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Essays on the consequences of financial expansion : growth, cycles and crises

Maxime Fajeau

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THÈSE

Présentée pour obtenir le titre de
Docteur en Sciences Économiques

ESSAIS SUR LES CONSÉQUENCES DE L'EXPANSION FINANCIÈRE : CROISSANCE, CYCLES ET CRISES

Maxime FAJEAU

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devant un jury composé de :

<i>Rapporteurs</i>	Olivier DAMETTE	Professeur des Universités Université de Lorraine
	Jérôme HÉRICOURT	Professeur des Universités Université de Lille
<i>Suffragants</i>	Alexandru MINEA	Professeur des Universités Université Clermont Auvergne
	Carine STAROPOLI	Maître de conférences, HDR Université Paris 1 Panthéon-Sorbonne
<i>Directeur</i>	Jean-Bernard CHATELAIN	Professeur des Universités Université Paris 1 Panthéon-Sorbonne



PARIS SCHOOL OF ECONOMICS
ÉCOLE D'ÉCONOMIE DE PARIS

Thèse réalisée à Paris-Jourdan Sciences Économiques - UMR8545
Université Paris 1 Panthéon Sorbonne
48 Boulevard Jourdan
75014 Paris

Web : <https://www.parisschoolofeconomics.eu>

Sous la direction de Jean-Bernard Chatelain jean-bernard.chatelain@univ-paris1.fr

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« Les hommes qui ont une foi excessive dans leurs théories ou dans leurs idées sont non seulement mal disposés pour faire des découvertes, mais ils font aussi de très mauvaises observations. Ils observent nécessairement avec une idée préconçue, et quand ils ont institué une expérience, ils ne veulent voir dans ses résultats qu'une confirmation de leur théorie. Ils défigurent ainsi l'observation et négligent souvent des faits très importants, parce qu'ils ne concourent pas à leur but. C'est ce qui nous a fait dire ailleurs qu'il ne fallait jamais faire des expériences pour confirmer ses idées, mais simplement pour les contrôler ; ce qui signifie, en d'autres termes, qu'il faut accepter les résultats de l'expérience tels qu'ils se présentent, avec tout leur imprévu et leurs accidents. »

Claude Bernard (1865)

Introduction à la médecine expérimentale

Avant-propos

Cette thèse, intitulée “Essais sur l’expansion financière : croissance, cycles et crise”, est composée de trois essais appliqués à l’analyse empirique des conséquences de l’expansion financière. Chaque essai correspond à un chapitre. Les liens et la logique entre les chapitres de la thèse sont présentés dans l’introduction générale. Toutefois, chaque chapitre peut être lu séparément.

⚠ *L’Université Paris 1 Panthéon Sorbonne et L’École d’économie de Paris n’entendent donner aucune approbation ni improbation aux opinions émises dans cette thèse. Ces opinions doivent être considérées comme propres à leur auteur.*

Résumé

La présente thèse propose une étude des conséquences de l'expansion financière initiée à la fin des Trente Glorieuses comme nouveau ressort d'une croissance économique alors en perte de vitesse. De manière incidente, ce travail se penche sur la problématique de la mesure des régularités économiques par le biais des outils économétriques. Ainsi, le premier chapitre conduit une analyse approfondie de la méthodologie qui guide une grande partie de la littérature empirique sur la relation finance-croissance. Il lève le voile sur un certain nombre de problèmes remettant en question la fiabilité des résultats antérieurs, dont notamment la conclusion selon laquelle la relation entre expansion financière et croissance économique serait régie par un effet de seuil. Nos estimations documentent en revanche un effet globalement négatif de l'expansion du crédit au secteur privé sur la croissance. Cet effet, qui s'est intensifié dans le temps, apparaît comme plus prononcé dans les économies avancées. L'analyse jointe de l'endettement privé et public, conduite dans le deuxième chapitre, montre que la dette publique tend à freiner la croissance, non pas initialement, mais à la suite d'une crise financière, si le gouvernement est incapable de conduire une politique budgétaire contra-cyclique. La trajectoire de l'endettement public est ici l'élément déterminant, plus encore que le niveau d'endettement. Le dernier chapitre, en étudiant le sauvetage des communes exposées aux emprunts structurés toxiques, offre une illustration d'une dérive de la libéralisation financière engagée en France dans les années 1980. Ce travail souligne toute l'ambivalence des fonds de soutien.

