration ratio, which indicates the share of the three largest banks' assets in the total assets of the country's banking system. Nevertheless, some studies (e.g. Claessens and Laeven, 2004) have shown that bank concentration is not an adequate indicator of the competitive nature of the system, as concentration and competition highlight different banking sector characteristics. In the spirit of better erring on the side of inclusion in

meta-analysis (Stanley, 2001), we also collect estimates that measure competition by the inverse of concentration, and conduct a robustness check where we exclude these estimates.

This section has illustrated how the variables used in the literature to proxy for financial stability and bank competition vary. Some of the measures are rather simplistic (for example, the NPL ratio as such cannot be regarded as a sufficient indicator of financial stability); nevertheless, we find it useful to collect all results that can be interpreted as estimates of the effect of bank competition on financial stability. By including dummy variables for different method choices and regressing the reported coefficients on these dummy variables, we can formally trace differences in the results to differences in methodology and explain the heterogeneity in the literature. By constructing a synthetic study with ideal parameters (for example, the maximum number of observations, best-practice methodology, and so on), we can place more weight on results that could be regarded as being of higher quality and thus do our best to extract a clear message from the large empirical literature on the topic.

3. The Data Set of Competition-Stability Estimates

The first step in any meta-analysis is to collect estimates from primary studies. We search for studies relevant to our meta-analysis using the Google Scholar and RePEc search engines and the following combinations of keywords: “competition” and “stability,” “competition” and “fragility,” “concentration” and “stability,” and “concentration” and “fragility.” We collect both published and unpublished studies, and try to include as many papers as possible. Since we need standard errors of the estimates to be able to use up-to-date meta-analysis methods, we have to omit studies that do not report statistics from which standard errors can be computed. In the end, we are left with 31 studies, which report 598 estimates; the oldest study in our sample was published in 2006. A typical study included in our meta-analysis uses approximately 10 years of data in its regressions and generally covers the pre-crisis period starting in the mid-to-late 1990s and ending in the mid-2000s. As for the country scope, a typical study has a cross-country focus and is oriented exclusively on developed or purely on developing and transition economies. In addition, a large majority of the studies collected ignore investigation of the persistence of financial distress by failing to include lags of financial stability measures on the right-hand side of the regression specifications. On the other hand, most studies include lags of other explanatory variables such as credit measures and bank-level variables in the regressions investigating the competition-stability nexus. We also collect 35 variables reflecting the context in which researchers obtain their estimates. Our data collection strategy, as well as all other aspects of this meta-analysis, conform to the Meta-Analysis of Economics Research Reporting Guidelines (Stanley et al., 2013).

Given the broad scope of the measures used in the literature to proxy for both bank competition and financial stability, it is imperative that we recompute the individual estimates to a common metric. Because some stability proxies measure financial fragility and some competition proxies investigate how uncompetitive the market is (for example, larger values of the Lerner index imply a less competitive nature of the system), we adjust the signs of the collected estimates so that they directly reflect the relationship between competition and stability. After this adjustment the collected estimates imply either that higher competition increases stability or that higher

competition decreases stability, and they could be compared with each other if all studies used the same units of measurement.

Due to the inconsistency in the use of measurement units of regression variables in the literature, we transform the reported estimates into partial correlation coefficients (PCCs). The PCC is a unitless measure of the strength and direction of the association between two variables, competition and stability in our case, while holding other variables constant (Stanley and Doucouliagos, 2012). The PCCs enable us to directly compare estimates reported in different studies. This technique is widely used in meta-analysis research nowadays; a related application can be found, for example, in Valickova et al. (2014).

The partial correlation coefficient is calculated according to the following formula:

$$PCC = \frac{t}{\sqrt{t^2 + df}}, \quad (4)$$

where t is the t-statistic of the reported coefficient and df denotes the number of degrees of freedom used for the estimation. The corresponding standard errors of the PCC are calculated as follows:

$$SEPCC = \sqrt{\frac{(1 - PCC^2)}{df}}. \quad (5)$$

Moreover, if the primary study assumes a quadratic relationship between competition and stability and thus reports two coefficients associated with the measure of competition, the overall impact on stability needs to be linearized using the following formula:

$$\beta = \widehat{\beta}_1 + 2\widehat{\beta}_2\bar{x} \quad SE(\beta) = \sqrt{SE(\widehat{\beta}_1)^2 + 4SE(\widehat{\beta}_2)^2\bar{x}^2}, \quad (6)$$

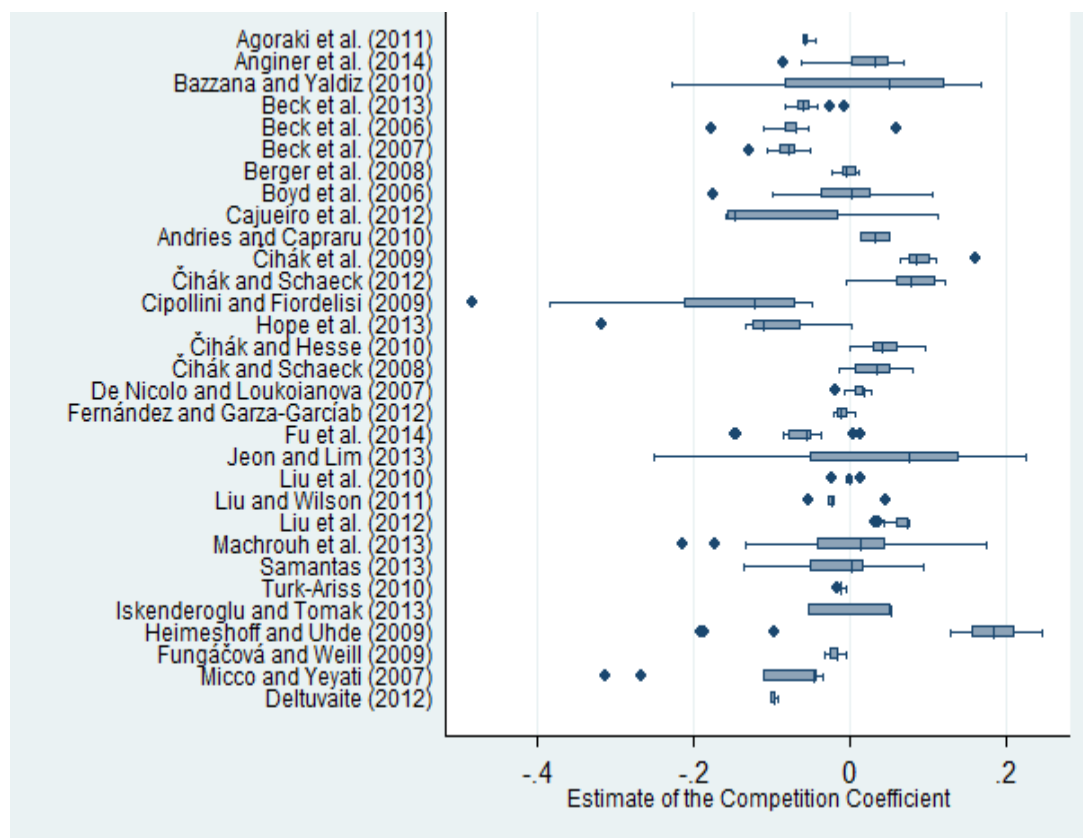
where $\widehat{\beta}_1$ is the estimate of the competition coefficient for the linear term, $\widehat{\beta}_2$ is the estimate of the competition coefficient for the quadratic term, \bar{x} is the sample mean of the competition measure in the study, $SE(\widehat{\beta}_1)$ is the standard error of the reported coefficient for the linear term, and $SE(\widehat{\beta}_2)$ is the standard error of the reported coefficient for the quadratic term. The covariance term is omitted from the $SE(\beta)$ formula due to the unavailability of the original data. The resulting coefficient of bank competition after linearization is subsequently transformed into the PCC in line with equations (4) and (5).

Figure 2 depicts the within- and between-study dispersion in the partial correlation coefficients of the competition-stability estimates reported in the 31 studies that we examine in this meta-analysis. It is apparent that the literature is highly heterogeneous, both between and within studies. Meta-analysis will help us to formally trace the sources of this heterogeneity.

Table 1 shows summary statistics for all the estimates and for two subsamples of the estimates that evaluate the effect for developed and developing countries. All the means reported in Table 1 are close to zero, indicating little interplay between competition and stability. The means of the competition coefficient estimates for developed countries are slightly larger than those for developing and transition countries. (The overall mean is slightly negative, while the means for both developing and developed countries are positive, which suggests that studies that mix these

two groups tend to find smaller estimates of the effect.) No strong inference can be made, however, as none of the reported means is significant at the 5% level of significance. Moreover, all these values are negligible and would be classified as implying no effect according to the guidelines for the interpretation of partial correlation coefficients in economics (Doucouliagos, 2011).

Figure 2: Variability in the Estimated Competition Coefficients across Individual Studies



Notes: The figure shows a box plot of the PCCs of the competition coefficient estimates (the PCCs of the β estimates from equation (1)) reported in individual studies. Full references for the studies included in the meta-analysis are available in the online

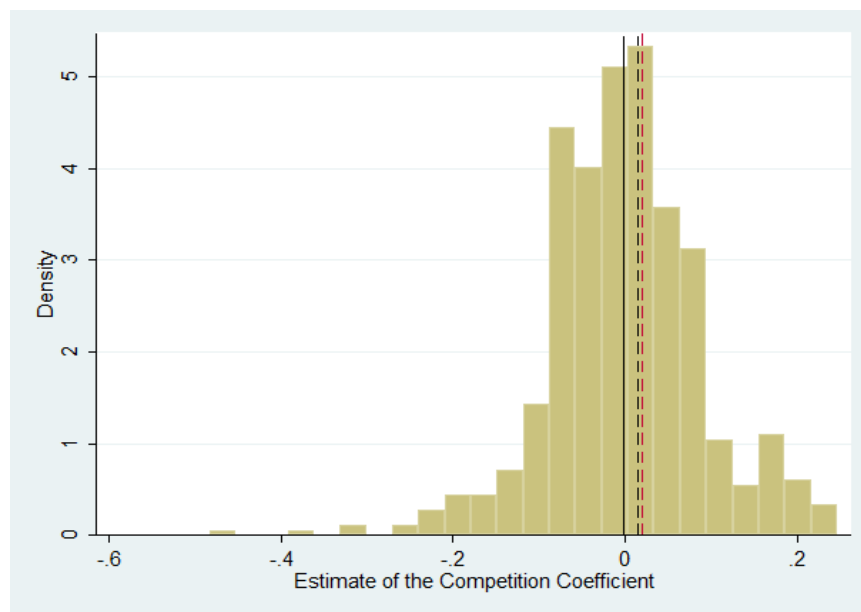
Table 1: Estimates of the Competition Effect for Different Country Groups

	<i>Unweighted</i>			<i>Weighted</i>			No. of estimates
	Mean	95% Conf. Int.		Mean	95% Conf. Int.		
All	-0.001	-0.025	0.023	-0.012	-0.035	0.011	598
Developed	0.020	-0.032	0.073	0.011	-0.030	0.052	201
Developing and transition	0.001	-0.022	0.023	-0.019	-0.051	0.012	194

Notes: The table presents the mean PCCs of the competition coefficient estimates (the PCCs of the β estimates from equation (1)) over all countries and for selected country groups. The confidence intervals around the mean are constructed using standard errors clustered at the study level. In the right-hand part of the table the estimates are weighted by the inverse of the number of estimates reported per study.

