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Does the inequality-credit-crisis nexus exist? An empirical re-examination

Xiao Chang^a, Guoqiang Li^b, Xinhua Gu^b and Chunyu Lei^b

^aDivision of Business and Management, United International College, Beijing Normal University-Hong Kong Baptist University, Zhuhai, China;

^bDepartment of Finance and Business Economics, Faculty of Business Administration, University of Macau, Taipa, MO, China

ABSTRACT

Rajan claims that rising inequality led to financial crises through credit booms in the U.S. Kumhof and Ranciere provide a theoretical formulation for this hypothesis. However, their assertions are not supported by cross-country evidence found in the work of Bordo and Meissner. A few subsequent empirical studies, albeit inspired by this pioneering work, find new evidence not in line with its conclusion but with the Rajan hypothesis. To clarify this controversial issue, we base our study on the B-M framework, resort to different estimators, and employ more model specifications by incorporating the role of deindustrialization. We find strong evidence for the inequality-credit-crisis nexus as modelled by Kumhof et al.

KEYWORDS

Deindustrialization; income inequality; credit boom; financial crisis; empirical evidence

JEL CLASSIFICATION

E20; E25; E51; N1

1. Introduction

Financial instability emerged with greater frequency and severity in the past decades. There were 137 banking crises between 1970 and 2007, with a major crisis occurring every ten years after financial liberalization (Caprio et al. 2003). The global cost of the 2007–09 crises is estimated to lie between 5 and 15 trillion US dollars (Adelson 2013). Such serious consequence has prompted academic research to identify the root causes of crises. While a large literature attributes crisis risk to various financial and real (macroeconomic) factors, recent interest focuses on the potential link from income inequality to financial instability (Lim and Khor 2011; Stockhammer 2015). Such research attention arises because banking crises broke out more frequently while rising inequality became widespread across countries at the same time; the labour income share is observed to have exhibited a persistent global drop since the early 1980s (Karabarbounis and Neiman 2014). The policy implication of this research is that income distribution has to be equitably structured if crises are to be avoided in the future (Lysandrou 2011).

Yet there is no consensus among existing studies regarding whether financial crises are really the result of rising inequality. This question may appear to be an outlandish suggestion; most mainstream accounts of

financial crises give no role to distributional considerations (Krugman 2009; Reinhart and Rogoff 2009; Atkinson and Morelli 2010). For example, the US Financial Crisis Inquiry Commission set up in 2009 examined 22 areas in search of the potential causes of the recent crisis, but none of these areas refers to inequality. Instead, financial crises are largely attributed to traditional factors, such as credit booms gone bust (Borio and White 2003; Mendoza and Terrones 2008; Schularick and Taylor 2012). On the other hand, however, a number of authors have begun to argue for rising inequality as a contributing factor leading to the occurrence of the 2007–08 US crisis (Fitoussi and Saraceno 2010; Stiglitz 2009; Rajan 2010; Kumhof et al. 2012 & Kumhof, Ranciere, and Winant 2015). Their argument is that average households with stagnating incomes were allowed to maintain their living standards by borrowing from the rich few with mounting incomes. This borrowing engendered a fast escalation of household debt and an over-expansion of financial institutions. The crisis eventually arrived and hit the banking sector badly when the debt accumulation became unsustainable. Such a crisis, albeit appearing to be financial, is deeply rooted in a structural distortion in income distribution. The above opposing views on the causes of crises imply that a further study is needed to clarify the issue, as will be done in this paper.

There remains scant formal research, theoretical or empirical, on the inequality-credit-crisis nexus despite the growing debate in opinion editorials, popular books, and policy papers (Tridico 2012; Wisman 2013). A few recent empirical studies are mainly inspired by the pioneering work of Bordo and Meissner (2012) (referred to as B-M thereafter) that includes inequality explicitly as a regressor, but this variable does not appear in any previous work on financial crises. These recent studies produce diverse results on the nexus due to the use of different econometric techniques (regression estimators and model specifications) and/or different data samples (country coverage and time span). Specifically, while B-M use the fixed-effects OLS estimators,¹ other studies employ similar specifications but different estimators. For example, the DSUR model is invoked in Malinen (2016), and the SUR, mixed-effects, and random-slope models are utilized in Gu and Huang (2014). Additionally, some determinants of financial instability not considered in B-M are included jointly with inequality by other authors. For example, asset bubbles are used as a covariate along with inequality by Roy and Kemme (2012) in their bivariate panel logit model and by Gu, Tam, and Lei (2019) in their system GMM model. The impact of public debt is considered together with the effect of inequality by Kirschenmann, Malinen, and Nyberg (2016) in their unbalanced fixed-effects penal logit model. Financial deregulation as well as inequality is emphasized by Perugini, Holscher, and Collie (2015) in their IV, PCSE, and GMM models. While B-M and traditional works find no evidence on the role of inequality in causing financial crises, the six new studies mentioned above arrive at their results opposite to B-M's, thus rendering the issue debatable. In this paper, we are particularly concerned with whether the B-M conclusion is robust to a more elaborate econometric analysis.

Our paper, albeit based on the B-M framework, departs from this reference work in terms of estimation approach and data sampling. Our study relies on three different types of regression models to seek out robust results. First, we use the same fixed effects OLS model as with B-M's, but deviate from it slightly

by partitioning their sample of 14 countries into two groups and cutting their observations of 1920–2008 into two types of sub-periods. The reason for so doing is that cross-sectional heterogeneity and longitudinal structural shift need to be addressed more carefully. Such two small deviations turn out to make a critical difference to the statistical significance of inequality effect. Second, we retreat from country grouping and use the same regressors as in B-M, but apply the system GMM as a different estimator to recent data. The justification for this additional deviation is that the problem of endogeneity can be purged more effectively. Again, we find a significant link from rising inequality to crisis risk. Finally, we deviate substantially from B-M to make regressions more realistic by incorporating the role of deindustrialization while using the PMG estimator. Such large deviations are due to the requirement that short-run dynamic movements converge to a long-run equilibrium outcome and to the fact that financial and other service sectors have been over-expanded in some advanced economies but not so much in others. Deindustrialization not considered in the previous literature is included in our regressions either independently of or interacting with inequality. Our estimation results robustly show that rising inequality implies higher crisis risk more significantly in some countries with heavy financialization (Martin, Kersley, and Greenham 2014) than in others with strong manufacturing.

The rest of the paper proceeds as follows.² Section 2 replicates the B-M fixed-effects OLS regression with two small deviations. Section 3 departs again from the B-M model and performs GMM estimation. Section 4 deviates from the B-M framework even further by using the PMG method and considering deindustrialization. Section 5 concludes.