Mots clés : Finance · Croissance · Cycles économiques · Crises · Dette publique · Crédit privé · Économétrie appliquée · Évaluation des politiques publiques

**Essays on the Consequences of
Financial Expansion:
Growth, Cycles and Crises**

Abstract

The present thesis studies the consequences of the financial expansion initiated at the end of The Glorious Thirty to spur a declining economic growth. Incidentally, this work questions the issue of measuring economic regularities through econometric tools. Thus, the first chapter carries out an in-depth analysis of the methodology that guides a substantial part of the empirical literature on the finance-growth nexus. It highlights an array of statistical issues casting serious doubts on the reliability of previous results, including, in particular, the conclusion that the relationship between financial expansion and economic growth is non-linear. However, our estimates document an overall negative effect of expanding credit to the private sector on growth. This effect, which has intensified over time, appears to be more pronounced in advanced economies. The joint analysis of private and public debt, conducted in the second chapter, shows that public debt tends to be a drag on growth, not initially, but in the aftermath of a crisis if policy-makers are unable to pursue a counter-cyclical fiscal policy. It is not the level of public debt but rather its trajectory that affects economic growth. Finally, the last chapter investigates the bailout of municipalities exposed to toxic structured loans. It illustrates a failure in the financial liberalization initiated in France in the 1980s to stimulate local governments' financing. This work highlights the ambivalence of the support funds.

Keywords : Finance · Growth · Business Cycles · Crises · Public Debt · Private Credit
· Applied Econometrics · Policy Evaluation

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*
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Introduction Générale

« Les théories que nous possédons sont loin de représenter des vérités immuables. »

Claude Bernard (1865)

Motivations et contexte général

Récemment encore, les développements de la science économique ont pu nous laisser croire à la compréhension et à la maîtrise des phénomènes économiques. C'est ainsi que Robert Lucas, lauréat du prix Nobel d'économie, débuta son discours inaugural à l'American Economic Association en 2003, en proclamant avec confiance le succès de la macroéconomie :

“Macroeconomics was born as a distinct field in the 1940s, as a part of the intellectual response to the Great Depression. The term then referred to the body of knowledge and expertise that we hoped would prevent the recurrence of that economic disaster. My thesis in this lecture is that macroeconomics in this original sense has succeeded : Its central problem of depression prevention has been solved, for all practical purposes, and has in fact been solved for many decades.” (Lucas, 2003, p.1)

Il ne fut pas nécessaire d'attendre longtemps pour que la crise financière mondiale de 2007-2008 ne balaie d'un revers de mains tant d'optimisme. Mettant en lumière les dérives

du système financier, cette crise nous invite à repenser à ce que nous considérons jusqu'à présent comme acquis.

Claude Bernard, en systématisant la médecine scientifique¹, insista sur la nécessaire imbrication de trois composantes fondamentales : la physiologie, la pathologie et la thérapeutique, celles-ci s'apportant un mutuel appui. Tout comme la médecine, une approche similaire doit conduire l'économie à une analyse systématisée dans l'optique de mener des interventions de politique publique (équivalence de la thérapeutique). Cependant, l'analogie ne saurait être parfaite. En effet, si le corps humain répond à des mécanismes immuables, il n'en va pas de même pour l'économie. Il paraît donc nécessaire de réactualiser nos savoirs et de réévaluer nos conclusions.

“We are far from definitive answers to the questions : does finance cause growth, and if it does, how ?” (Levine, 2005, p.868)

La présente thèse n'entend pas fournir des réponses définitives, ni clore un débat ouvert il y a maintenant plusieurs décennies. Plus modestement, l'objectif de cette thèse est d'apporter un éclairage sur certaines failles de la science économique, et de présenter des réponses en traitant au mieux les différentes limites rencontrées jusqu'alors.