Figure 3 depicts the distribution of the partial correlation coefficients of all the competition coefficient estimates. It appears that the PCCs are symmetrically distributed around zero with a mean of -0.0009, while the mean of the study-level medians is also close to zero and equals 0.0099. We also report the mean of the PCCs of the estimates that are reported in studies published in peer-reviewed journals, as opposed to those reported in unpublished manuscripts. In total, 21 of the 31 studies in our sample were published in peer-reviewed journals, yielding 376 estimates of the competition coefficient. The mean for published studies is 0.0116: it appears that journals tend to report slightly larger estimates of the competition coefficient compared to the grey literature.

Figure 3: Studies Published in Journals Report Slightly Larger Estimates



Notes: The figure shows the histogram of the PCCs of the competition coefficient estimates (the PCCs of the β estimates from equation (1)) reported in individual studies. The solid vertical line denotes the mean of all the PCCs. The dashed lines denote the mean of the median PCCs of the estimates from the studies and the mean of the PCCs of those estimates that are reported in studies published in peer-reviewed journals, respectively.

4. Testing for Publication Bias

Publication selection bias arises when an estimate's probability of being reported depends on its sign or statistical significance. Rosenthal (1979) refers to this phenomenon as the "file drawer problem," implying that researchers may hide estimates that are either insignificant or have a counterintuitive sign in their file drawers, and seek instead to obtain new estimates that would be easier to publish. A number of studies, e.g., by DeLong and Lang (1992), Card and Krueger (1995), and Ashenfelter et al. (1999), identify publication selection bias in empirical economics. In addition, Doucouliagos and Stanley (2013) conduct a survey of meta-analyses and find that most fields of empirical economics suffer from publication bias. The bias tends to inflate the mean

estimates reported by empirical studies. For example, Doucouliagos and Stanley (2009) estimate that the adverse employment effect of minimum wage increases is seriously overstated in the published empirical literature. In our case there are opposing theories concerning the effect of competition on stability, so both positive and negative estimates are publishable, which might alleviate publication bias. In this section, we test for potential publication bias in the literature evaluating the competition-stability nexus before we proceed with the analysis of heterogeneity in the next section.

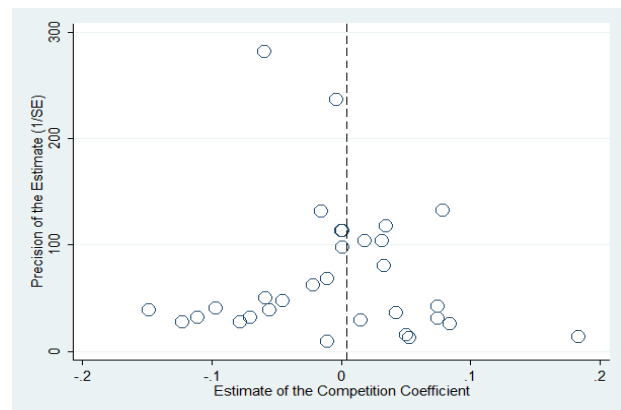
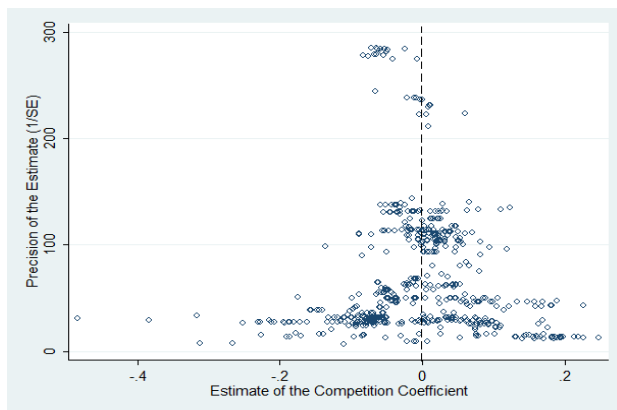
We start with visual tests for the presence of publication bias. The most commonly applied graphical test uses the so-called funnel plot (Egger et al., 1997), which depicts the magnitude of the estimated effect on the horizontal axis and precision (the inverse of the estimated standard error) on the vertical axis. The most precise estimates (located at the top of the funnel) should be close to the true underlying effect. With decreasing precision, the estimates get more dispersed; overall, they should form a symmetrical inverted funnel. If there is publication bias in the literature, the funnel is either asymmetrical due to the exclusion of estimates of a certain sign or size, or hollow due to the omission of insignificant estimates, or displays both these properties.

Figure 4A shows the funnel plot for the PCCs of all the competition coefficient estimates reported in the studies, while Figure 4B depicts the funnel plot for the median values of the PCCs of the estimates reported in individual studies. We observe that both funnels are relatively symmetrical, and the most precise estimates are close to the mean reported PCC of the estimates. Moreover, the funnels are not hollow, and even estimates with very little precision (and large p-values) at the bottom of both plots are reported. Therefore, we can infer that these funnel plots do not point to the presence of publication bias in the competition-stability literature, as opposed to the findings in most other fields in economics and finance (for example, Havranek and Irsova, 2011; Havranek and Irsova, 2012; Havranek et al., 2012).

Figure 4: Funnel Plots Do Not Suggest Strong Publication Bias

A) All estimates

B) Median estimates from studies



Notes: In the absence of publication bias the funnel should be symmetrical around the most precise estimates of the competition coefficient (the PCC of the β estimate from equation (1)). The dashed vertical lines denote the mean of the PCCs of all the estimates in Figure 4A and the mean of the study-level medians reported in Figure 4B.

A more rigorous approach to testing for publication bias consists in funnel asymmetry tests. These tests explore the relationship between the collected coefficient estimates and their standard errors following the methodology suggested by Card and Krueger (1995). In the presence of publication selection, the reported estimates are correlated with their standard errors. For example, if negative estimates are omitted, a positive relationship appears between the reported coefficient estimates and their standard errors because of heteroskedasticity in the equation (Stanley, 2008). Similarly, researchers who prefer statistical significance need large estimates to offset large standard errors. Thus, we estimate the following equation:

$$PCC_i = \beta_0 + \beta_1 SE(PCC_i) + \varepsilon_i, \quad (7)$$

where PCC_i is the partial correlation coefficient of the competition coefficient estimate, $SE(PCC_i)$ is the standard error of the partial correlation coefficient, β_0 is the mean PCC corrected for the potential publication bias, β_1 measures the extent of publication bias, and ε_i is a disturbance term. Equation (7) is commonly called the funnel asymmetry test, as it follows from rotating the axes of the funnel plot and inverting the values on the new horizontal axis so that it now shows standard errors instead of precision.¹

The results of the funnel asymmetry tests are presented in Table 2. The coefficient estimates in the upper part of the table result from fixed effects estimation with standard errors clustered at the level of individual studies and from instrumental variable estimation (where the number of observations is used as an instrument for the standard error). Fixed effects control for method or other quality characteristics specific to individual studies. We also report results for the subsample of estimates reported in published studies to see whether they show different levels of publication selection bias. The bottom half of the table presents results from regressions weighted by the inverse of the number of estimates reported per study in order to diminish the effect of studies reporting many estimates. In all specifications in Table 2, both coefficient estimates are significant at least at the 5% level. A moderate negative publication bias is present, and the estimated size of the competition-stability effect beyond publication bias appears to be close to zero, especially for weighted results. For unweighted results we obtain small effect sizes according to the guidelines by Doucouliagos (2011) for partial correlations reported in the field of industrial organization.

¹ It is worth noting at this point that authors of primary studies do not directly report partial correlation coefficients; we compute the PCCs from the statistics the authors provide. Because the PCCs are non-linear transformations of the original estimates and standard errors, a linear relation between estimates and standard errors does not translate into a linear relation between PCCs and SE (PCC). In consequence, our publication bias estimates might be biased downwards.

Table 2: Funnel Asymmetry Tests Show Moderate Publication Bias

<i>Unweighted regressions</i>	Fixed Effects	Fixed Effects_Published	Instrument	Instrument_Published
SE (publication bias)	-1.671**	-1.898**	-1.614***	-2.291***
Constant (effect beyond bias)	0.044**	0.073**	0.043***	0.086***
No. of estimates	598	376	598	376
No. of studies	31	21	31	21
<i>Weighted regressions</i>	Fixed Effects		Fixed Effects_Published	
SE (publication bias)	-1.568***		-1.636***	
Constant (effect beyond bias)	0.034***		0.044***	
No. of estimates	598		376	
No. of studies	31		21	

Notes: The table presents the results of the regression specified in equation (6). The standard errors of the regression parameters are clustered at the study level. Published = we only include published studies. Fixed Effects = we use study dummies. Instrument = we use the logarithm of the number of observations in equation (1) as an instrument for the standard error and employ study fixed effects. The regressions in the bottom half of the table are estimated by weighted least squares, where the inverse of the number of estimates reported per study is taken as the weight. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

that the meta-analyst cannot collect all relevant information on the methodology used in the primary studies. If the meta-analyst omits an aspect of methodology that influences both the reported coefficients and their standard errors in the same direction, he or she will obtain biased estimates of the magnitude of the publication bias. Our results suggest that in the case of the competition-stability nexus endogeneity is not an important issue.

Equation (7), however, suffers from heteroskedasticity, because the explanatory variable directly captures the variance of the response variable. To achieve efficiency, many meta-analysis applications divide equation (7) by the corresponding standard error, i.e., they multiply the equation by the precision of the estimates. This specification places more emphasis on precise results. Dividing equation (7) by the corresponding SE of the PCC, we obtain the following equation:

$$t_i = \beta_1 + \beta_0(1/SE(PCC_i)) + \mu_i, \quad (8)$$

where β_0 is the mean PCC of the coefficient estimate corrected for the potential publication bias, β_1 measures the extent of publication bias, and t_i is the corresponding t-statistic. Table 3 below presents results from the heteroskedasticity-corrected equation (8).

Table 3: Heteroskedasticity-corrected Funnel Asymmetry Tests Confirm the Presence of Publication Bias

<i>Weighted by precision</i>	Fixed Effects	Fixed Effects_Published	Instrument	Instrument_Published
1/SE (effect beyond bias)	0.005	0.065	0.019**	0.053***
Constant (publication bias)	-0.757	-4.000*	-1.706**	-3.344***
No. of estimates	598	376	598	376
No. of studies	31	21	31	21
<i>Weighted by precision and no. of observations</i>	Fixed Effects	Fixed Effects_Published		
1/SE (effect beyond bias)	0.013	0.056**		
Constant (publication bias)	-1.539**	-4.339**		
No. of estimates	598	376		
No. of studies	31	21		

Notes: The table presents the results of the regression specified in equation (7). The standard errors of the regression parameters are clustered at the study level. Published = we only include published studies. Fixed Effects = we use study dummies. Instrument = we use the logarithm of the number of observations in equation (1) as an instrument for the standard error and employ study fixed effects. The regressions in the bottom half of the table are additionally weighted by the inverse of the number of estimates reported per study. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

We can observe from Table 3 that publication bias is not equally strong across all specifications, in contrast to Table 2. Moreover, the true underlying effect beyond publication bias is only significant when equation (8) is estimated by means of instrumental variables or by fixed effects for the subsample of published studies. Table 3 confirms that the competition-stability effect beyond publication bias is indeed close to zero, as no estimate surpasses the threshold defined by Doucouliagos (2011) to denote at least a weak effect. The story changes for publication bias, which now seems to be much stronger in published studies than in unpublished manuscripts, which would suggest that journal editors or referees prefer papers that show results consistent with the competition-fragility hypothesis.