II. Fixed-effects OLS regressions

This section presents estimation results using the same fixed-effects OLS regressions as with B-M but adopting a different approach to data sampling. To eschew possible problems of cross-sectional heterogeneity and time-series breaks, we partition the 14 countries into two groups and cut the sample of

¹In this paper, OLS stands for 'ordinary least squares,' SUR for 'seemingly unrelated regressions,' DSUR for 'dynamic SUR,' GMM for 'generalized method of moments,' IV for 'instrumental variables,' PCSE for 'panel-corrected standard error,' and PMG for 'pooled mean group.'

²A detailed review of the related literature is presented as the *Supplementary material* that is available upon request.

1920–2008 into two types of sub-periods.³ Our study, albeit based on B-M, turns out to generate some results different from theirs. Specifically, we find significant evidence linking financial crises to rising inequality via credit booms for one group but not for the other, and such statistical significance may be valid for the two groups combined in some sub-periods but not in the whole sample period.

It is necessary to describe B-M's work briefly. They first examine the link between banking crises and lending booms, and then explore whether there is a connection between credit growth and income inequality. Theoretical models posit there is a positive association of banking crises with lending booms, and this assertion is confirmed by various empirical studies. While agreeing with B-M on such confirmed association, we are concerned with whether there can be any room for defending their conclusion on the inexistence of any relationship between credit growth and rising inequality, given that this conclusion has now been questioned by a few new studies mentioned earlier.

The hypothesis tested by B-M is that credit growth bears no relation to changes in income concentration when conditioning on other factors. Two scenarios of data are used in their estimation. One focuses on medium-term relationships by sampling cumulative changes over five years for

each variable, with regression results presented in their Table 2. The other uses yearly data for robustness check despite their volatility, with results reported in their Table 3. The two tables show that credit growth has no significant bearing on income concentration. This result implies that rising inequality does not lead to financial crises since Table 1 in B-M shows a higher risk of banking crisis associated with faster credit growth. Instead, they find per capita real income growth and short-term nominal interest rates to be the only two strong drivers for credit growth and hence crisis risk.

In the B-M annual-data regressions, all explanatory variables are lagged by one year to deal with the possible problem of simultaneity. Traditional covariates are used to explore the relationships between credit growth and other macroeconomic aggregates. One-year lagged values for these variables may not serve as instrument variables sufficiently well. Remaining endogeneity has yet to be handled by different estimators or other model specifications, as done in the next two sections. For the time being, we stick to B-M's empirical framework with only two small deviations – country grouping and period partitioning. Such deviations are just following traditional tricks for sophisticated analysis to achieve empirical significance since including too

Table 1. Regressions for real credit growth in the two country groups: five-year data for 1920–2008.

Variables	Group 1 (AUS, CAN, ESP, GBR, ITA, USA)				Group 2 (CHE, DEU, DNK, FRA, JPN, NLD, NOR, SWE)			
	Reg1	Reg2	Reg3	Reg4	Reg5	Reg6	Reg7	Reg8
Δ Top 1% share	0.031* (2.27)	0.034*** (6.52)	0.029** (2.90)	0.039* (2.57)	−0.013 (−0.96)	−0.001 (−0.08)	−0.012 (−0.91)	0.012 (0.73)
Δ ln (GDP/capita)		0.464** (2.91)	0.503** (2.89)	−0.573 (−1.67)		1.532*** (5.61)	1.602*** (4.50)	0.979** (2.67)
Δ Top 1% share \times Δ ln (GDP/capita)			0.042 (0.93)	−0.071* (−2.49)			0.114 (1.70)	−0.289 (−1.80)
Δ Short-term nominal interest rate				0.130 (0.19)				0.577 (1.49)
Δ (Investment/GDP)				1.875 (1.68)				0.639 (1.40)
Δ ln (Money/price level)				0.795*** (18.62)				0.477* (2.11)
Δ ln (Credit/price level) $t-1$	0.211 (1.80)	0.267* (2.05)	0.257 (1.88)	0.281 (1.65)	0.188** (2.87)	0.153* (1.97)	0.138* (1.94)	0.024 (0.52)
Total observations	312	312	312	241	494	494	494	387
R-squared	0.208	0.255	0.258	0.500	0.135	0.449	0.456	0.480
Number of countries	6	6	6	6	8	8	8	8

OLS estimation and data are the same as those in Table 2 of Bordo and Meissner (2012). Dependent variable $\Delta \ln(\text{Credit/price level})$ is the cumulative change in the share of total income earned by the top percentile over the five years leading up to and including 1920, 1925, 1930, 1935, 1940, 1955, 1960, 1965, ..., 2005. Quinquennial period dummies are included in all regressions as are country fixed effects and period fixed effects. Robust standard errors are reported throughout, and t -statistics are in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

³The 14 countries are: Australia (AUS), Canada (CAN), Denmark (DNK), France (FRA), Germany (DEU), Italy (ITA), Japan (JPN), Netherlands (NLD), Norway (NOR), Spain (ESP), Sweden (SWE), Switzerland (CHE), United Kingdom (GBR), and United States (USA).

Table 2. Regressions for real credit growth in the two types of sub-periods: type 1 – annual data starting from 1920; type 2 – five-year data ending in 2008.

Variables	Type 1 sub-periods (from 1920 forwards)				Variables	Type 2 sub-periods (from 2008 backwards)			
	Reg1 1920–1955	Reg2 1920–1959	Reg3 1920–1975	Reg4 1920–1980		Reg5 1985–2008	Reg6 1970–2008	Reg7 1965–2008	Reg8 1960–2008
Δ Top 1% share $t-1$	0.020** (2.74)	0.020** (2.77)	0.016* (1.90)	0.015* (1.83)	Δ Top 1% share	0.074** (2.19)	0.048* (1.84)	0.050* (2.04)	0.040* (1.97)
Δ ln (GDP/capita) $t-1$	0.384* (2.15)	0.412* (2.24)	0.219** (2.77)	0.201** (2.42)	Δ ln (GDP/capita)	2.260** (2.82)	1.729*** (6.10)	1.815*** (8.15)	1.737*** (7.67)
Δ Short-term nominal interest rate $t-1$	–1.116 (–0.74)	–1.144 (–0.85)	–1.275*** (–3.84)	–0.611** (–2.28)					
Δ (Investment/GDP) $t-1$	–1.239*** (–4.21)	–1.226*** (–4.02)	0.011 (0.08)	0.008 (0.05)					
Δ ln (Money/price level) $t-1$	–0.198 (–1.12)	–0.206 (–1.20)	0.016* (1.90)	0.015* (1.83)					
					Δ Top 1% share \times ln (GDP/capita)	–0.813** (–2.55)	–0.428* (–1.90)	–0.442* (–2.00)	–0.370* (–1.85)
Δ ln (Credit/price level) $t-1$	0.224** (2.31)	0.234** (2.62)	0.231** (2.49)	0.264** (2.67)	Δ ln (Credit/price level) $t-1$	–0.199 (–1.52)	–0.096 (–0.90)	–0.064 (–0.70)	–0.027 (–0.37)
Total observations	112	127	270	330	Total observations	65	95	102	105
R-squared	0.441	0.468	0.326	0.433	R-squared	0.260	0.277	0.347	0.327
Number of countries	9	9	11	12	Number of countries	14	14	14	14

Data and regressions are based on Table 3 in Bordo and Meissner (2012) for the type-1 sub-periods and on their Table 2 for the type-2 sub-periods. Dependent variable Δ ln(Credit/price level) involves the annual change for the type-1 sub-periods and the five-year change for the type-2 sub-periods. Estimation is by OLS. Year dummies for the type-1 sub-periods and quinquennial period dummies for the type-2 sub-periods are included in all regressions as are country fixed effects. Robust standard errors are reported throughout, and *t*-statistics are in parentheses. ****p* < 0.01, ***p* < 0.05, and **p* < 0.1.