Aux origines de l'expansion financière

Il est indéniable que le système financier est aujourd'hui au cœur de l'économie moderne. Toutefois, l'idée selon laquelle le système financier est essentiel à l'activité économique est demeuré longtemps une considération triviale. En effet, la théorie néoclassique ne fait pas de la finance un cas d'étude. Le système financier est absent aussi bien de la théorie du consommateur que du producteur. L'absence de dimension temporelle dans l'étude néoclassique des choix des agents économiques explique pour beaucoup ce désintérêt. Si nous pouvons retrouver, au travers du *théorème de séparation* de Fisher (1907), l'idée qu'un système financier permet d'accroître l'utilité d'un agent économique en lui offrant la possibilité de dissocier sa décision d'épargne de sa décision d'investissement, le système financier demeure à ce stade une entité abstraite (un taux d'intérêt exogène), ne faisant pas l'objet d'une attention particulière. Le propos de Robinson (1952, p.80)

1. Introduction à la médecine expérimentale, 1865.

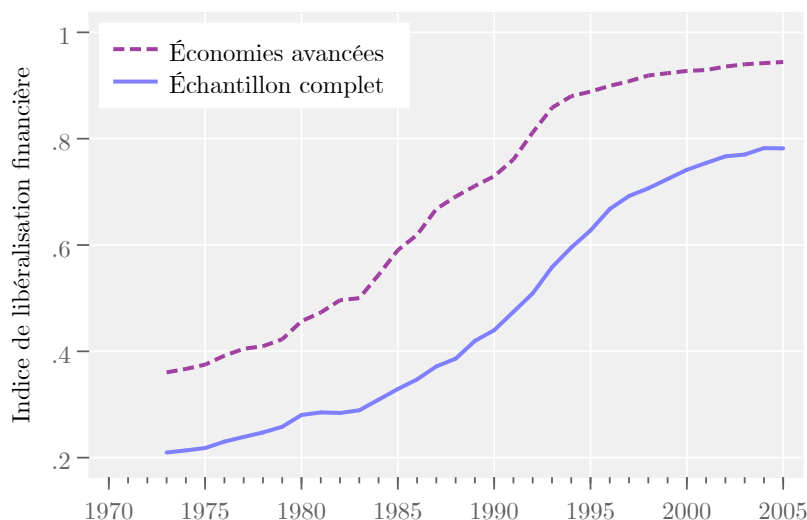


Figure .1 – Libéralisation du système financier dans le monde. Indice de libéralisation financière basé sur sept dimensions. Observations pour 91 pays. Source : *Database of Financial Reforms*, (Abiad, Detragiache et Tressel, 2010).

résume en substance le regard porté jusqu’alors sur le système financier comme auxiliaire de l’activité économique : “where enterprise leads, finance follows”.

À partir des années 1970 émerge l’idée qu’un système financier performant peut faire plus que suivre l’activité économique, et devenir un moteur de celle-ci. La fin des Trente Glorieuses, marquée par les premiers chocs pétroliers, est propice à la recherche de nouveaux leviers de croissance de l’activité économique. On redécouvre alors les arguments de Schumpeter (1911) en faveur du système financier, essentiel pour l’innovation et donc promoteur de l’activité économique. Ce n’est donc pas un hasard si les travaux pionniers de McKinnon (1973) et Shaw (1973) sont publiés précisément à cette période. Ils soulignent les bénéfiques – alors considérés comme indéniables – de la libéralisation financière pour l’activité économique, appelant de leurs vœux la fin de la *répression financière*. Celle-ci se manifeste pour ces auteurs par la prégnance de l’État dans le système financier. L’État oriente l’allocation du crédit vers des secteurs qu’il considère comme prioritaires, et impose par ailleurs des plafonds sur les taux d’intérêts ou encore des réserves obligatoires (Amable et Chatelain, 1995).