For evaluation of the extent of publication bias, Doucouliagos and Stanley (2013) provide guidelines for the value of the constant in the funnel asymmetry test specified by equation (8). They identify that the literature suffers from substantial selectivity if $\hat{\beta}_1$ from equation (8) is statistically significant and, at the same time, $1 \leq |\hat{\beta}_1| \leq 2$. Both conditions hold for the value of the constant estimated by fixed effects and weighted by the inverse of the number of observations, as well as for the constant in regressions estimated by the instrumental variable method. The values of the coefficient estimated in Table 3 for published studies are even larger than 2, which would suggest severe publication bias according to the guidelines by Doucouliagos and Stanley (2013). Nevertheless, we believe the overall evidence points to only moderate publication bias, because the corrected estimates of the competition-stability nexus are close to the simple mean of all the estimates uncorrected for publication bias.

5. Why the Reported Coefficients Vary

5.1 Variable Description and Methodology

In this section we add the characteristics of the studies and estimates into equation (7) to explore what drives the heterogeneity in the literature. We do not weight the resulting equation by precision as is the case in equation (8): weighting by the estimates' precision introduces artificial variation into variables that are defined at the study level (for example, the impact factor of the study) or that tend to vary little within studies (for example, sample size). In contrast, we weight the regressions by the inverse of the number of estimates reported per study to give the same importance to each study in our data set. In the next section we also perform a robustness check for regressions not weighted by the number of estimates per study.

Table 4 describes all the variables that we collect from the primary studies. For each variable the table also shows the mean, the standard deviation, and the mean weighted by the inverse of the number of estimates reported per study. For ease of exposition we divide the collected variables into eight groups.

Table 4: Overview and Summary Statistics of Regression Variables

Variable	Description	Mean	SD	WM
<i>Data characteristics</i>				
competition coefficient	The coefficient capturing the effect of bank competition on financial stability (recomputed to the partial correlation coeff.)	-0.001	0.090	-0.012
SEPCC	The estimated standard error of the competition coefficient	0.027	0.022	0.029
samplesize	The logarithm of the number of cross-sectional units used in the competition-stability regression	7.835	1.615	7.760
T	The logarithm of the number of time periods (years)	2.224	0.743	2.264
sampleyear	The mean year of the sample period on which the competition-stability regression is estimated (base: 1992,5)	8.889	4.328	9.340
<i>Countries examined</i>				
developed	equals 1 if the researcher only examines OECD countries	0.336	0.473	0.366
developing and transition	equals 1 if the researcher only examines non-OECD countries	0.324	0.469	0.376
Reference case: mixed	equals 1 if the researcher examines both OECD and non-OECD countries (omitted category)	0.339	0.474	0.258
<i>Design of the analysis</i>				
quadratic	equals 1 if the square of the competition coefficient is included in the regression	0.119	0.324	0.217
endogeneity	equals 1 if the estimation method accounts for endogeneity	0.635	0.482	0.713
macro	equals 1 if the competition-stability regression is estimated using country-level data	0.256	0.437	0.133
someAveraged	equals 1 if the competition-stability regression uses variables in the form of country-level averages over banks	0.120	0.326	0.085
<i>Treatment of stability</i>				
dummies	equals 1 if stability is measured by a crisis dummy or a bank failure dummy	0.142	0.349	0.129
NPL	equals 1 if stability is measured by non-performing loans as a share of total loans	0.050	0.218	0.095
Zscore	equals 1 if stability is measured by the Z-score statistic	0.452	0.498	0.537
profit_volat	equals 1 if stability is measured by ROA volatility or ROE volatility	0.075	0.264	0.039
profitability	equals 1 if stability is measured by ROA or ROE	0.043	0.204	0.045
capitalization	equals 1 if stability is measured by the capital adequacy ratio	0.069	0.253	0.040

	(CAR) or the equity-total assets ratio			
DtoD	equals 1 if stability is measured by Logistic R2 Merton's distance-to-default or probability of bankruptcy	0.065	0.247	0.047
<i>Reference: other_stability</i>	<i>equals 1 if stability is measured by a less frequently used method (omitted category)</i>	<i>0.104</i>	<i>0.305</i>	<i>0.069</i>
<i>Treatment of competition</i>				
Hstatistic	equals 1 if competition is measured by the H-statistic	0.090	0.287	0.098
Boone	equals 1 if competition is measured by the Boone indicator	0.075	0.264	0.108
concentration	equals 1 if competition is measured by concentration measures C3 or C5	0.157	0.364	0.147
Lerner	equals 1 if competition is measured by the Lerner index	0.360	0.480	0.414
HHI	equals 1 if competition is measured by the Herfindahl-Hirschman index	0.266	0.442	0.197
<i>Reference: other_competition</i>	<i>equals 1 if competition is measured by a less frequently used method (omitted category)</i>	<i>0.052</i>	<i>0.222</i>	<i>0.037</i>
<i>Estimation methods</i>				
Logit	equals 1 if the logit or probit model is used in the estimation of the competition-stability regression	0.172	0.378	0.161
OLS	equals 1 if OLS is used in the estimation	0.137	0.344	0.115
FE	equals 1 if fixed effects are used in the estimation	0.229	0.421	0.136
RE	equals 1 if random effects are used in the estimation	0.067	0.250	0.043
GMM	equals 1 if GMM is used in the estimation	0.182	0.386	0.309
TSLS	equals 1 if two-stage least squares are used in the estimation	0.149	0.356	0.110
<i>Reference: other_method</i>	<i>equals 1 if a less frequently used method is employed (omitted category)</i>	<i>0.064</i>	<i>0.244</i>	<i>0.126</i>
<i>Control variables</i>				
regulation	equals 1 if regulatory/supervisory variables are included in the competition-stability regression	0.239	0.427	0.282
ownership	equals 1 if bank ownership is controlled for in the competition-stability regression	0.166	0.372	0.271
global	equals 1 if macroeconomic variables are included in the competition-stability regression	0.794	0.405	0.764
<i>Publication characteristics</i>				
citations	The logarithm of the number of Google Scholar citations normalized by the difference between 2015 and the year the study first appeared in Google Scholar (collected in July 2014)	2.045	1.222	1.790
firstpub	The year when the study first appeared in Google Scholar (base: 2003)	6.453	2.979	6.677
IFrecursive	The recursive impact factor of the outlet from RePEc (collected in July 2014)	0.243	0.210	0.205
reviewed_journal	equals 1 if the study is published in a peer-reviewed journal	0.629	0.484	0.677

Notes: SD = standard deviation. WM = mean weighted by the inverse of the number of estimates reported per study. All variables except for citations and the impact factor are collected from studies estimating the competition coefficient from equation (1). The search for studies was terminated on July 1, 2014, and the list of studies included is available in the online appendix. Citations are collected from Google Scholar and the impact factor from RePEc.

Group 1 – Data characteristics: We control for the number of cross-sectional units and time periods used to estimate the competition coefficient in equation (1). Ceteris paribus, we intend to place more weight on studies that use larger samples to minimize the potential small-sample bias, and it is therefore important to check whether such studies yield systematically different results. Although being correlated with the standard error, the number of cross-sectional units and time periods bring additional information to our model, and the results can suggest whether the bias identified in the previous section is due to publication selection or small samples. Moreover, we control for the age of the data used in the primary studies by including the variable *sampleyear*, which represents the midpoint of the data period used by researchers. Although Figure 1 suggests no significant time trend in the estimates of the competition-stability nexus, perhaps the literature

can be shown to converge to a particular result when data and method heterogeneity in primary studies is controlled for.

Group 2 – Countries examined: We account for potential cross-country heterogeneity by including dummies for developed (OECD member) countries and developing and transition (non-OECD) countries. The characteristics of the banking sector (measured, for example, by the credit-to-GDP ratio) differ greatly between developed and developing countries, which can affect the results of primary studies. In our sample, 34% of all the estimates collected are obtained using a sample of developed countries, while 32% of estimates are extracted from studies focusing on developing and transition countries. The reference case for this group of dummy variables is estimation that mixes these two groups.

Group 3 – Design of the analysis: We control for the general design of the studies in our sample, captured by the variables quadratic, endogeneity, macro, and averaged. First, the dummy variable quadratic controls for the inclusion of the square of the competition measure in the regressions. In total, 12% of the estimates in our sample have to be linearized because researchers test for possible non-linear relationships between bank competition and stability (in the next section we will discuss how our results change when we conduct separate meta-analyses of the linear and quadratic term). The dummy variable endogeneity reflects whether individual studies account for potential endogeneity in their analysis, either by employing estimation methods with instruments or by using lagged values of bank competition in equation (1). Later we also include dummy variables for estimation methods, some of which control for endogeneity. Nevertheless, the correlations between these variables and endogeneity do not exceed 0.42. Next, the dummy variable macro assigns the value 1 to an estimate if the estimate is calculated using data constructed at the aggregate level, as opposed to studies using bank-level data. The motivation behind this control emerges from the narrative literature survey by Beck (2008), who notes that bank-level studies tend to obtain smaller estimates of the competition effect, perhaps because they fail to capture spillovers to other sectors of the economy. Finally, the dummy variable averaged assigns the value 1 to an estimate if the regressors in equation (1) in the original study are constructed as country-level averages over banks, even though the data are technically bank-level. This simplification decreases the variance available for the estimation, and might lead to aggregation bias. 12% of the collected competition effect estimates are extracted from studies that use explanatory variables in the form of averages over the observed period in their regressions (e.g. Berger et al., 2009; Levy Yeyati and Micco, 2007).

Group 4 – Treatment of stability: Due to the large diversity of the approaches to measuring financial stability in the literature, it is possible that a portion of the variation in the competition coefficient estimates is due to a different definition of stability. We distinguish between the seven most common approaches. Some researchers use dummy variables representing either the outbreak of a systemic banking crisis or a bank failure (e.g. Beck et al., 2006 a,b; Fungacova and Weill, 2009). Popular methods for measuring financial stability include the ratio of non-performing loans to total bank loans, the Z-score, an aggregate measure of bank stability, fluctuations in the return on assets (ROA) or the return on equity (ROE) as indicators of bank profit volatility, ROA or ROE as measures of bank profitability, measures of capitalization, the capital adequacy ratio or equity to assets ratio, and measures of distance to default. The reference case for this group of dummy variables accounts for additional approaches to quantifying financial stability that are used less frequently, such as the ratio of loan loss reserves to total

assets, the ratio of deposits to total bank liabilities, or the shareholder value ratio expressed as economic value added over the capital invested by shareholders.

Group 5 – Treatment of competition: Similarly to the indicators of stability, there is large diversity in the approaches to quantifying competition within the banking sector. We control for the five most commonly used measures. We include Panzar and Rosse's (1987) H-statistic and Boone's (2008) index. Quite frequently, measures of market structure are applied to assess the intensity of competition in the sector; concentration ratios are one such measure. For 36% of the estimates in our sample, competition is measured via the Lerner index. Herfindahl-Hirschman indices (HHI) are another example of market structure measures extensively used in the literature. Overall, market structure measures are used to compute 42% of the estimates in the sample (e.g., by Beck et al., 2006 a,b; Berger et al., 2009; Boyd et al., 2006; Cipollini and Fiordelisi, 2009). We decide to include the estimates arising from the use of these market structure measures in our analysis despite the recent assertions in the literature that concentration is not a suitable proxy for a lack of competition (e.g. Claessens and Laeven, 2004; Bikker, 2004). As a robustness check in the online appendix, we estimate the impact of competition on stability after excluding these potentially misspecified estimates from our sample. The reference case for this group of dummy variables covers alternative and infrequently used proxies of market competition, e.g. the extent of entry barriers into banking and the percentage of applications to enter banking denied (Anginer et al., 2014), a market pressure dummy (Jeon and Lim, 2013), and market power calculated as the difference between total revenues and total costs over total bank revenues (Bazzana and Yaldiz, 2010).