Table 3. System GMM regressions for real credit growth in the 13 countries: 1995–2007.

Variables	Reg1	Reg2	Reg3	Reg4	Reg5	Reg6	Reg7
Δ ln (Credit/price level) $t-1$	0.344*** (3.05)	0.320*** (3.22)	0.311*** (2.88)	0.298*** (2.81)	0.469** (1.98)	0.217, (1.49)	0.536*** (2.66)
Δ Top 1% share	0.012** (2.04)	0.013*** (1.66)	0.012** (2.08)	0.013* (1.71)	0.016** (2.28)	0.011* (1.79)	0.017* (1.89)
Δ ln (GDP/capita)	1.115 *** (7.26)	1.192, (1.45)	1.142*** (6.55)	1.309 (1.50)	1.278* (1.94)	0.973 (1.31)	0.967, (1.12)
Δ Short-term nominal interest rate	0.104 (0.21)	0.095, (0.18)	0.178 (0.39)	0.142 (0.29)	–0.869 (–1.06)	0.296 (0.55)	–2.052 (–1.50)
Δ ln (Money/price level)	0.257*** (2.91)	0.276*** (3.03)	0.277*** (2.97)	0.296*** (2.82)	0.237** (2.06)	0.526 (1.37)	0.686 (1.25)
Δ (Investment/GDP)	0.002 (0.73)	0.002 (0.77)	–0.001 (–0.15)	–0.001 (–0.30)	0.000 (0.00)	–0.000 (–0.05)	0.002 (0.20)
Δ (Current account/GDP)	–0.000 (–0.01)	–0.000, (–0.04)	–0.001 (–0.32)	–0.001 (–0.38)	–0.001 (–0.33)	–0.001 (–0.41)	–0.002 (–0.58)
Constant	0.008 (1.06)	0.007,,, (0.29)	0.009 (1.19)	0.006 (0.22)	–0.004 (–0.17)	0.008 (0.29)	–0.023 (–0.65)
Instrumented variables	<i>a, b</i>	<i>a, b, c</i>	<i>a, b, d</i>	<i>a, b, c, d</i>	<i>a, b, c, d, e</i>	<i>a, b, c, d, f</i>	<i>a, b, c, d, e, f</i>
Total observations	143	143	143	143	143	143	143
Number of countries	13	13	13	13	13	13	13
Arellano-Bond AR(1) test	–2.58, [0.010]	–2.29, [0.022]	–2.45, [0.014]	–2.16, [0.031]	–1.62, [0.104]	–2.36, [0.018]	–1.44, [0.150]
Arellano-Bond AR(2) test	–0.20, [0.840]	–0.39, [0.698]	–0.71, [0.478]	–0.82, [0.414]	0.25, [0.805]	–0.77, [0.441]	0.22, [0.826]
Sargan overid. test	9.05, [0.171]	10.70 [0.152]	10.73, [0.151]	11.77 [0.162]	9.07, [0.431]	12.47, [0.188]	6.15, [0.802]
Hansen overid. test	5.49, [0.482]	5.94,,, [0.546]	5.80,,, [0.563]	6.33,,, [0.610]	6.09,,, [0.731]	5.49,,, [0.790]	3.42, [0.970]

Our system GMM estimation is based on the 14-country data and regression specifications (dependent and explanatory variables) in Bordo and Meissner (2012) for a recent period of 1995–2007, with 13 countries considered due to the exclusion of Germany. Instrumental variables in our estimation are designed for: (a) Δ ln(Credit/price level) $_{t-1}$, (b) Δ Top 1% share, (c) Δ ln(GDP/capita), (d) Δ (Investment/GDP), (e) Δ Short-term nominal interest rate, and (f) Δ ln(Money/price level). External instrumented variables take on values of the WJP Rule of Law Index 2014 (World Bank), trade/business openness (the 2014 Index of Economic Freedom), and the Trade Union Density (OECD Economic Outlook database). *t*-Statistics for coefficient estimates in parentheses are based on robust standard errors. *p*-Values for various tests are recorded in square brackets. ****p* < 0.01, ***p* < 0.05, and **p* < 0.1.

many ‘outliers’ to pursue generality tends to produce insignificant estimates (Baumol 1986).

These two deviations are justified by economic situations and data particularities. First, the 14 countries in the B-M sample, albeit all within the OECD, are quite different in one way or another. As shown in Figure 1, for example, income inequality rose faster or reached higher levels in some countries than in others. This difference has also been noted by other authors (e.g. see three data graphs in Figure 1 of Kumhof et al. 2012). Some

economies were more heavily financialized than were others (Hein 2012; Tridico 2012). It is then likely that the country fixed effects alone cannot account well for cross-sectional heterogeneity. It is thus necessary to resort to other estimators (such as SUR or random-slope models as in Gu and Huang 2014) or partition countries into groups. Our grouping is such that countries are plausibly similar within a group but substantially different between groups. Second, structural shifts in sampled economies took place in the past century

under B-M consideration. A typical example is the widespread deindustrialization that occurred only in the past few decades not in the preceding decades (Mankiw and Swagel 2006). This structural change also differed between countries in terms of its size and pace as well as its effect on income inequality and financial instability. Thus the time fixed effects common to all sampled economies are unlikely to fully capture country-specific time-series shifts in economic structure. This problem is resolved in our work by simply cutting the 1920–2008 into sub-periods that feature their respective structures.

Our Table 1 regressions are based on the Table 2 work of B-M in terms of 5-year data, model specifications, and the fixed-effects OLS estimator. The dependent variable is real credit growth, and its lagged value appears as a regressor to take into account growth inertia and make the model somewhat dynamic as in B-M. Two country groups are used in Table 1, with group 1 including six countries (Australia, Canada, Italy, Spain, UK, and US) while group 2 contains eight countries (Denmark, France, Germany, Japan, Netherlands, Norway, Sweden, and Switzerland). There are four model specifications used for each country group, which consist of progressively more covariates along with the top income share as the risk determinant of interest. The interaction term used in B-M is also used in our Table 1 to explain the possibility that when inequality rises with income a credit boom

arises for consumption smoothing by low- and middle-income people.