Un processus de libéralisation financière débute ainsi dans les années 1970, comme le souligne la figure .1. Une fois admis qu’un système financier peut faire une différence vis-

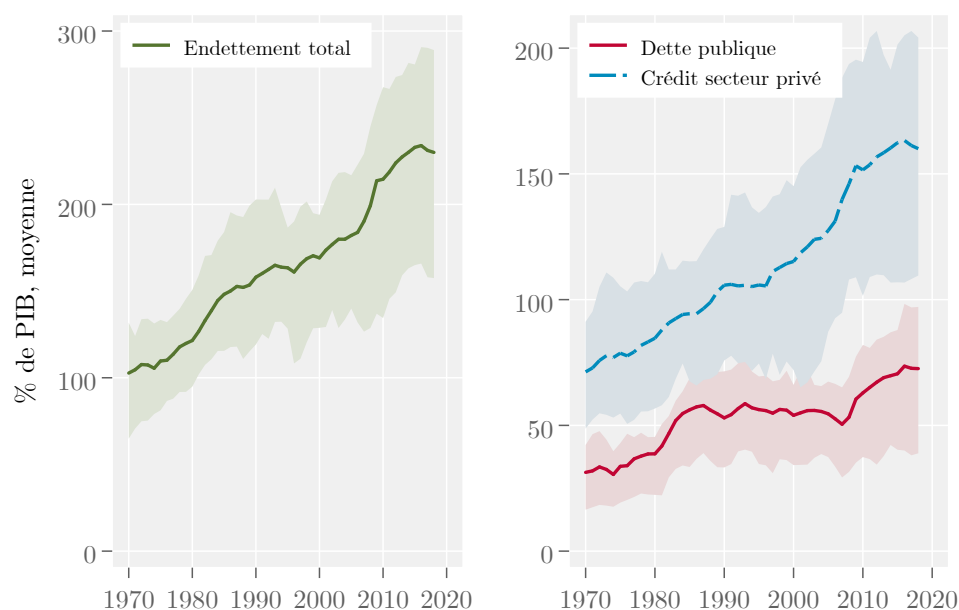


Figure .2 – Cinquante ans d’expansion du crédit au secteur privé et de l’endettement public. Observations pour 34 pays dans l’échantillon étudié dans le chapitre 2. Les intervalles de confiance représentent le premier et le troisième quartiles. Source : *Banque des Règlements Internationaux*.

à-vis de la prise de décision des agents économiques, et en conséquence, avoir un impact sur l’allocation des ressources, l’objectif devient alors de libérer le système financier des distorsions et des entraves imposées par l’État. La libéralisation financière est ainsi guidée par la recherche des bénéfices du premier théorème du bien-être selon lequel un marché en concurrence pure et parfaite permet d’atteindre un optimum de Pareto. Ce théorème peut être vu comme l’analogie actuel de la “main invisible” d’Adam Smith, où nous retrouvons la notion qu’un marché libre permet d’atteindre un optimum social. Cette politique de libéralisation sera activement promue par le Fonds Monétaire International et la Banque Mondiale dans de nombreux pays en développement. Le système financier peut alors jouer à la fois sur le volume et sur l’orientation de l’investissement, et par ce biais sur la productivité du capital (humain comme physique), autant de canaux importants de la croissance économique.

L’expansion financière apparaît alors comme une conséquence de ce phénomène de libéralisation et de dérégulation financière qui s’étend à partir de la fin des Trente Glorieuses (Rousseau et Wachtel, 2011), comme le souligne la figure .2. Cette expansion

financière se caractérise par un accroissement sans précédent non seulement du crédit alloué au secteur privé, mais également de l'endettement public². Le volume total d'endettement a ainsi plus que doublé depuis 1970, tiré en grande partie par le financement du secteur privé (entreprises et ménages).

Une littérature contradictoire : des bénéfices incertains

Présumer des effets vertueux de l'expansion financière sur le seul fait que le volume du crédit a plus que doublé serait trompeur. En effet, le bénéfice de l'expansion financière sur l'activité économique dépendra en grande partie du succès du système financier libéralisé à exercer ses fonctions. Le système financier doit ainsi permettre de mutualiser des ressources, assurer le transfert de celles-ci dans le temps et dans l'espace, assurer la liquidité des investissements, fournir des mécanismes de gestion des risques, révéler l'information et créer des outils permettant de résoudre les problèmes d'incitation (Merton, 1990).