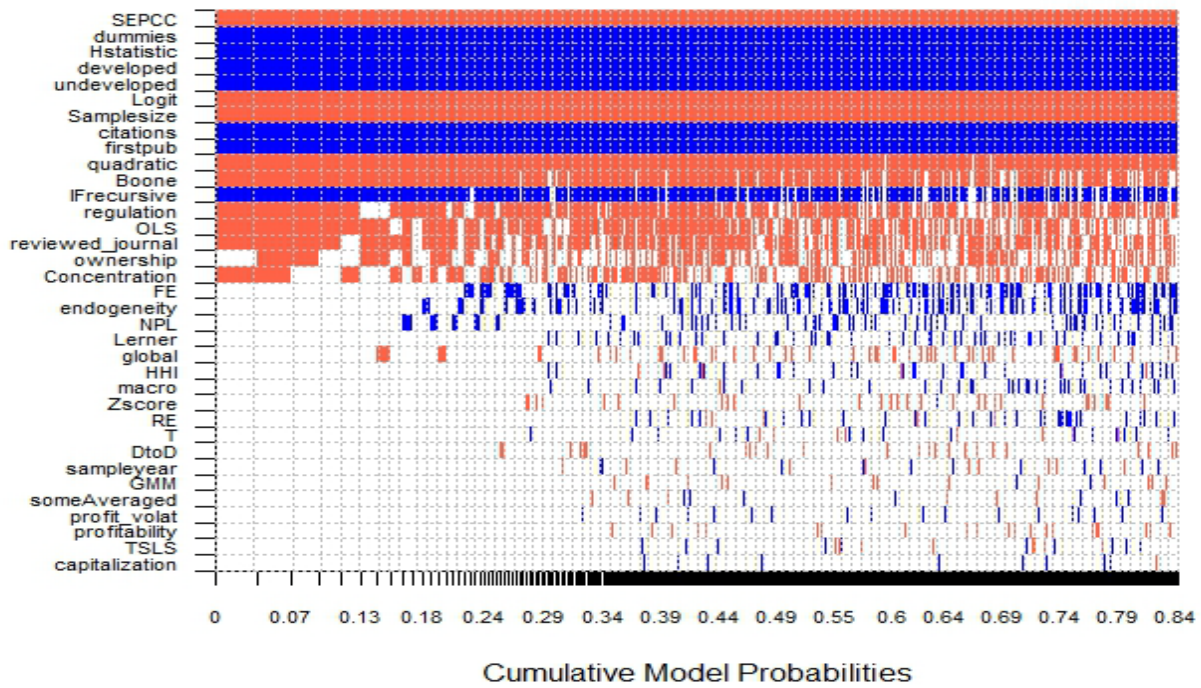
Group 6 – Estimation methods: We control for six different estimation methods in our analysis: logit, OLS, FE, RE, GMM, and TSLS. Based on the findings of many previous meta-analyses, we assume that different methods might systematically affect the resulting estimates of the competition coefficient. As to the frequency of use, 17% of estimates originate from logit estimation, 14% from OLS, 23% from fixed effects, 7% from random effects, 18% from GMM, and 15% from TSLS. In our data set the variable reflecting the use of logit is not identical to the variable that captures the use of dummy variables on the left-hand side, because some of the studies that employ dummy variables use linear estimation techniques. Moreover, other studies, e.g. Cipollini and Fiordelisi (2009), incorporate either random effects or GMM estimators into logit and probit models, which we in turn classify into the RE or GMM categories. The reference case for this group represents sporadically used estimation methods in the literature, for example Tobit regressions (Fu et al., 2014; Turk Ariss, 2010), generalized least squares (Liu et al., 2012), and weighted least squares (Levy Yeyati and Micco, 2007).

Group 7 – Control variables: The most commonly used controls in the estimation of the competition-stability relationship in equation (1) are regulatory and supervisory variables such as capital stringency, supervisory power, the investor protection index, economic and banking freedom, the share of market entry restrictions or governance (e.g. Cihak et al., 2009; Beck et al., 2006 a,b; Beck et al., 2013; Anginer et al., 2014; Agoraki et al., 2011), ownership controls, i.e., foreign and state bank ownership (e.g. Bazzana and Yaldiz, 2010; Berger et al., 2009; De Nicolo and Loukoianova, 2007) and macroeconomic variables defined at the country level, such as GDP growth and the real interest rate (Agoraki et al., 2011), trade as a share of GDP, private credit as a share of GDP (Anginer et al., 2014), the terms of trade, inflation, the M2 share of reserves (Beck et al., 2006a,b) or the exchange rate (Boyd et al., 2006). Including macroeconomic variables in

regressions in original studies aims to proxy the economic climate (e.g. Beck et al., 2006 a, b). Specifically, short-term real interest rates reflect the banks' cost of funds, which may impact bank profitability via default rates. Similarly, foreign exchange risk, measured by exchange rate depreciation and the ratio of M2 to foreign exchange reserves, captures a bank's vulnerability to abrupt capital outflows. Moreover, credit growth controls for potential large credit expansion, which can lead to asset price bubbles and, when they burst, to a subsequent crisis in the sector. Regulatory and supervisory controls are used in 24% of regressions, ownership variables are used in 17% of estimations, and macroeconomic variables are used as controls in 79% of regressions.

Group 8 – Publication characteristics: We control for study quality by including the number of citations. This control reflects additional aspects of study quality not captured by the other variables described above. Although the number of citations is an imperfect control for quality (and may also be influenced by the results of the study), we find it appealing to place more weight on highly cited studies, other things (especially data and methodology) being equal. To control for the potential time trend in the literature, we add the year when each study first appeared in Google Scholar. Another control we use to account for study quality is the recursive RePEc impact factor of the outlet. Finally, in order to evaluate whether studies published in peer-reviewed journals report systematically different estimates in comparison to unpublished studies after we control for data and methodology, we include a corresponding dummy variable.

We would like to run a regression with the PCC of the estimates of the competition coefficient as the dependent variable and all the variables introduced above as explanatory variables. Nevertheless, including all of the variables at the same time is infeasible as we would probably obtain many redundant regressors in the specification. With such a large number of explanatory variables, we initially do not know which ones should be excluded from the model. An ideal approach would be to run regressions with different subsets of independent variables to ensure that our results are robust: to this end, we employ Bayesian model averaging (BMA) to resolve the model uncertainty problem, an issue that is inevitable in meta-regression analysis. BMA runs many regressions with different subsets of all the 2^{35} possible combinations of explanatory variables (we have 35 regressors at our disposal). To make the estimation feasible, we employ the Monte Carlo Markov Chain algorithm to go through the most promising of the potential models (we use the *bms* package for R developed by Feldkircher and Zeugner, 2009). BMA gives each model a weight, which can be thought of as an analogy of the adjusted R-squared, to capture the model's fit. Finally, BMA reports weighted averages from the models for posterior mean values of regression parameters and posterior standard deviations, which capture the distribution of regression parameters across individual models. Moreover, a posterior inclusion probability (PIP) is reported for each variable to show the probability with which the variable is included in the true model. Raftery et al. (1997) and Eicher et al. (2011) provide further details on BMA in general. Detailed diagnostics of our BMA exercise can be found in Appendix A.

Figure 5: Bayesian Model Averaging – Model Inclusion

Notes: The response variable is the PCC of the estimate of the competition coefficient (the PCC of the β estimate from equation (1)). All regressions are weighted by the inverse of the number of estimates reported per study. Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures the cumulative posterior model probabilities. Numerical results of the BMA estimation are reported in Table 5. A detailed description of all the variables is available in Table 4.

5.2 Results

Figure 5 shows the results of the BMA exercise. The columns in the figure denote the individual regression models, while their width indicates the models' posterior probabilities. The variables are sorted by their PIP in descending order. If the sign of a variable's regression coefficient is positive, it is denoted by blue color (darker in grayscale). Conversely, if the sign of a variable's coefficient is negative, it is colored in red. Where a variable is excluded from a model, the corresponding cell is left blank. The horizontal axis measures the cumulative model probabilities: the models that are the most successful in explaining the heterogeneity in the estimates of the competition effect are on the left, and we can see that they include less than a half of all the variables.

Table 5: Explaining Heterogeneity in the Estimates of the Competition Coefficient

Response variable:	Bayesian model averaging			Frequentist check (OLS)		
Competition effect	Post. Mean	Post. SD	PIP	Coef.	Robust Std. Err.	P-value
<i>Data characteristics</i>						
SEPCC	-1.7883	0.2046	1.0000	-1.1940	0.6511	0.067
samplesize	-0.0367	0.0035	1.0000	-0.0240	0.0089	0.007
T	0.0005	0.0039	0.0517			
sampleyear	0.0000	0.0005	0.0456			
<i>Countries examined</i>						
developed	0.2015	0.0219	1.0000	0.1761	0.0295	0.000
developing and transition	0.1072	0.0169	1.0000	0.0985	0.0262	0.000
<i>Design of the analysis</i>						
quadratic	-0.0533	0.0124	0.9971	-0.0441	0.0128	0.001
endogeneity	0.0100	0.0212	0.2371			
macro	0.0025	0.0124	0.0699			
someAveraged	-0.0004	0.0047	0.0397			
<i>Treatment of stability</i>						
dummies	0.2115	0.0282	1.0000	0.1841	0.0194	0.000
NPL	0.0020	0.0060	0.1323			
Zscore	-0.0005	0.0027	0.0630			
profit_volat	0.0006	0.0051	0.0371			
profitability	-0.0003	0.0030	0.0354			
capitalization	0.0001	0.0029	0.0271			
DtoD	-0.0013	0.0078	0.0504			
<i>Treatment of competition</i>						
Hstatistic	0.1083	0.0217	1.0000	0.1140	0.0181	0.000
Boone	-0.0709	0.0313	0.8974	-0.0583	0.0225	0.010
concentration	-0.0185	0.0226	0.4742			
Lerner	0.0036	0.0130	0.1217			
HHI	0.0023	0.0108	0.0847			
<i>Estimation methods</i>						
Logit	-0.1874	0.0230	1.0000	-0.1599	0.0190	0.000
OLS	-0.0352	0.0244	0.7558	-0.0382	0.0184	0.038
FE	0.0113	0.0211	0.2774			
RE	0.0018	0.0115	0.0581			
GMM	-0.0003	0.0029	0.0402			
TSLS	-0.0001	0.0030	0.0323			
<i>Control variables</i>						
regulation	-0.0321	0.0197	0.7982	-0.0356	0.0138	0.010
ownership	-0.0147	0.0175	0.4811			
global	-0.0017	0.0058	0.1156			
<i>Publication characteristics</i>						
citations	0.0497	0.0092	1.0000	0.0461	0.0095	0.000
firstpub	0.0219	0.0044	1.0000	0.0233	0.0033	0.000
IFrecursive	0.1060	0.0528	0.8749	0.0964	0.0477	0.043
reviewed_journal	-0.0249	0.0186	0.7254	-0.0151	0.0142	0.289
Constant	-0.0004	NA	1.0000	-0.1184	0.0860	0.169
Studies		31			31	
Observations		598			598	

Notes: The response variable of the PCC of the β estimate from equation (1). PIP = posterior inclusion probability. Post. SD = posterior standard deviation. In the frequentist check we only include explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at the study level. More details on the BMA estimation are available in Table A1 and Figure A1. A detailed description of all the variables can be found in Table 4.