It is interesting to compare empirical results in our Table 1 with those in Table 2 of B-M. First, while top income growth in group 2 countries mostly has ambiguous effects on real credit growth as in B-M, this effect is statistically insignificant in our regressions as implied by Figure 1. Second, contrary to B-M, however, we find that top income growth in group 1 countries contributes to real credit growth in a significant as well as positive manner. Thus according to the B-M logic one can hardly claim that financial crises have no bearing on rising inequality in these countries. Third, it is found from most of our regressions that per capita income growth is strongly related to credit growth in the two groups. While this result is in line with B-M and others', our interaction term provides mixed evidence. Fourth, our estimated effect of interest rates is different from that found in B-M in terms of the estimate's sign. Fifth, we find that investment and real money growth are positive or/and even significant contributors to real credit growth, and this finding seems economically intuitive, albeit different from B-M. Finally, credit inertia is found to exist in our regressions as in B-M, but unlike theirs, half our estimates of such an effect are also significant.

Our regressions in Table 2 for up to 14 countries are based on those in both Tables 2 and 3 of B-M to provide another perspective. Real credit growth is

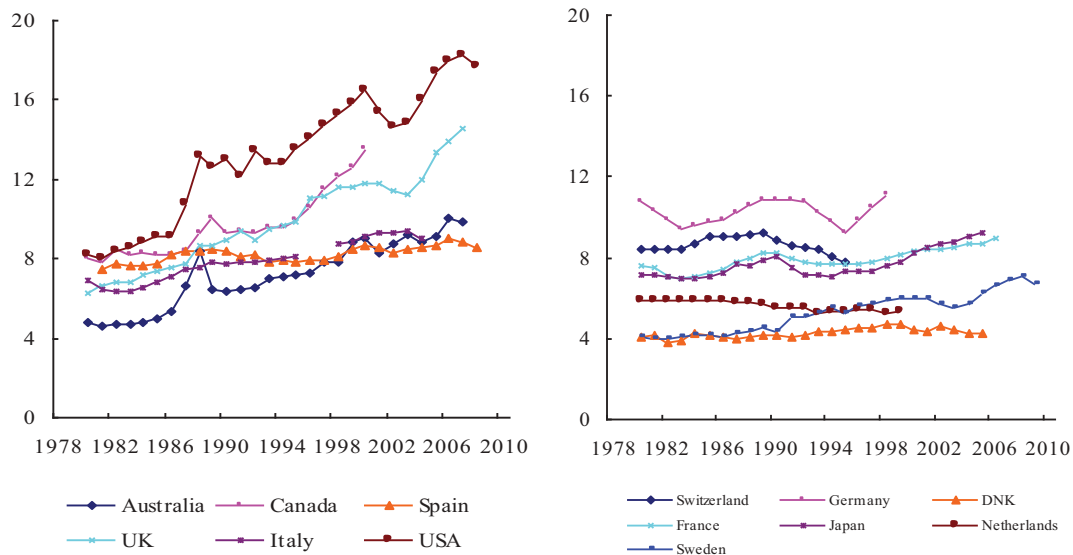


Figure 1. Top 1% income share. Notes: The data source is Bordo and Meissner (2012)

still used as the dependent variable. Unlike [Table 2](#) of B-M with 5-year data, their [Table 3](#) uses annual data to look at whether estimates are robust to different samples. Two types of sub-periods are used in our [Table 2](#): one is from 1920 forwards with annual data and the other is from 2008 backwards with 5-year data. In each of the two types of sub-periods there are four sub-samples that progressively span more years or more 5-year periods. In fact, similar sampling is also used in B-M to reflect important structural changes in economic fundamentals. Time frames in Reg's 1 and 6 of our [Table 2](#) are similar in spirit to those in models 4 and 5 of B-M [Table 3](#), respectively. Our Reg5 covers another recent period starting from 1985 that is an important year in the economic and financial sense. The use of such sample division rests on the fact that financial liberalization since the 1970 s interacted with low expected inflation since the mid-1980 s to generate a large number of boom-bust episodes and a rising frequency of banking crises compared to the 1950–1972 period.

Estimation results in our [Table 2](#) are compared below with findings in [Tables 2](#) and [3](#) of B-M. First, once again, cross-country evidence is found for top income shares as a significant driver for credit growth and hence crisis risk. This finding is robust to all regressions we deploy for the two types of sub-periods, but inconsistent with the main conclusion in B-M. Second, our [Table 2](#) echoes our [Table 1](#) in terms of a significantly positive relation between GDP growth and credit growth, and this result is in line with findings in [Tables 2](#) and [3](#) of B-M. The interaction term in Reg's 5 to 8 of our [Table 2](#) shows the positive effect of income growth on credit growth is reduced by rising inequality. This result is similar to that in [Table 2](#) of B-M in terms of the estimate's sign, but such an indirect effect of inequality is significant in our estimation. Third, the negative effect of interest rates on credit growth is established in Reg's 1 to 4 of our [Table 2](#) as in [Tables 2](#) and [3](#) of B-M, and this effect is significant in most of their regressions but in only some of ours. Fourth, investment expansion and real credit growth is found to have mixed effects on credit growth in our [Table 2](#) estimation as in [Tables 2](#) and [3](#) of B-M. Finally, the estimated inertia effect is similar in sign and significance between B-M's [Table 3](#) work and our type 1 sub-periods

estimation, but different in sign between B-M's [Table 2](#) models and our type 2 sub-periods regressions.

III. System GMM regressions

The last section presents estimation results from fixed-effects OLS regressions for real credit growth by following B-M, with small modifications applied to data sampling. As mentioned earlier, the problem of endogeneity needs to be further addressed for greater econometric efficiency. The OLS estimator is biased and inconsistent for dynamic panels (Baltagi 1995) as in [Tables 1](#) and [2](#) of our work as well as in [Tables 2](#) and [3](#) of B-M. This section turns to system GMM as a more suitable estimator, but still sticks to the same framework as with B-M in terms of data samples and variable specifications. The GMM is also deployed by other authors to look for evidence linking rising inequality to banking crises, but they use different data and specifications (Perugini, Holscher, and Collie 2015; Gu, Tam, and Lei 2019). Once again, our result from using this additional estimator robustly rejects the hypothesis that changes in financial credit have no relationship with top income shares.