Le système financier doit donc traiter un problème d'information afin de mettre en relation les agents à besoin de financement avec ceux possédant des capacités de financement. Les intermédiaires financiers, comme les banques, exercent un rôle central de collecte et de création d'information. Cela pour contourner deux problèmes principaux. La sélection adverse d'une part, qui survient dans le processus d'allocation, l'aléa moral d'autre part, qui intervient dans le déroulement de l'opération de financement. Un système financier qui traiterait avec succès les problèmes d'anti-sélection et d'aléa moral inhérents aux marchés financiers stimulerait certainement l'investissement dans un stock de capital plus productif. Par contraste, une économie dont le système financier dysfonctionnerait aurait toutes les chances d'être caractérisée par un degré élevé d'incertitude, des investissements improductifs et en conséquence, une croissance faible. Il est donc nécessaire d'évaluer si le système financier, par cette expansion, a rempli avec une plus grande acuité ses fonctions. L'accroissement du volume du crédit ne garantit pas à lui

2. Les termes *crédit* et *endettement* sont ici à entendre au sens large comme l'ensemble des passifs qui nécessitent une forme de paiement d'intérêt et/ou de principal par le débiteur au créancier, que celui-ci soit national ou international, à une date ultérieure. Voir Dippelsman, Dziobek et Gutierrez Mangas (2012) pour plus de détails sur la construction de cette variable.

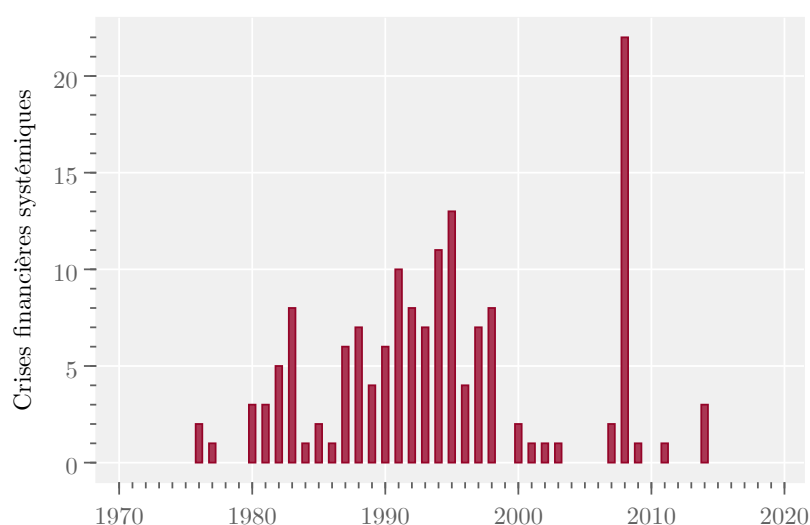


Figure .3 – Occurrence de 151 crises financières systémiques dans le monde depuis 1970. Source : *Systemic Banking Crisis Database* (Laeven et Valencia, 2018).

seul un bénéfice pour l'économie. Répondre à cette question est un problème empirique, qui sera l'objectif central de cette thèse.

Il faudra attendre le travail de King et Levine (1993) pour que la recherche se saisisse de cette question et approfondisse l'étude empirique des conséquences de l'expansion financière³. Cette étude est à l'origine d'une ligne de recherche construite autour d'un consensus sur les bénéfices de cette expansion⁴.

Par contraste avec les études documentant les bénéfices de l'expansion financière, le constat selon lequel l'expansion financière a été accompagnée d'une série de crises financières d'ordre systémique est sans appel. Les travaux de Laeven et Valencia (2018) ont ainsi permis de recenser non moins de 151 crises financières systémiques dans le monde depuis 1970 (voir la figure .3). La crise financière mondiale de 2007-2008 marque alors une rupture. Accompagnée par l'évidence des conséquences dramatiques de la crise, la relation positive entre la finance et la croissance semble disparaître à mesure que l'on considère des données après 1990 (Rousseau et Wachtel, 2011 ; Demetriades et Rousseau, 2016).

3. Pour une revue de littérature détaillée, voir Amable et Chatelain (1995) et Levine (1997) pour la littérature des années 1990, pour la contribution suivante, Levine (2005), et enfin, Popov (2018) pour les travaux plus récents.

4. Voir, parmi d'autres, De Gregorio et Guidotti (1995), Beck, Levine et Loayza (2000), Levine, Loayza et Beck (2000), Rioja et Valev (2004) et Demetriades et Law (2006).