The numerical results of the BMA exercise are reported in Table 5. On the right-hand side of the table we also report the results of OLS estimation with standard errors clustered at the level of individual studies. From this “frequentist check” we exclude the variables that prove to be irrelevant for the explanation of the variability in the literature (that is, have PIP lower than 0.5). The OLS regression thus includes 15 variables identified by BMA to help explain the variation in the reported competition effects. Overall, OLS with clustered standard errors yields results consistent with BMA for variables with high inclusion probabilities. The signs of the variables’ regression parameters are the same and the size of their parameter estimates is similar as well. Therefore, we can conclude that our results are robust to error-clustering, as BMA by definition does not cluster standard errors in the estimation. Eicher et al. (2011) provide a framework for the identification of the strength of the variables’ effect in BMA. The effect of a variable is considered weak if the corresponding PIP is between 0.5 and 0.75, substantial if it is between 0.75 and 0.95, strong if it is between 0.95 and 0.99, and decisive if it exceeds 0.99.

The results of our BMA exercise support the notion of the presence of publication bias (the regression coefficient on the standard error is similar to the one presented in Section 4); it seems that positive and insignificant estimates are underreported in the literature, because researchers tend to prefer results that are consistent with the competition-fragility hypothesis. Next, the larger the size of the data sample, the smaller the reported coefficient appears to be. As for country coverage, it seems that the estimates for developed countries tend to be slightly larger than those for non-OECD countries. The use of a quadratic relationship between competition and financial stability is associated with estimates that are on average 0.05 smaller, and the corresponding variable has a decisive posterior inclusion probability. Interestingly, the choice between micro and macro data in specifying the empirical exercise in primary studies does not influence the results significantly.

When financial stability is proxied by dummy variables for financial distress, the resulting competition coefficient estimates tend to be inflated by 0.21. In contrast, the use of macro data does not affect the reported results in a systematic way. This finding is at odds with the literature survey by Beck (2008, p. 6), who notes that “while bank-level studies do not provide unambiguous findings on the relationship between competition and stability, cross-country studies point mostly to a positive relationship.” Similarly, our results contrast the finding by Schaeck and Cihak (2012), who argue that banks have higher capital ratios in more competitive environments, and thus that capitalization is one of the channels through which competition enhances stability. On the contrary, after controlling for many other method choices, we find that the use of capitalization as a proxy for stability does not affect the reported estimates of the effect of competition on stability.

As for the measures of competition, the reported estimates tend to be larger by 0.11 when Panzar and Rosse’s (1987) H-statistic is used to measure bank competition. This systematic measurement issue could be due to the fact that the H-statistic imposes restrictive assumptions on a bank’s cost function that are only valid when the market in question is in equilibrium (Beck, 2008). When competition is measured by the Boone index, the estimations yield smaller effects on stability (by 0.05) and the explanatory power of this variable measured by the PIP is substantial. Concerning the suitability of market structure measures of competition, i.e., concentration ratios and HHI, neither of these measures was selected in our BMA exercise as useful in explaining the variation in the literature. To further check the robustness of this result, we repeat the BMA analysis in the

online appendix after excluding coefficient estimates obtained from regressions where competition was proxied by measures of concentration and HHI.

Regarding estimation methods, our results suggest that estimating equation (1) by a logit or a probit model tends to decrease the competition coefficient estimates by 0.19, while estimation by ordinary least squares (therefore, ignoring potential endogeneity) causes a moderate downward bias of about 0.04. Controlling for regulatory and supervisory measures decreases the estimated coefficient by approximately 0.03, which is in line with the arguments raised by Barth, Caprio, and Levine (2004) and Beck et al. (2006 a,b).

All publication characteristics that we control for have relatively high posterior inclusion probabilities. A higher recursive impact factor and more study citations are associated with larger reported estimates. Conversely, peer-reviewed journals seem to publish estimates 0.02 smaller than those reported in unpublished manuscripts, though the inclusion probability for this variable suggests only a weak effect. Moreover, our results indicate that the reported estimates of the competition coefficient increase over time.

As a final step of our analysis, we attempt to calculate the mean estimate of the competition-stability nexus after correcting for potential misspecifications and placing greater weight on estimates published in quality outlets. This part of our analysis is the most subjective as it requires a definition of “best practice” in estimating the competition coefficient. For each variable deemed useful by the BMA exercise, i.e., with PIP larger than 0.5, we plug in a preferred value, a sample minimum or a sample maximum, or, in the case of no preference, a sample mean. Then we compute a linear combination of regression parameters and obtain the value of the partial correlation coefficient conditional on our definition of best practice. We plug in the sample maxima for the size of the data set, the recursive impact factor, and the number of citations. We also prefer if the study is published in a peer-reviewed journal, if the estimation controls for regulation measures, as a higher degree of restrictions on banks’ activities and barriers to bank entry is linked to systemic banking distress (Barth, Caprio, and Levine, 2004; Beck et al., 2006 a,b), and if the researcher uses the Boone index, a relatively novel approach to measuring competition arising from the industrial organization literature.

Because our focus rests primarily on the most precise competition coefficient estimates, we plug in the value 0 for the standard error of the PCC of the estimate (similarly as in Section 4, this approach corrects for publication bias). We also prefer if OLS is not used for the estimation of the competition-stability nexus, because it does not account for potential endogeneity. We prefer if a continuous variable is used as a proxy for stability, and if simple logit is not used for the estimation (again, because it does not allow for addressing endogeneity). We plug in zero for the dummy variable that corresponds to the assumed quadratic relation between competition and stability; in this case we have to linearize the estimates, which might induce a bias. We prefer if the H-statistic is not used in the estimation, because, as we have mentioned, it imposes restrictive assumptions on a bank’s cost function that are only valid when the market in question is in equilibrium (Beck, 2008). We plug in sample means for all the other variables.

Table 6: Best-practice Estimates of the Competition Coefficient

Best practice	Weighted				Unweighted			
	Estimate	95% Conf. Int.	Diff.		Estimate	95% Conf. Int.	Diff.	
All countries	0.022	-0.022	0.066	0.034	0.038	0.000	0.076	0.039
Developed	0.096	0.049	0.144	0.085	0.091	0.045	0.137	0.071
Developing and transition	0.019	-0.035	0.072	0.038	0.055	0.011	0.099	0.054

Notes: The table presents estimates of the competition coefficient for selected country groups implied by Bayesian model averaging and our definition of best practice. We take the regression coefficients estimated by BMA with $PIP > 0.5$ and construct fitted values of the competition coefficient conditional on control for publication characteristics and other aspects of methodology (see the text for details). Diff. = the difference between these estimates and the means reported in Table 1. The confidence intervals are constructed using study-level clustered standard errors estimated by OLS. The right-hand part of the table presents results based on the robustness check using unweighted regressions (Table 8

Table 6 summarizes the results of our best-practice estimation. Apart from the baseline results reported in the left-hand part of the table, we also report results for unweighted regressions (discussed in more detail in the next section) in the right-hand part. The column denoted “Diff.” shows the difference between the best-practice coefficient estimates and the simple means of the reported coefficients presented in Table 1 for all countries, developed countries, and developing and transition countries. In general, all the best-practice coefficient estimates are larger than the means reported in Table 1, which captures both the correction for publication bias and alleged misspecifications. Concerning the baseline results, however, only the estimate for developed countries is positive and statistically significant at the 5% level. Nevertheless, based on the guidelines for the interpretation of the size of partial correlation coefficients in economics (Doucouliagos, 2011), even the largest estimate reported in Table 6 represents merely a small effect. According to the classic Cohen (1988) guidelines, the estimate is below the threshold set for small effects. Overall, even the best-practice exercise does not reveal any important effect of bank competition on financial stability.

6. Robustness Checks

In this section we present the results of four robustness checks, which we obtain by estimating the model presented in the previous section with some modifications. First, we report the results of BMA when employing alternative priors (g-prior and model size). Second, we present the results for unweighted regressions with the same priors for BMA as in the baseline estimation in Section 5. Third, we only use frequentist methods (OLS and fixed effects). Fourth, we use inverse-variance weights, which are more common in meta-analysis.

The baseline estimation presented in the previous section employs the unit information prior for Zellner’s g-prior. In this setting, the prior contains the same amount of information as one observation in the data set, and the prior is commonly used in the literature. Moreover, the uniform model prior used in the baseline specification gives the same prior probability to each

model; Eicher et al. (2011) show that their choice of priors often delivers the best predictive performance. Nevertheless, the uniform model prior favors models with the mean number of regressors, i.e., $35/2 = 17.5$, because they are the most numerous among all the possible model combinations. Therefore, our first alternative specification uses a beta-binomial prior that places the same probability on each model size, in contrast to each model (Ley and Steel, 2009). We accompany the beta-binomial model prior with the BRIC g-prior as in Fernandez et al. (2001).

Table 7 presents the results of our BMA exercise with alternative priors. The results are qualitatively as well as quantitatively very similar to those of the baseline specification. We observe no significant differences in the magnitude of the posterior means of individual variables, and the same statement holds for their posterior inclusion probabilities. The subset of regressors identified as useful (with PIP above 0.5) fully coincides with that of the baseline specification.

Second, we run the BMA exercise with the same priors as in our baseline specification but for regressions not weighted by the inverse of the number of observations reported in studies (Table 8). In this case studies with fewer reported competition coefficient estimates become less influential in the meta-analysis, and the results are dominated by papers that produce many estimates. In this robustness check the BMA only selects 14 variables with inclusion probability higher than 0.5 as opposed to 15 variables in the baseline specification. In addition, the results of the robustness check suggest that measuring stability by means of bank profitability tends to lower the coefficient estimate by 0.03. In contrast, estimating equation (1) by fixed effects or instrumental variables increases the estimated competition coefficient by 0.05, with a decisive PIP in both cases. These findings are consistent with our results from the previous section, where we report that using OLS (which disregards endogeneity) is associated with smaller reported estimates.

Furthermore, including controls for bank ownership decreases the reported estimate by 0.06 with a decisive PIP. This finding supports the results by Barth, Caprio, and Levine (2004), who argue that bank ownership matters for stability. In particular, they find that foreign bank entry tends to be positively related to financial stability, while government ownership impacts competitiveness as well as stability in a negative way. In contrast to the baseline specification, here we do not find the following aspects important: controlling for a non-linear relationship between competition and stability, measuring competition via the Boone index, estimating equation (1) by means of OLS, controlling for regulation and supervision in the banking sector, and publication of the study in a peer-reviewed journal. As for the signs and magnitudes of the estimated coefficients for individual regressors, they broadly coincide with the baseline specification. Nevertheless, the robustness check shows less evidence for publication bias in the literature. Also, the estimated coefficients for dummy variables reflecting developed and developing countries are much smaller, shrinking the difference between the implied competition coefficients for the different country groups.