Clarifying this hypothesis is the key for us to decide whether rising inequality leads to financial instability according to the empirical strategy set forth earlier. This strategy includes two-step regressions: the first step reveals the link of crisis risk to lending booms and the second step examines the relation between credit growth and rising inequality. Since the extensive literature has reached a consensus over the first-step link (Elektag and Wu 2011; Schularick and Taylor 2012), what we should do next is to continue to focus on the second-step relation. It is helpful to look at data plots in [Figure 2](#) before moving on to formal GMM regressions. This figure clearly shows the existence of a close relation between income inequality and bank credit in the reported economies. Since mortgage loaning and consumer credit constituted a major portion of banking business in the recent few decades, growth in bank credit was accompanied by a rise in household leverage leading up to financial crises. The inequality, credit/leverage, crisis nexus has been formally modelled in the latest studies (Kumhof, Ranciere, and Winant 2015; Gu, Tam, and Lei 2019), which enable

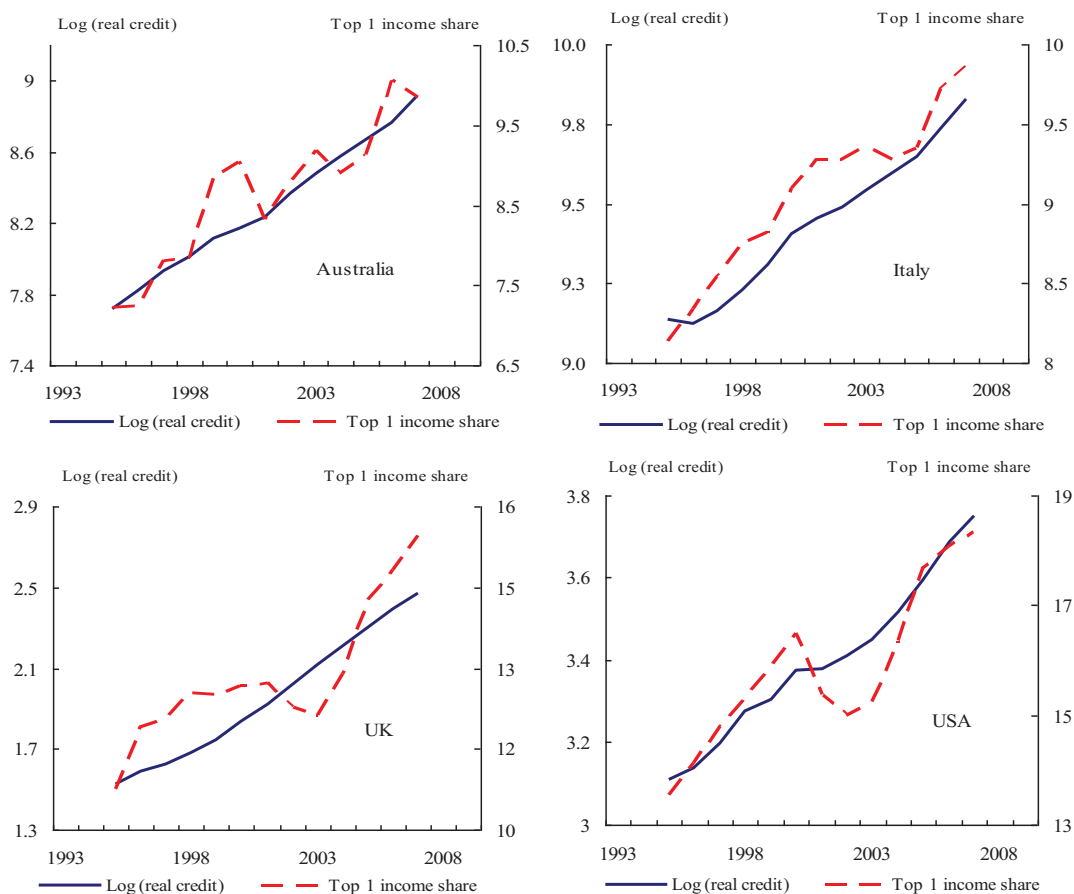


Figure 2. Real credit versus top 1% income share. The data source is Bordo and Meer (2012).

us to avoid ad hoc estimation without theoretical backing.

The sample period for our GMM regression is limited to the years between 1995 and 2007 preceding the recent global crisis. This period saw both deepened financial deregulation in advanced countries and widening income disparities among different households. It was also in this period when the fast run-up of housing and credit bubbles eventually led to the 2008–09 financial crises and then to the Great Recession. Notably, in the US, interstate banking was permitted with the passage of the Riegle-Neal Act in 1994, and universal banking was also allowed after the Gramm-Leach-Bliley bill was enacted in 1999. Our choice of the sample period is an exact reflection of the impacts of deregulation and inequality on lending frenzies and banking crises.

Our motivation for the use of a GMM estimator lies with necessity for a better treatment of endogeneity problems as well as dynamic regressions. Possible reverse causality may give rise to endogeneity that is detrimental for estimation precision.

Possibly, credit growth can have two-way causal relationships with income inequality, capital formation, or GDP growth (Borio and White 2003; Elekdag and Wu 2011). It then becomes necessary to find appropriate instruments for those endogenous variables. In system GMM, endogenous regressors in levels are instrumented with their lagged first differences, which are assumed to be uncorrelated with current error terms in levels. Besides these internal instruments, external ones can also be used by choosing institutional factors related to labour and products markets, the rule of law, and trade openness. Our data for external instruments come from the following sources: the trade union density is obtained from the OECD Economic Outlook Database, the legal protection for private rights is taken from the WJP Rule of Law Index, and the trade/business openness is extracted from the Index of Economic Freedom. To check for the robustness of results, we treat different sets of regressors as endogenous and instrument them with their lagged first differences for equations in

levels in addition to their lagged levels for equations in first differences.

In the case of system GMM estimation for panels with small cross-sections and long time-series (relatively, large and short panels are efficiently better), the cluster-robust standard error, the Arellano and Bond autocorrelation (A-B) test, and the Hanson test may become unreliable due to the excessive use of instruments. To eschew possible small-sample bias, heteroskedasticity, and autocorrelation arising from such instrument proliferation, we limit the number of lags used for instrumentation and create only one instrument for each variable and lag distance. The A-B test is used for the null of no first- or second-order serial correlation in the first-differenced residuals (Arellano and Bond 1991). This test for $AR(1)$ processes in first differences usually rejects the null hypothesis. The test for $AR(2)$ processes in first differences, if detecting autocorrelation in levels, will signal poor instruments used. The validity of instruments can also be tested under the null that they are exogenous as a whole with respect to over-identifying restrictions using the Sargan and Hansen statistics, where homoskedasticity is required in the former test but not in the latter. In all our regressions the null hypothesis is not rejected, thereby signifying the validity of the instruments used in our estimation. For more precise statistical inference, standard errors are corrected to cope with heteroskedasticity and autocorrelation within panels.