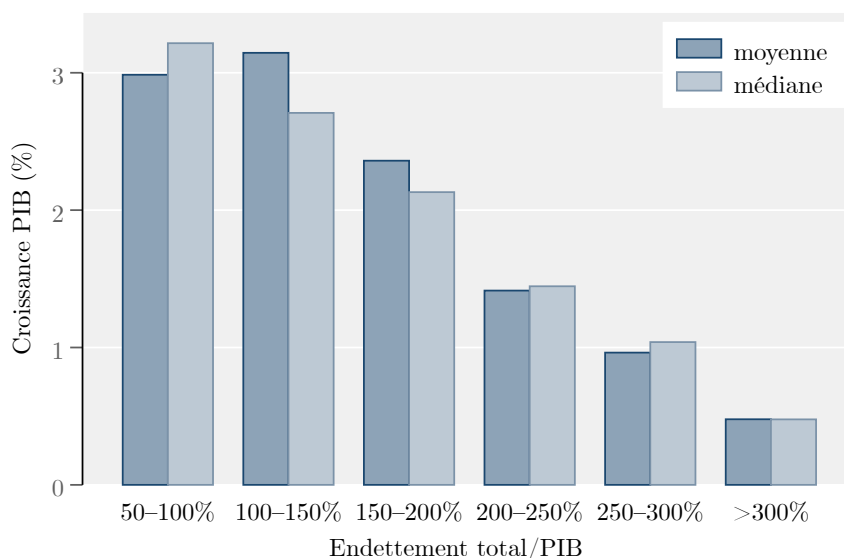


Figure .4 – Expansion financière et croissance. Ce graphique associe l’endettement total (privé et public) à la croissance observée en moyenne au cours des cinq années suivantes. Observations pour 34 pays dans l’échantillon étudié dans le chapitre 2. Source : *Banque des Règlements Internationaux*.

Le consensus sur les bénéfices de l’expansion financière se heurte alors à une littérature de plus en plus fournie sur le lien entre l’occurrence des crises et la croissance du crédit (Schularick et Taylor, 2012). Des travaux concluant à un effet globalement négatif de cette expansion financière sur la croissance se multiplient⁵.

L’absence de consensus devient alors caractéristique de la littérature empirique sur l’impact de l’expansion financière sur la croissance. Dans l’optique de concilier des résultats en apparence contradictoires, une branche de la littérature s’intéresse aux possibles effets de seuils. L’approfondissement financier aurait ainsi un effet bénéfique sur l’activité économique jusqu’à un certain seuil, estimé à 100% du PIB, qui une fois atteint, laisse place aux effets néfastes de la finance⁶.

Un effet de seuil similaire a été présenté par l’étude controversée de Reinhart et Rogoff (2010) concluant à un effet négatif de l’endettement public au delà de 90% du PIB. Leur méthode repose sur des histogrammes mettant en relation le taux de croissance avec le

5. Voir, parmi d’autres, Cournède et Denk (2015), Cecchetti et Kharroubi (2015), Karagiannis et Kvedaras (2016), Demetriades, Rousseau et Rewilak (2017), Benczur, Karagiannis et Kvedaras (2019) et Cheng, Chien et Lee (2020).

6. Voir notamment les contributions de Cecchetti et Kharroubi (2012), Arcand, Berkes et Panizza (2015), Benczur, Karagiannis et Kvedaras (2019) et Swamy et Dharani (2020).

taux d'endettement public. Il est frappant de constater que de nombreuses recommandations de politiques publiques se sont fondées sur ce seuil pour déterminer un objectif de dépenses publiques. Reprenant la méthodologie de Reinhart et Rogoff, la figure .4 souligne a priori une relation globalement négative entre la croissance de l'activité économique et l'expansion financière. Les niveaux élevés d'endettement total de l'économie étant associés à des niveaux faibles de croissance, et vice versa. Ainsi, les bénéfices de l'expansion financière ne semblent pas au rendez-vous. Cependant, il est nécessaire d'aller plus loin que cette simple association visuelle.

Évolution méthodologique

Au delà de cette évolution dans le regard porté à l'expansion financière, un mouvement parallèle s'est opéré quant aux méthodes d'estimation, au travers de l'essor de nouvelles méthodes économétriques.