Table 7: Results with Alternative BMA Priors

Response variable:	Bayesian model averaging			Frequentist check (OLS)		
Competition effect	Post. Mean	Post. SD	PIP	Coef.	Robust Std. Err.	P-value
<i>Data characteristics</i>						
SEPCC	-1.7527	0.2120	1.0000	-1.1940	0.6511	0.067
samplesize	-0.0362	0.0036	1.0000	-0.0240	0.0089	0.007
T	0.0003	0.0034	0.0373			
sampleyear	0.0000	0.0005	0.0335			
<i>Countries examined</i>						
developed	0.1976	0.0248	1.0000	0.1761	0.0295	0.000
developing and transition	0.1030	0.0188	1.0000	0.0985	0.0262	0.000
<i>Design of the analysis</i>						
quadratic	-0.0517	0.0141	0.9884	-0.0441	0.0128	0.001
endogeneity	0.0159	0.0269	0.3037			
macro	0.0028	0.0132	0.0672			
someAveraged	-0.0004	0.0043	0.0310			
<i>Treatment of stability</i>						
dummies	0.2179	0.0315	1.0000	0.1841	0.0194	0.000
NPL	0.0012	0.0047	0.0818			
Zscore	-0.0004	0.0023	0.0427			
profit_volat	0.0004	0.0043	0.0255			
profitability	-0.0002	0.0024	0.0236			
capitalization	0.0001	0.0024	0.0186			
DtoD	-0.0007	0.0060	0.0313			
<i>Treatment of competition</i>						
Hstatistic	0.1074	0.0228	1.0000	0.1140	0.0181	0.000
Boone	-0.0637	0.0375	0.8020	-0.0583	0.0225	0.010
concentration	-0.0182	0.0244	0.4183			
Lerner	0.0032	0.0128	0.0946			
HHI	0.0021	0.0107	0.0659			
<i>Estimation methods</i>						
Logit	-0.1883	0.0237	1.0000	-0.1599	0.0190	0.000
OLS	-0.0296	0.0265	0.6208	-0.0382	0.0184	0.038
FE	0.0160	0.0258	0.3261			
RE	0.0020	0.0119	0.0521			
GMM	-0.0002	0.0023	0.0272			
TSLS	-0.0002	0.0031	0.0258			
<i>Control variables</i>						
regulation	-0.0313	0.0205	0.7625	-0.0356	0.0138	0.010
ownership	-0.0129	0.0176	0.4014			
global	-0.0013	0.0051	0.0837			
<i>Publication characteristics</i>						
citations	0.0476	0.0101	1.0000	0.0461	0.0095	0.000
firstpub	0.0207	0.0050	1.0000	0.0233	0.0033	0.000
IFrecursive	0.0958	0.0622	0.7699	0.0964	0.0477	0.043
reviewed_journal	-0.0211	0.0198	0.6028	-0.0151	0.0142	0.289
Constant	-0.0004	NA	1.0000	-0.1184	0.0860	0.169
Studies		31			31	
Observations		598			598	

Notes: The response variable is the competition effect. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we only include explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at the study level. In this specification we use the beta-binomial prior advocated by Ley & Steel (2009) (the prior model probabilities are the same for all model sizes) and the BRIC g-prior following Fernandez et al. (2001). More details on the BMA estimation are available in Table A2 in Appendix A. A detailed description of all the variables is available in Table 4.

Table 8: Results for Unweighted Regressions

Response variable:	Bayesian model averaging			Frequentist check (OLS)		
Competition effect	Post. Mean	Post. SD	PIP	Coef.	Robust Std. Err.	P-value
<i>Data characteristics</i>						
SEPCC	-0.7259	0.5667	0.7003	-0.5768	0.7862	0.4630
samplesize	-0.0258	0.0082	1.0000	-0.0248	0.0092	0.0070
T	0.0008	0.0034	0.0735			
sampleyear	0.0006	0.0015	0.1946			
<i>Countries examined</i>						
developed	0.1529	0.0172	1.0000	0.1519	0.0175	0.0000
developing and transition	0.1127	0.0172	1.0000	0.1156	0.0170	0.0000
<i>Design of the analysis</i>						
quadratic	0.0012	0.0050	0.0755			
endogeneity	0.0056	0.0110	0.2461			
macro	-0.0103	0.0161	0.3408			
someAveraged	0.0000	0.0024	0.0219			
<i>Treatment of stability</i>						
dummies	0.1861	0.0281	1.0000	0.1660	0.0176	0.0000
NPL	0.0138	0.0249	0.2739			
Zscore	0.0091	0.0166	0.2660			
profit_volat	0.0176	0.0238	0.4350			
profitability	-0.0281	0.0233	0.6587	-0.0451	0.0246	0.0660
capitalization	0.0101	0.0196	0.2437			
DtoD	-0.0015	0.0080	0.0674			
<i>Treatment of competition</i>						
Hstatistic	0.1294	0.0223	1.0000	0.1123	0.0173	0.0000
Boone	-0.0021	0.0088	0.0873			
concentration	0.0159	0.0244	0.3626			
Lerner	0.0136	0.0211	0.3566			
HHI	0.0103	0.0199	0.2488			
<i>Estimation methods</i>						
Logit	-0.1304	0.0303	0.9999	-0.1275	0.0121	0.0000
OLS	0.0000	0.0019	0.0214			
FE	0.0621	0.0134	1.0000	0.0503	0.0113	0.0000
RE	0.0128	0.0204	0.3355			
GMM	0.0000	0.0018	0.0221			
TSLS	0.0532	0.0132	0.9999	0.0515	0.0147	0.0000
<i>Control variables</i>						
regulation	0.0002	0.0020	0.0281			
ownership	-0.0595	0.0096	1.0000	-0.0588	0.0289	0.0420
global	0.0016	0.0054	0.1033			
<i>Publication characteristics</i>						
citations	0.0377	0.0063	0.9996	0.0407	0.0087	0.0000
firstpub	0.0179	0.0033	0.9997	0.0205	0.0029	0.0000
IFrecursive	0.0470	0.0419	0.6405	0.0490	0.0379	0.1960
reviewed_journal	0.0019	0.0080	0.0807			
Constant	-0.1269	NA	1.0000	-0.1263	0.0870	0.1460
Studies		31			31	
Observations		598			598	

Notes: The response variable is the competition effect. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we only include explanatory variables with PIP > 0.5. The standard errors in the frequentist check are clustered at the study level. In this specification we do not weight the regressions by the inverse of the number of estimates reported per study. More details on the BMA estimation are available in Table A3 in Appendix A. A detailed description of all the variables is available in Table 4.

Third, we only use frequentist methods in estimations; the results are reported in Table 9. The left-hand panel of the table shows OLS, while the right-hand panel shows fixed effects estimation. The advantage of fixed effects estimation is that it removes all idiosyncratic effects of individual studies (such as study quality) on the results, but it also automatically removes all variables constant within studies and greatly reduces the variance of some of the method variables. The results are, in general, consistent with our baseline estimation: in both specifications we find evidence for downward publication bias, and the best-practice estimate of the effect of bank competition on financial stability is small and statistically insignificant (see Table C11 in the online appendix for best-practice estimation related to this robustness check).

There are, of course, some differences in the estimated magnitude and statistical significance for some of the moderator variables. Using OLS, we find that estimates obtained with the Lerner index as a proxy for competition tend to be somewhat larger than those computed with other competition proxies. Moreover, the use of fixed effects in primary studies tends to be associated with larger reported coefficients. Apart from that, it seems that Bayesian and frequentist methods yield very similar results. We observe more differences when we use study fixed effects, which wipe out a large portion of the variance in our data. With fixed effects, the variable controlling for the number of cross-sectional units used in the primary study becomes insignificant. In contrast, the dummy variable that equals one for macro-level studies becomes statistically significant and large, indicating that macro studies report much larger estimates of the competition-stability nexus compared to bank-level studies. The choice of the measure of competition seems to be more important now than in our baseline estimation. Interestingly, however, the two dummy variables that reflect the use of competition proxies based on concentration (variables HHI and concentration) are not jointly statistically significant – which we corroborate in the online appendix by conducting a separate meta-analysis of the estimates that are obtained using concentration measures. Estimation methods influence the results significantly: the use of OLS, fixed effects, and GMM is associated with larger reported coefficients than the use of other estimation methods.

Fourth, we estimate our baseline model with weights based on the inverse variance of the reported estimates instead of the inverse of the number of estimates reported in a study. The left-hand panel of Table 10 shows the results of BMA; the right-hand panel shows OLS. Once again, we find evidence for downward publication bias: the corresponding variable has high posterior inclusion probability, although in the OLS regression its statistical significance decreases (nevertheless, that is due to the inclusion of many potentially redundant variables in the frequentist setting). The best-practice estimate, reported in Table C12 in the online appendix, once again shows no effect of bank competition on financial stability, which is consistent with our previous results. The individual regression coefficients are broadly similar to the baseline case, with the exception of the choice of a proxy for stability: now it seems to be more important, and each choice can be expected to yield different results. Similarly to the previous case, however, the largest difference is caused by the use of a binary variable on the left-hand side in primary studies; this method choice is associated with competition-stability estimates 0.2 larger compared with the reference case.

Table 9: Results for Frequentist Methods

Response variable:	OLS			Fixed effects		
		Robust Std.			Robust Std.	
Competition effect	Coef.	Err.	P-value	Coef.	Err.	P-value
<i>Data characteristics</i>						
SEPCC	-1.5708	0.8567	0.067	-1.6234	0.6912	0.026
samplesize	-0.0363	0.0110	0.001	0.0148	0.0212	0.491
T	0.0141	0.0107	0.188	-0.0511	0.0268	0.067
sampleyear	0.0040	0.0033	0.222	0.0057	0.0032	0.082
<i>Countries examined</i>						
developed	0.1689	0.0211	0.000	(omitted)		
undeveloped	0.1008	0.0166	0.000	0.1020	0.0760	0.189
<i>Design of the analysis</i>						
quadratic	-0.0080	0.0204	0.694	-0.0071	0.0135	0.604
endogeneity	0.0240	0.0292	0.410	-0.0292	0.0163	0.084
macro	-0.0040	0.0364	0.914	0.1882	0.0138	0.000
someAveraged	-0.0023	0.0285	0.935	0.0226	0.0151	0.146
<i>Treatment of stability</i>						
dummies	0.2232	0.0373	0.000	(omitted)		
NPL	0.0299	0.0259	0.250	0.0239	0.0232	0.310
Zscore	0.0116	0.0249	0.641	0.0172	0.0228	0.456
profit_volat	0.0284	0.0206	0.168	0.0192	0.0214	0.378
profitability	-0.0142	0.0270	0.600	-0.0048	0.0270	0.860
capitalization	0.0184	0.0240	0.443	0.0052	0.0254	0.838
DtoD	-0.0157	0.0337	0.641	0.0217	0.0284	0.452
<i>Treatment of competition</i>						
Hstatistic	0.1629	0.0308	0.000	0.0577	0.0201	0.007
Boone	0.0010	0.0271	0.970	0.0744	0.0112	0.000
concentration	0.0351	0.0356	0.324	0.0709	0.0346	0.050
Lerner	0.0485	0.0188	0.010	0.0721	0.0189	0.001
HHI	0.0444	0.0257	0.084	0.0654	0.0252	0.014
<i>Estimation methods</i>						
Logit	-0.1481	0.0405	0.000	(omitted)		
OLS	-0.0022	0.0218	0.919	0.0225	0.0108	0.045
FE	0.0624	0.0247	0.011	0.0392	0.0180	0.038
RE	0.0317	0.0382	0.406	-0.0042	0.0182	0.819
GMM	0.0014	0.0159	0.932	0.0437	0.0206	0.043
TSLs	0.0393	0.0230	0.087	0.0223	0.0186	0.239
<i>Control variables</i>						
regulation	-0.0184	0.0138	0.181	0.0062	0.0104	0.558
ownership	-0.0341	0.0227	0.133	-0.0193	0.0311	0.539
global	0.0112	0.0176	0.524	0.0239	0.0152	0.125
<i>Publication characteristics</i>						
citations	0.0408	0.0146	0.005	(omitted)		
firstpub	0.0159	0.0067	0.017	(omitted)		
IFrecursive	0.0890	0.0363	0.014	(omitted)		
reviewed_journal	-0.0042	0.0271	0.876	(omitted)		
Constant	-0.1350	0.1124	0.230	-0.1783	0.1656	0.290
Studies		31			31	
Observations		598			598	

Notes: The response variable of the PCC of the β estimate from equation (1). PIP = posterior inclusion probability. Post. SD = posterior standard deviation. In the frequentist check we include all explanatory variables. The standard errors in the frequentist check are clustered at the study level. The regressions are estimated by weighted least squares, where the inverse of the number of estimates reported per study is taken as the weight. The left-hand side of the table presents the results of OLS regression while the right-hand part presents the results of fixed effects regression. A detailed description of all the variables can be found in Table 4.