In Table 3 our system GMM estimation involves 13 out of the 14 countries considered in B-M after Germany has been ruled out for the reason implied in Aizenman and Sengupta (2011). Our regression variables are the same as those in B-M for direct comparison of estimation results. GMM is also deployed in Perugini, Holscher, and Collie (2015) and Gu, Tam, and Lei (2019), but asset bubble and financial deregulation are added as key covariates to their regressions. We still stick to the B-M specifications in order to focus on the effect of inequality on credit, and our result on this effect turns out to be different from that in B-M but similar to that in Gu et al. and Perugini et al. Although different combinations of instruments are used in various model specifications in

Table 3, most of our estimates exhibit remarkable stability in terms of their signs, magnitudes, and significance levels.

Empirical findings from our Table 3 GMM estimation are briefly interpreted below. First, top income shares are related significantly and positively to real credit growth, a key result of ours that is at odds with B-M's. The result is robust across all regression models after controlling for traditional credit determinants. This result combined with the Table 1 result of B-M implies a causality link from rising inequality to financial crises. Second, credit growth is found, as expected, to exhibit significant inertia. This result, robust to almost all model specifications, justifies our use of dynamic regressions. Third, according to B-M's estimation, credit growth is significantly affected by per capita income growth in a positive manner and by interest rates in a negative manner. By contrast, only fewer than half our regressions arrive at the significantly positive role of income growth for credit expansion, and all our regressions show short-term interest rates have an insignificant effect on credit booms with this effect being mostly positive. Fourth, more than half of our models show that credit booms are coupled with money growth in a significant way, and all these models produce insignificant estimates for the link of credit booms with investment expansion. The signs of estimates for the two variables are mostly similar to those in Perugini, Holscher, and Collie (2015). These results are different from Schularick and Taylor (2012) who observe a deviation of credit change from the money supply, but somewhat close to Kindleberger (1996) who finds the association of credit growth with higher investment. Finally, the current account enters into our GMM regressions, signalling a possible link between external and financial imbalances. Our estimation identifies an insignificant but negative impact of trade balance on credit booms. This result seems consistent with Mendoza and Terrones (2008), who show that external deficits necessitate capital inflows, which in turn encourage credit growth.

IV. PMG regressions

This section departs from B-M further by resorting to yet another estimator and new model specifications.

Deindustrialization is a sheer structural factor, and its economic and financial impacts are pervasive and profound (McKinnon 2013).⁴ The inclusion of this factor as a key covariate allows us to estimate the role of the economic structural change in causing income inequality and financial fragility. Countries in question are grouped according to their degrees of deindustrialization. The PMG estimator is deployed to test whether top income shares have additional explanatory power for not only the short-run dynamics of financial credit but also for a long-run relationship with its potential determinants. This estimator can effectively cope with endogeneity through sufficiently dynamic specification that the regressors are strictly exogenous and the resulting residuals are serially uncorrelated. A similar estimator is used in the literature to study the empirical link of rising inequality to current account imbalances (Kumhof et al. 2012) but not to financial crises as we do here. Again, we find a significant link from rising inequality to credit booms and hence to banking crises.

Country grouping here for PMG is similar to what was done earlier for OLS estimation, except that only a subset of the 14 countries is partitioned into two groups due to missing data and outlier removal. As illustrated in Figure 1, most years of the sample period saw relatively high or rising inequality (and current

account deficits) in group 1, which includes Italy and Anglo-Saxon countries (i.e. Australia, Canada, UK, and US). In contrast, low or stable inequality (along with current account surpluses) was maintained in much of the sample period by group 2, which consists of Japan and continental Europe (i.e. France, Germany, Netherlands, and Switzerland). Figure 3 delivers more messages about underlying differences between the two groups, showing that group 1 experienced greater and steeper deindustrialization than group 2. Notably, financial systems are market-based and aggressive in most group 1 countries, but bank-based and conservative in many group 2 countries. The implication of deindustrialization for financial instability has been completely overlooked in the literature. Our work shows that grouping countries to consider their heterogeneous degrees of financialization makes a critical difference to the signs and significance levels of the estimates for coefficients on the variables governing the inequality-crisis nexus.

New data for our Table 4 regressions are described below. First, total bank loans used are the same as B-M data, and another measure is also used for financial credit. This measure is the ratio of private credit by depository banks and other financial institutions relative to GDP, which is far broader than the ratio of private credit by deposit

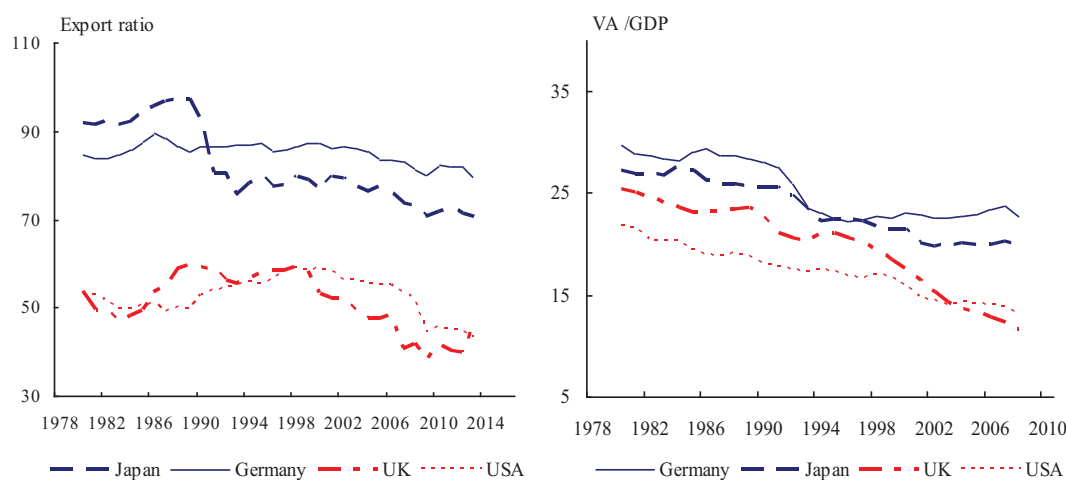


Figure 3. Ratio of manufacturing to total exports and industry value added relative to GDP. The data source is the World Development Indicators of the World Bank.

⁴The literature on offshoring or outsourcing in the course of deindustrialization has been primarily positive for its economic effects other than its financial implications. Yet existing research still lags behind popular interest in offshoring issues, with debates often generating more heat but shedding less light (Mankiw and Swagel 2006). There has been little theoretical analysis on the welfare effects of offshoring (e.g. job loss and income inequality), and empirical studies are also tentative because of limited data.

Table 4. PMG regressions for financial credit in the two country groups: 1980–2008.