Un premier questionnement a cherché à déterminer l'existence d'une association quelconque entre expansion financière et activité économique. À ce stade, les premiers travaux empiriques ont recours à des observations en moyenne par pays sur une longue période, en présumant que la relation étudiée ne varie pas substantiellement entre pays ou dans le temps. Par la suite, le recours à des données de panel a permis de mobiliser la dimension temporelle pour traiter le biais lié aux variables omises, constantes dans le temps (également connues comme "effets fixes").

La deuxième étape a consisté à établir si cette relation était causale. Pour dépasser les biais des analyses originelles en *cross-country*, et pour s'approcher d'une lecture causale dans la relation, une solution maintenant classique en économétrie consiste à utiliser des variables instrumentales. Il s'agit de variables corrélées à l'expansion financière, mais non corrélées à la croissance de l'activité économique autrement que par le biais de l'effet de l'expansion financière sur celle-ci. Cette technique permet de retirer le biais de causalité inverse dans la mesure. Au regard de la définition, de tels instruments sont manifestement difficiles à trouver.

La première contribution en ce sens est celle de La Porta et al. (1997) qui propose d'utiliser l'*origine légale* d'un pays comme instrument pour identifier l'impact causal de

la finance sur la croissance. L'origine légale d'un pays est basée sur les travaux de droit comparé et fait référence à la classification des systèmes légaux nationaux au sein de plusieurs familles (française, allemande, scandinave et anglosaxonne). Chaque famille induit un rapport à la loi et une protection différente des créiteurs, qui seraient déterminants dans l'expansion financière d'un pays. L'appartenance à une famille est suffisamment ancienne pour écarter tout effet de rétroaction avec la croissance économique.

Cependant, le recours aux origines légales comme instrument a soulevé de sérieux doutes quant à sa validité. Très usité en effet, puisque nous retrouvons ces origines légales notamment comme instrument pour la corruption, le taux d'imposition, le degré de régulation et de formalisme dans les procédures légales, l'inflation, les inégalités de patrimoine foncier ou bien encore la taille relative des PME. Chaque fois, l'utilisation de l'instrument suggère qu'il n'est pas valide dans ses autres emplois, l'instrumentation ne pouvant être valide au mieux que pour l'une de ces variables (Bazzi et Clemens, 2013). Cette énumération, non exhaustive, souligne en filigrane la grande difficulté que suscite la recherche d'instruments.

Si la recherche d'instruments dits "externes" est en soi un exercice délicat, l'essor de méthodes à variables instrumentales "internes" a permis de contourner – en apparence – cette difficulté. Ainsi, plutôt que de chercher une tierce variable pouvant servir d'instrument, les travaux empiriques plus récents ont exploité très largement les possibilités offertes par ces nouvelles stratégies d'identification, dans l'esprit de Arellano et Bond (1991), Arellano et Bover (1995) et Blundell et Bond (1998). Sans entrer dans une présentation exhaustive, sur laquelle nous reviendrons⁷, ces techniques reposent sur la création de variables instrumentales tirées parmi les observations passées des variables endogènes du modèle.

Ces méthodes à variables instrumentales "internes" obèrent, pour la plupart d'entre elles, toute considération quant à la qualité de l'instrumentation sous-jacente. Pourtant elles sont au cœur de la littérature économique sur les conséquences de l'expansion financière. Ce constat sera le point de départ de cette thèse, dont la première contribution est méthodologique.

7. Pour une présentation détaillée, voir l'annexe A du chapitre 1, pages 199 et 207.

En définitive, l'expansion financière est la conséquence d'une volonté de stimuler l'activité économique pour trouver un nouveau ressort de croissance à la fin des Trente Glorieuses, par la libéralisation du système financier. Cette expansion s'est accompagnée de la multiplication de crises financières systémiques, dont la dernière en 2007-2008 s'est avérée la plus importante depuis la crise de 1929.