Table 10: Results for Specifications Weighted by Inverse Variance

Response variable:	Bayesian model averaging			OLS		
Competition effect	Post. Mean	Post. SD	PIP	Coef.	Robust Std. Err.	P-value
<i>Data characteristics</i>						
SEPCC	-1.4216	NA	1.0000	-1.0152	0.9359	0.278
samplesize	-0.0279	0.0039	1.0000	-0.0276	0.0067	0.000
T	0.0031	0.0054	0.3013	0.0177	0.0097	0.069
sampleyear	0.0001	0.0007	0.0828	0.0043	0.0034	0.201
<i>Countries examined</i>						
developed	0.1266	0.0074	1.0000	0.1257	0.0158	0.000
undeveloped	0.0730	0.0066	1.0000	0.0729	0.0216	0.001
<i>Design of the analysis</i>						
quadratic	0.0007	0.0029	0.0817	0.0059	0.0120	0.620
endogeneity	0.0004	0.0034	0.0935	0.0060	0.0187	0.747
macro	-0.0035	0.0071	0.2561	-0.0123	0.0190	0.519
someAveraged	0.0003	0.0023	0.0644	0.0010	0.0125	0.934
<i>Treatment of stability</i>						
dummies	0.2132	0.0269	1.0000	0.2346	0.0240	0.000
NPL	0.0441	0.0096	0.9996	0.0427	0.0231	0.065
Zscore	0.0384	0.0065	1.0000	0.0377	0.0192	0.049
profit_volat	0.0580	0.0075	1.0000	0.0576	0.0187	0.002
profitability	0.0134	0.0122	0.6193	0.0193	0.0153	0.207
capitalization	0.0407	0.0076	1.0000	0.0402	0.0228	0.078
DtoD	0.0703	0.0106	1.0000	0.0776	0.0303	0.010
<i>Treatment of competition</i>						
Hstatistic	0.0795	0.0134	1.0000	0.0866	0.0309	0.005
Boone	0.0000	0.0020	0.0414	0.0153	0.0155	0.323
concentration	-0.0002	0.0030	0.0427	0.0084	0.0220	0.701
Lerner	0.0000	0.0011	0.0419	0.0058	0.0055	0.297
HHI	0.0001	0.0014	0.0446	0.0108	0.0091	0.234
<i>Estimation methods</i>						
Logit	-0.1250	0.0275	0.9995	-0.1366	0.0352	0.000
OLS	-0.0002	0.0020	0.0481	0.0111	0.0227	0.626
FE	0.0698	0.0077	1.0000	0.0757	0.0207	0.000
RE	-0.0008	0.0074	0.0483	-0.0235	0.0619	0.704
GMM	0.0002	0.0017	0.0469	-0.0005	0.0197	0.980
TSLs	0.0504	0.0063	1.0000	0.0559	0.0218	0.010
<i>Control variables</i>						
regulation	0.0003	0.0014	0.0723	0.0049	0.0059	0.409
ownership	-0.0028	0.0069	0.1872	-0.0255	0.0210	0.226
global	0.0005	0.0020	0.0854	0.0083	0.0143	0.561
<i>Publication characteristics</i>						
citations	0.0226	0.0074	0.9398	0.0282	0.0131	0.032
firstpub	0.0094	0.0031	0.9350	0.0050	0.0058	0.389
IFrecursive	-0.0007	0.0049	0.0575	-0.0131	0.0446	0.768
reviewed_journal	0.0028	0.0084	0.1445	0.0108	0.0213	0.612
Constant	-0.0004	0.0114	0.0457	-0.0845	0.0856	0.324
Studies		31			31	
Observations		598			598	

Notes: The response variable of the PCC of the β estimate from equation (1). PIP = posterior inclusion probability. Post. SD = posterior standard deviation. In the frequentist check we include all explanatory variables. The standard errors in the frequentist check are clustered at the study level. The regressions are estimated by weighted least squares, where the inverse of the estimates' variance is taken as the weight. A detailed description of all the variables can be found in Table 4.

We provide more robustness checks and additional results in the online appendix. Pages 1–6 of the “additional results” file in the online appendix describe how excluding estimates produced using a concentration-based proxy for bank competition does not alter our main conclusions. Next, as an anonymous referee suggests, the strength of publication bias in the literature may be associated with the affiliations of the authors of primary studies. We try to estimate the funnel asymmetry test for the subsample of studies written by researchers not affiliated with policy institutions (such as central banks, ministries, and supra-national institutions). The results, reported in Table C1, show that the coefficient for publication bias loses statistical significance. Nevertheless, this finding is mostly due to the decreased number of degrees of freedom available for the estimation. Table C2 includes an interaction term of the standard error and a dummy variable for studies co-authored by researchers affiliated with policy institutions; the interaction is insignificant, which implies that the extent of publication bias is similar among these two groups of studies. Moreover, the underlying effect of competition on stability corrected for any potential bias is small and statistically insignificant in all these specifications.

Our data sample includes several studies that were published in the same journal. Patterns of publication selection might vary across journals, so in Table C3 we exclude all studies published in journals from which we have more than one study. Similarly to the case of excluding studies co-authored by researchers affiliated with policy institutions, the statistical significance of the publication bias coefficient decreases because we have far fewer observations available in the regression. Nevertheless, the two groups of studies do not differ substantially in the magnitude of publication bias, as illustrated by Table C4, where we add an interaction of the standard error and a dummy variable that equals one for journals that provide more than one study for our data set.

Next, we investigate potential bi-directional publication bias in the literature. The disadvantage of the classical funnel asymmetry test is that it only identifies publication bias in one direction. If insignificant estimates, both positive and negative, are discarded, the coefficient on the standard error will be a biased estimate of the extent of publication selection in the literature (nevertheless, the estimate of the underlying effect corrected for the bias will be unbiased, because the classical funnel asymmetry test effectively filters out the net publication bias, either downward or upward). Authors that favor the competition-stability hypothesis might treat negative estimates with suspicion, while authors preferring the competition-fragility hypothesis might tend to discard positive estimates. The results of the test of bi-directional bias are reported in Tables C5 and C6. We follow Bom and Ligthart (2014) and replace the standard error in the funnel asymmetry test by interactions of the standard error and dummy variables that equal one if the estimate of the competition-stability nexus is positive and negative, respectively. In most specifications the estimated coefficients for these interaction terms are quantitatively similar, but we reject the hypothesis that they are equal. Our results are thus consistent with the presence of some bi-directional publication selection, and we conclude that our estimates of the extent of publication selection presented in Section 4 are probably downward biased. The corrected mean effect of competition on stability is still close to zero.

A third of the studies in our sample investigate potential non-linearity in the effect of competition on stability by including both a linear and quadratic form of the competition measure on the right-hand side of the regression. Our default approach in this meta-analysis is to approximate the first-order effect using the sample mean of the competition proxy and the delta method to calculate the corresponding standard error. In Tables C7 and C8 we present separate funnel asymmetry tests for

the estimates of linear and quadratic terms reported in studies that include both terms in the regression. We find some evidence for downward publication bias among the estimates of the linear term, which is consistent with our baseline results (but now, with far fewer degrees of freedom, our results are less precise, reducing the statistical significance). More importantly, the estimates of the mean effect for both the linear and quadratic term are virtually zero, which corroborates our conclusion that, on average, the available empirical literature does not point to any relationship between bank competition and financial stability. Further, in Tables C9 and C10 we show that excluding non-linear estimates does not alter our conclusions concerning publication bias in the literature: the non-linear estimates show a similar pattern of publication selection. Our results are consistent with several studies that fail to find non-linearity in the effect of competition on stability, such as Agoraki et al. (2011), Turk Ariss (2010), and Fungacova and Weill (2009).

Several general remarks on our methodology are in order. We prefer to use weights based on the inverse of the number of estimates presented in each paper in contrast to weights based on the inverse variance of each estimate, which are typically employed in meta-analysis. We have five reasons for this choice. First, although multiple Monte Carlo simulations (for example, Stanley and Doucouliagos, 2015) show that inverse-variance weights yield optimal results in meta-analysis, these simulations do not consider the case where each study reports several estimates of the effect in question, and moreover if the number of estimates per study varies. When weights are not constant across panels, the interpretation of the weighted results with panel data is unclear, which is why some statistical packages (for example, Stata) do not allow the use of such weights with panel estimators. Second, in applications of meta-analysis researchers typically include variables defined at the level of individual studies, such as the number of citations or publication year. With multiple estimates reported per study the introduction of inverse-variance weighting introduces artificial variation into the study-level variables, because they suddenly vary within-studies (and are heavily correlated with other weighted variables). Again, it is not clear how to interpret such results, and there have been no Monte Carlo simulations to help us with inference.

Third, in meta-analysis the reported standard errors are likely to be endogenous with respect to the reported point estimates. Certain method choices (for example, simple OLS versus instrumental variables) influence both the standard errors and the point estimates. If the influence of the method on the two statistics goes in the same direction, a large coefficient in the funnel asymmetry test may simply reflect this endogeneity instead of any publication or small-sample bias (moreover, as meta-analysis becomes better known in economics, standard errors themselves might become the target of publication selection in order for researchers to increase the weight of their results in meta-analyses). One solution is to use the inverse of the square root of the number of observations as an instrument for the standard error, because this instrument is proportional to the standard error but not likely to be correlated with method choices. It is unclear how to interpret the results of a specification where the weights employed are potentially endogenous to both the response and explanatory variable.

Fourth, inverse-variance weights are highly sensitive to outliers in precision. In most meta-analyses there are a couple of studies that report very small standard errors for no obvious reasons other than idiosyncratic methodology, and very often they also report small point estimates (this issue is connected to the endogeneity problem). The meta-analyst can either omit these studies, which is difficult to justify, winsorize these observations (as in Havranek et al., 2015b), or include them as they are. The differences between these three approaches increase dramatically when

inverse-variance weights are used. Fifth, the weights based on the inverse of the number of observations reported in a study give each study the same importance, which in our opinion is more intuitive than giving each study a weight based on the number of estimates it reports (which is what happens when we do not use our preferred weights). Certainly more research is needed to determine the optimal weighting scheme in meta-analysis with panel data. An important step in this direction is presented by Reed et al. (2015), but unfortunately they do not consider the case where different primary studies report a different number of empirical estimates.