Variables	Group 1 (AUS, CAN, GBR, ITA, USA)					Group 2 (CHE, DEU, FRA, JPN, NLD)				
	Reg1	Reg2	Reg3	Reg4	Reg5	Reg6	Reg7	Reg8	Reg9	Reg10
Δ Income inequality	0.124*** (7.86)	0.099*** (3.14)	0.144*** (3.42)	2.186* (1.88)	0.933* (1.85)	0.020 (0.53)	0.054 (1.49)	0.113 (0.97)	0.001 (0.00)	−0.004 (−0.70)
Δ Industrialization in Reg's 1 to 3 and 6 to 10	−0.045*** (−5.31)	−0.059*** (−5.32)	−0.041** (−2.54)	0.935* (1.83)	0.412* (1.92)	−0.011 (−0.62)	−0.019 (−1.00)	−0.016 (−0.56)	−0.039 (−0.77)	−0.002** (−2.10)
Δ Deindustrialization in Reg's 4 & 5										
Δ (Top 5% \times deindustrialization) in Reg 4 & 5				−0.029* (−1.81)	−0.012* (−1.76)					
Δ Monetary aggregates			6.956*** (3.13)	25.137* (1.71)	11.546* (1.83)	0.180 (1.21)	0.112 (0.88)			0.000 (1.58)
Δ Short-term nominal interest rate	0.120 (0.31)	0.886* (1.96)	1.098 (0.63)	25.435* (1.66)	10.666 (1.58)		1.123 (1.06)	−3.969 (−0.90)	0.322 (0.06)	0.029 (0.14)
Δ (Investment/GDP) in Reg's 1 & 2 and 6 to 9	7.813*** (8.55)	7.555*** (8.05)			−0.057* (−1.73)	8.051*** (3.76)	4.971** (2.47)	36.910* (1.68)	32.535** (1.99)	
Δ (Current account/GDP) in Reg5										
Δ GDP growth	0.000* (1.72)	0.000** (2.06)	0.002** (2.26)	0.004 (1.16)	0.002 (1.03)				−0.001 (−0.81)	0.000*** (3.90)
Error correction coefficient	−0.165** (−2.05)	−0.151** (−2.39)	−0.077*** (−3.05)	−0.023** (−1.97)	−0.044* (−1.79)	−0.128*** (−3.30)	−0.140*** (−2.72)	−0.057*** (−5.40)	−0.071*** (−3.56)	−0.709** (−2.54)
Total observations	112	123	123	123	123	101	101	101	97	103
Number of countries	5	5	5	5	5	5	5	5	5	5

Y_{bo} stands for private credit by deposit banks and other financial institutions relative to GDP as in Kumhof et al. (2012) and $Y_{dl,cp}$ for $\Delta \ln(\text{Credit}/\text{price})$ as in B-M. Dependent variable is Y_{bo} in Reg's 1–9 and $Y_{dl,cp}$ in Reg10. Income inequality is measured by top 1% income shares in Reg (1, 2, 6–10), top 10% in Reg3, and top 5% in Reg (4, 5). The degree of industrialization is measured by manufacturing value added/GDP in Reg1 and industry value added/GDP in Reg (2, 3, 6–10), while the extent of deindustrialization is proxied by service employment relative to total employment in Reg (4, 5). Monetary aggregates are money growth in Reg's 3–5 and the money supply in Reg (6, 7, 10). z-Values are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

money banks to GDP.⁵ The related data are taken from the World Bank Financial Structure Database. Second, only the top 1% income share is used in B-M, but top 5% and 10% income shares are also used in our study. The data are collected from the World Top Income Database. Third, three negative or positive proxies for the degree of deindustrialization are adopted: manufacturing value added as a percentage of GDP, industry value added relative to GDP, and the share of service-sector employment in total employment. The data are taken from the World Development Indicators of the World Bank. Finally, our PMG study is limited to the period between 1980 and 2008 for the same reason as described in Hein (2011) and Martin, Kersley, and Greenham (2014). This period witnessed widespread deindustrialization, financial liberalization, and economic globalization. Moreover, income inequality began to pick up considerably from 1980 onwards in some of advanced countries, as shown in Figure 1.

Our empirical strategy is designed for higher precision of estimation by paying attention to differences in the extent and pace of

deindustrialization between country groups. The PMG estimator is not applied to the 10 countries as a whole, but rather to the two country groups separately to capture between-group slope heterogeneity. Structural differences, albeit observed to be substantial between groups as implied in Figure 3, are not so large between countries within a group that pooled regression can be applied to each group. Conceivably, long-run parameters are applicable to countries within a group, and the model can be estimated as a system by pooling these parameters while short-run coefficients may vary both within and between groups (Pesaran and Smith 1995). Specifically, an unrestricted autoregressive distributed lag system of equations is reparameterized first as a vector error correction model system. Certain restrictions are then imposed on the resulting system, such that the long-run parameter should be common for all countries within a group but the short-run dynamic and error correction terms vary between countries regardless of group division. Finally, the maximum likelihood method is used to deal with contemporaneous residual covariance when

⁵Other financial institutions are added for estimation to be more realistic since private credit from these institutions (e.g. financial companies specializing in credit cards, car loans, mortgage origination etc.) increased five-fold in the U.S. between 1980 and 2008 (from 37% to 150% of GDP), while private credit from deposit money banks increased only moderately (from 55% to 65% of GDP) (Kumhof et al. 2012).

estimating the PMG model. Its estimates are consistent and asymptotically normal for both the stationary and non-stationary regressors.

In [Table 4](#) two measures of financial credit are regressed on its determinants. The PMG estimator is applied separately to each of the two country groups. Five model specifications are used for each group to check the robustness of estimation results. Only estimates for long-run parameters are reported here to conserve space. Our estimated coefficient on the error correction term for short-run adjustment is significant, negative, and less than one in absolute value, implying the convergence at a normal pace towards the long-run equilibrium relationships between credit and its affecting factors. Interpretations of our [Table 4](#) results are given below along with comparison between the two groups.

First, Reg's 1–5 show that income inequality is a strong long-run driver for lending booms in group 1. This result is consistent with our earlier findings in [Tables 1–3](#), and all these results are in line with the Rajan hypothesis but different from the B-M conclusion. In contrast, Reg's 6–10 suggest that top income shares are not a significant long-run predictor for credit booms in group 2. This finding gets closer to the B-M result, indicating that the Rajan hypothesis may work for the recent crisis in some advanced economies such as the US; but 'it is not an iron law' for all countries or all crises (The Economist 17/03/2012). Aside from statistical significance, it is also observed that the magnitude of estimate for the coefficient on top income shares is larger for group 1 than for group 2, suggestive of a stronger impact of rising inequality on credit booms in group 1 than in group 2 perhaps due to their different degrees of financialization. Our work also shows whether using credit growth or credit size relative to GDP makes no difference to estimation results (Malinen 2016), and the effect of inequality on credit can be robust to diverse measures of credit. This result combined with [Table 1](#) in B-M seems to clarify the link from rising inequality to financial instability via lending frenzies, a hotly debated issue among the recent studies surveyed earlier.