A second non-standard feature of our analysis is the reliance on Bayesian model averaging instead of the frequentist methods used in most economics meta-analyses (especially OLS or its inverse-variance-weighted variations). While we show that using frequentist instead of Bayesian methods would not change our main results much, we prefer to use BMA. A common objection to BMA is the claim that the method is atheoretical, throwing in many potential explanatory variables and using statistical techniques to find the most important ones. The problem is that in meta-analysis we always have a large number of explanatory variables that might (or might not) potentially influence the reported point estimates. For some of them our economic intuition is stronger and for some of them it is weaker; nevertheless, we want to control for all the major aspects of data, methodology, and publication characteristics (as recommended by Stanley et al., 2013). The economic theory rarely helps us decide which of the variables we should omit, and the choice between BMA and OLS with sequential t-tests (the standard approach in meta-analysis) is not connected to this issue. Sequential t-tests are not statistically valid, because each subsequent test does not take into account that the result is conditional on the previous one. BMA, in contrast, can be thought of as an extension of the typical frequentist practice in which different specifications with various control variables are estimated to evaluate the robustness of the results.

We admit, however, that in our experience BMA and sequential t-tests often yield similar results, although there is no reason why this finding should hold in general. Then a meta-analyst faces a trade-off between a method that is statistically valid and one that is easier to compute. We opt for the first one, and would recommend other meta-analysts to do so when the number of potential explanatory variables in the meta-analysis is large (10 may be an acceptable rule of thumb, although the threshold is obviously arbitrary). With less than 10 variables we believe the meta-analyst does not have to resort to sequential t-tests, but can simply evaluate the OLS regression with all variables, and additionally perform several robustness checks. Most economics meta-analyses, however, have more than 10 explanatory variables, which makes BMA an attractive method for this field, because it helps tackle model and parameter uncertainty. BMA, however, does not help with multicollinearity – with so many variables included in the meta-regression, the multicollinearity problem is inevitable, and we therefore focus on the best-practice estimate of the competition-stability nexus (a linear combination of the regression coefficient and our definition of best practice) rather than on individual regression coefficients. BMA techniques similar to those employed in this paper have already been used in economics meta-analyses by Moeltner and Woodward (2009), Irsova and Havranek (2013), and Havranek and Irsova (2015). Havranek et al. (2015a) propose a modification for the case where a group of explanatory variables are strongly predicted to be important by economic theory: these variables are fixed in BMA, which means that they are included in all the estimated models, while the subsets of control variables vary.

7. Concluding Remarks

We conduct a meta-regression analysis of 598 estimates of the relationship between bank competition and financial stability reported in 31 studies. We complement the previous narrative reviews of the literature (Beck, 2008; Carletti and Hartmann, 2002) with a formal treatment of publication bias and heterogeneity in estimations of the competition-stability nexus. Our results suggest that the mean reported estimate of the relationship is close to zero, even after correcting for publication bias and potential misspecification problems. We find evidence for publication selection against positive results; that is, some authors of primary studies tend to discard estimates inconsistent with the competition-fragility hypothesis. To uncover the dependence of the reported estimates on the aspects of study design, we employ Bayesian model averaging, which helps us address model uncertainty.

Our results indicate that data characteristics matter for the reported coefficients corresponding to the competition-stability nexus. Researchers who use heterogeneous samples of countries (including both developed and developing economies) tend to obtain smaller estimates. The effect of competition on stability is larger in developed countries, but even there the positive effects do not seem to be strong. Next, accounting for potential non-linearities in the effect of competition on stability is important and typically yields smaller estimates of the competition-stability nexus. We also find that, in general, researchers who have more data at their disposal tend to report smaller estimates. In contrast, it does not seem to matter for the results whether the authors of primary studies use micro or macro data.

Furthermore, we show that the definition of the proxy for financial stability is important for the results of primary studies. For example, if dummy variables (usually indicating financial crises) are used as a proxy for stability, the authors tend to report much larger estimates than when a continuous measure of financial stability is used. In a similar vein, the results of primary studies are systematically affected by the choice of the proxy for bank competition. Studies using the H-statistic tend to report larger estimates of the competition-stability nexus, while studies that employ the Boone index usually show smaller estimates; nevertheless, we find no evidence of systematic differences between the results of the studies that use competition measures and the studies that use concentration as a proxy for competition. Next, if the researchers ignore the endogeneity problem in regressing financial stability on bank competition, they tend to underestimate the effect.

We also find that controlling for supervisory and regulatory conditions in regressions usually decreases the reported estimates, which supports the notion that banking systems with more activity restrictions and greater barriers to entry are more likely to suffer from systemic financial distress (Beck et al., 2006 a,b). Finally, studies that receive more citations and are published in journals with a high impact factor tend to report larger estimates of the competition-stability nexus. In the last step of our analysis we construct a weighted average of all the estimates and give more weight to the ones whose authors avoid potential misspecifications (such as ignoring endogeneity) and that have better publication characteristics (for example, more citations). Because several potential misspecifications influence the results in opposite ways, the resulting estimate still points to a very weak or non-existent link between bank competition and financial stability.

The principal limitation of meta-analysis is that it can only correct for problems in the literature that have already been addressed by some researchers. If, in contrast, all studies in the field share a common misspecification that causes a systematic bias, meta-analysis gives biased results as well. It is possible that the underlying effect of banking competition on financial stability is nonzero, but that the data and methods that are currently used in the literature do not allow researchers to identify such an effect. Nevertheless, we show that the bulk of the existing empirical literature provides little support for either the competition-fragility or competition-stability hypothesis.

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Appendix A: BMA Diagnostics

Table A1: Summary of BMA Estimation, Baseline Estimation

<i>Mean no. regressors</i>	<i>Draws</i>	<i>Burnins</i>	<i>Time</i>	<i>No. models visited</i>
16.7873	2.00E+06	1.00E+06	8.946665 mins	428100
<i>Modelspace 2^K</i>	<i>% visited</i>	<i>% Topmodels</i>	<i>Corr PMP</i>	<i>No. Obs.</i>
3.4e+10	0.0012	85	0.9991	598
<i>Model Prior</i>	<i>g-Prior</i>		<i>Shrinkage-Stats</i>	
uniform / 17.5	UIP		Av=0.9983	

Notes: In this specification we employ the priors suggested by Eicher et al. (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure A1: Model Size and Convergence, Baseline Estimation

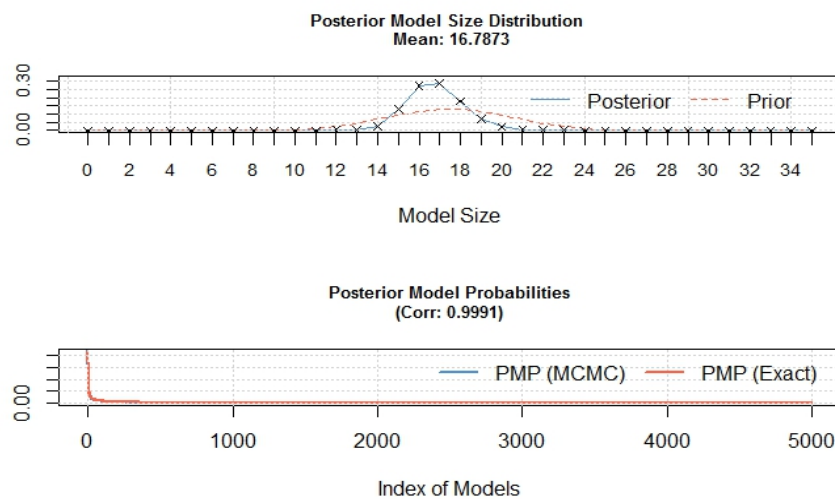


Table A2: Summary of BMA Estimation, Alternative Priors

<i>Mean no. regressors</i>	<i>Draws</i>	<i>Burnins</i>	<i>Time</i>	<i>No. models visited</i>
15.9075	2.00E+06	1.00E+06	9.343995 mins	340418
<i>Modelspace 2^K</i>	<i>% visited</i>	<i>% Topmodels</i>	<i>Corr PMP</i>	<i>No. Obs.</i>
3.4e+10	0.00099	92	0.9991	598
<i>Model Prior</i>	<i>g-Prior</i>		<i>Shrinkage-Stats</i>	
random / 17.5	BRIC		Av=0.9992	

Notes: The “random” model prior refers to the beta-binomial prior used by Ley & Steel (2009); the prior model probabilities are the same for all possible model sizes. In this specification we set Zellner’s g prior in line with Fernandez et al. (2001).

Figure A2: Model Size and Convergence, Alternative Priors

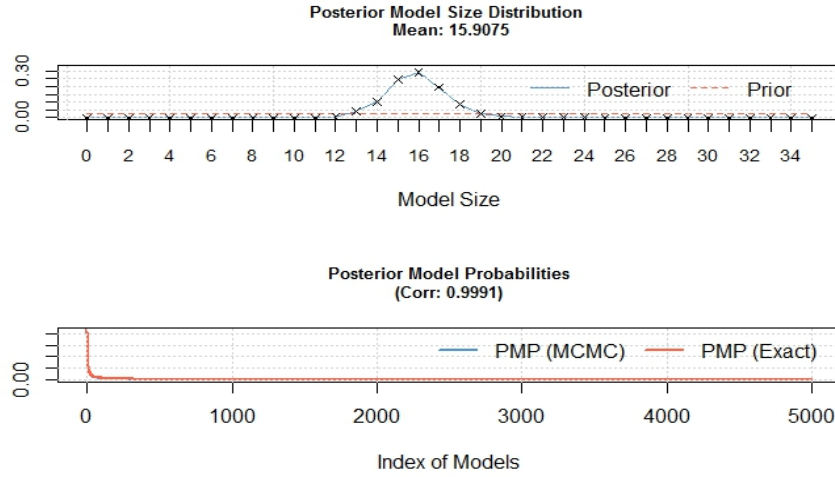
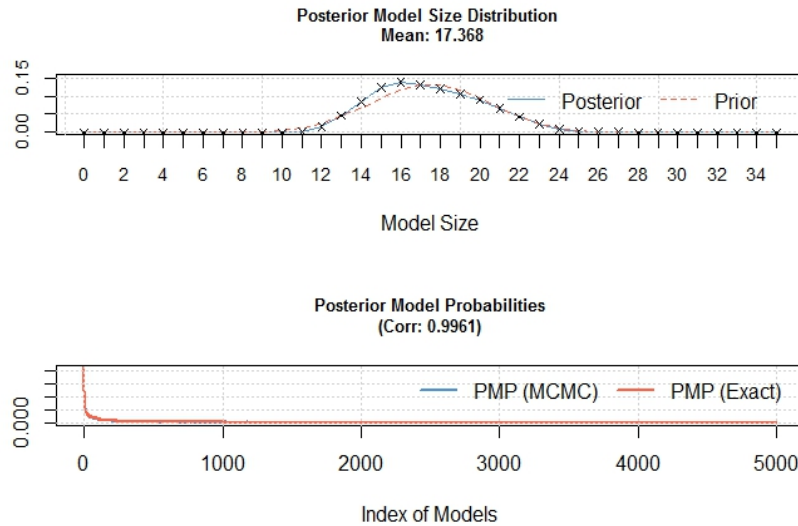


Table A3: Summary of BMA Estimation, Unweighted Regressions

<i>Mean no. regressors</i>	<i>Draws</i>	<i>Burnins</i>	<i>Time</i>	<i>No. models visited</i>
17.3680	2.00E+06	1.00E+06	9.077281 mins	543559
<i>Modelspace 2^K</i>	<i>% visited</i>	<i>% Topmodels</i>	<i>Corr PMP</i>	<i>No. Obs.</i>
$3.4e+10$	0.0016	69	0.9961	598
<i>Model Prior</i>		<i>g-Prior</i>	<i>Shrinkage-Stats</i>	
uniform / 17.5		UIP	Av=0.9983	

Notes: In this specification we employ the priors suggested by Eicher et al. (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure A3: Model Size and Convergence, Unweighted Regressions



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