Second, all ten models in [Table 4](#) show that growing service sectors, especially financial services for credit-based consumption growth, play a

significantly positive role for lending frenzies and hence financial fragility in group 1, as implied negatively by two indexes of industrialization in Reg's 1 to 3 and positively by one index for deindustrialization in Reg's 4 and 5. This result is similar in spirit to what was discussed in recent policy-oriented studies (Hein 2012; Wisman 2013). Our estimation for group 2 in Reg's 6 to 10 uses a negative proxy for deindustrialization, finding that this structural change mostly has an insignificant impact on credit growth in this group and that the size of this impact is also smaller in group 2 relative to group 1. As shown in Reg's 4 and 5, the positive effects on credit booms of top incomes and service expansions in group 1 are significantly reduced by interactions between these two factors. Conceivably, lending booms could persist due to this offsetting interactive effect.

Third, we observe that the money supply or its growth still has a positive effect on lending booms, and that this effect is significant in group 1 but not in group 2. It is then doubtful whether the money view should give way to the credit view in accounting for financial crises in group 1 even though most of its member economies are heavily financialized. The relative importance of the two views discussed in the literature (Schularick and Taylor 2012) seems applicable only to group 2 countries under our consideration. Additionally, we note that interest rates are statistically a weaker factor for credit cycles in our regressions than in B-M's. We observe that this factor is largely pro-cyclical because it mostly carries a positive estimated coefficient unlike the negative one in B-M. Our result accords well with observed long-run trends of nominal short rates (Demirgüç-Kunt and Detragiache 1998; Mishkin 2003).

Finally, three other macroeconomic factors are included as control variables in our [Table 4](#) regressions: investment relative to GDP, the current account balance to GDP ratio, and GDP growth. Unlike B-M, all our reported models find the investment ratios are significantly and positively associated with the credit measures in both country groups, and this result confirms a long literature on capital formation that needs to be financed with credit (Minsky 1986; Kindleberger 1996). We also establish some significant evidence on credit booms that are linked to rising current account deficits, and this evidence repeatedly appears in

the literature (Mendoza and Terrones 2008; Kirschenmann, Malinen, and Nyberg 2016). We observe that the predictive power of GDP growth for credit expansion in the two groups of countries is not as strong in our estimation as in B-M, and our estimated impact of growth on credit is partly significant and also has a small value. Even a negative impact of some GDP related factor (its real growth or per capita level) on financial credit and fragility has been identified in the literature (Demirguc-Kunt and Detragiache 1998; Perugini, Holscher, and Collie 2015).

V. Conclusion

Our paper examines whether Rajan's political-economy hypothesis or B-M's pioneering empirical work is defensible by applying various estimators to a dataset close to B-M's. Rajan attributes the recent banking crises to rising income inequality, redistributive housing policy, and credit growth spurt. Testing this hypothesis with very long data for 14 advanced economies, B-M find evidence for a strong link from credit growth to financial instability, but observe no clear link from rising inequality to credit booms. However, none of the studies that follow B-M provides support for their conclusion on the inexistence of the inequality-crisis nexus. We attempt to clarify this controversy, but find it hard to corroborate B-M regardless of whether the same or different estimators are used as long as data sampling is altered in a more realistic manner. The estimators we use are OLS, GMM, and PMG while the sampling adjustments we make include grouping countries, partitioning sample periods, and adding a new covariate to consider the role of deindustrialization. Our estimation for

the financial effect of inequality has a theoretical base as modelled by Kumhof, Ranciere, and Winant (2015), and all its empirical results reach a conclusion that is opposite to B-M's but similar to several latest studies⁶.

All studies attempt to look for the true explanation for crisis risk: which one is getting closer to the reality? We feel it important to know why sharp differences in estimation results emerge between B-M and their followers. While there may be no problem to assert that the inequality, credit, crisis nexus is not a *general* relationship, it is problematic to claim the inexistence of such a nexus as a *general* conclusion. This point can be made clearer by looking back into empirical tests for the Solow growth theory, for a similar problem has ever arisen in these tests as described in Perkins et al. (2001, 64–71).⁶ Their description reveals a sensible trick to derive significant evidence by keeping to similar cross-sections within a panel-data model. A similar trick used in monetary economics is for testing the Phillips curve by keeping off structural changes within a time-series model (Dornbusch et al. 2002, pp.118–126).⁷ We utilize these old tricks in our OLS and PMG estimation, which thus sheds new light on the controversial issue of the inequality-crisis nexus. As shown in several latest studies (Perugini, Holscher, and Collie 2015; Kirschenmann, Malinen, and Nyberg 2016; Gu, Tam, and Lei 2019), however, system estimators such as GMM and DSUR for sufficiently dynamic specifications can generate similar insights into the issue without resort to the old tricks. These studies along with ours, albeit inspired by B-M, seem to become more and more supportive of the Rajan hypothesis. Even so, the issue remains open to debate as long as inconsistencies still exist.

⁶The Solow model predicts that poorer countries, if with the same potential level of income, will grow faster and eventually catch up to their richer peers at the steady state. If this were true, data plots would display a strong downward tendency in a graph, where initial levels of income per capita are plotted horizontally and subsequent rates of economic growth plotted vertically. But there is no such pattern evident in Figure 2–9 of Perkins et al. plotted from data for 124 countries over 1965–97. It is thus clear that there has been no *general* tendency. If anything, the opposite has been true. The reason for such a divergence of incomes among so many countries is that they are so different that the Solow condition of 'the same potential' is violated in terms of population growth, saving rates, production technology etc. The trick is to find a smaller group of countries plausibly with 'the same potential', as plotted in Figure 2–10 of Perkins et al. for 24 OECD countries over 1965–90 and in their Figure 2–11 for 30 open economies over 1965–94. In the latter two graphs with clear downward tendencies, countries within each of the two groups are sufficiently similar that strong evidence emerges for convergence of incomes among group member economies.

⁷The Phillips curve (relating inflation to unemployment rates) appears consistent with the Canadian data over a period up to the 1960s, but does not fit the data after the 1960s. The reason is that inflation had been running 8–10% in much of the 1970s up to the early 1980s but at low levels in other periods of time (e.g. about 2% in the late 1990s). People seeing a persistent inflation change will revise their expectations of future inflation. Thus there should be different Phillips curves for different expected inflation regimes. This reasoning justifies cutting the sample period into three sub-periods to make the data consistent with the inflation augmented Phillips curve. Although data plot for 1957–2000 exhibits no *general* pattern in Figure 7–4 of Dornbusch et al., this plot is found to actually portray three identifiable short-run Phillips curves for three sub-periods (i.e. 1957–71 plus 1992–2000, 1972–83, and 1984–1991) as depicted in their Figure 7–6.

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