À ce stade, deux questionnements émergent. Un premier porte sur le fond, au regard de l'ambivalence des études empiriques sur les bénéfices de l'expansion financière. Un deuxième questionnement porte sur la méthode d'estimation. Nous pouvons donc nous demander si ces deux questionnements ne sont pas liés. L'absence de consensus est-elle liée à un problème de fond ou de méthode ? Au regard de ces différentes considérations, la présente thèse traite le questionnement suivant : **quelle a été l'incidence de l'expansion financière sur l'activité économique et le comportement des agents ?**

Pour répondre à la question des conséquences de l'expansion financière, nous adoptons un raisonnement en deux temps. Un premier chapitre propose un éclairage méthodologique. Il s'agit d'évaluer notre capacité à mesurer une relation entre l'expansion financière et l'activité économique sur le plan macroéconomique. Les deux chapitres suivants proposent une contribution sur le fond au travers de deux regards sur la question.

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Synthèse des chapitres

Chapitre 1

Too Much finance or Too Many Weak Instruments ?

Motivation et contexte Nous avons exposé que l’essor de la recherche empirique s’était construit sur l’émergence de méthodes à variables instrumentales “internes”. Nous constatons que ces méthodes obèrent toute considération quant à la qualité de l’instrumentation sous-jacente. Ce constat est le point de départ de ce premier chapitre dont la contribution est méthodologique.

Le premier objectif, par ordre et non par importance, n’est pas de répondre à une nouvelle question mais de se concentrer sur ce qui a été fait dans une perspective méthodologique. Pour cela, nous nous intéressons à une évolution centrale de la recherche sur les liens entre finance et croissance : l’effet de seuil. Cette conclusion, nous l’avons dit, permet une lecture unifiée de résultats considérés jusqu’ici comme contradictoires. En ce sens, elle est une clef de voûte de l’analyse de l’expansion financière. Les travaux concluant à un tel effet non linéaire reposent sur de larges panels de pays, couvrant des périodes étendues et reposant sur une estimation à variables instrumentales internes de type *System-GMM*⁸.

Cette littérature récente a en commun, au-delà de sa conclusion, de reposer sur une méthodologie dont l’opacité empêche toute appréciation quant à sa fiabilité. En ce sens, si plusieurs études aboutissent à un même résultat, cela ne doit pas forcément nous conforter dans la véracité scientifique de ce dernier. La même méthode étant systéma-

8. Voir notamment Cecchetti et Kharroubi (2012), Arcand, Berkes et Panizza (2015), Benczur, Karagiannis et Kvedaras (2019) et Swamy et Dharani (2020).

tiquement employée, nous ne sommes pas à l’abri de répéter de manière chronique la même erreur.

Le présent chapitre entend ainsi prolonger sur le volet empirique un débat initié par Romer (2016), poursuivi par Chatelain et Ralf (2018b) et Blanchard (2018) sur les dérives de la macroéconomie comme discipline scientifique :

“The treatment of identification now is no more credible than in the early 1970s but escapes challenge because it is so much more opaque.” (Romer, 2016, p.1)

Ce constat, porté à l’origine sur l’essor de modèles d’équilibre général à dynamique stochastique (DSGE), trouve un écho tout particulier dans les développements économétriques depuis maintenant plusieurs années. Il est reproché à la macroéconomie théorique son manque de transparence et sa complexité, brouillant l’interprétation et la compréhension des conclusions.

La “pathologie de l’identification”, pour reprendre les mots de Paul Romer, n’est pas circonscrite au seul champ théorique. Le problème soulevé par l’instrumentation, notamment les instruments faibles, n’est pas nouveau. Pourtant, de nombreux travaux de recherche semblent oublier ce qui a été découvert concernant les problèmes – bien réels – d’identification⁹.

L’essor de nouvelles méthodes d’estimation à variables instrumentales “internes” a néanmoins substantiellement complexifié l’évaluation, au point de devenir une “boîte noire” (Roodman, 2009a). La démarche d’évaluation étant moins aisée, de nombreux travaux font l’hypothèse qu’elle n’est pas nécessaire. En effet, les travaux basés sur le System-GMM, proposant une évaluation complète de la qualité de l’instrumentation, sont rares, voire inexistants. Et ce, malgré les mises en garde répétées depuis plusieurs années :

“Many applications of instrumental variable procedures in the empirical growth literature [are] undermined by the failure to address properly the question of whether these instruments are valid.”
(Durlauf, Johnson et Temple, 2005, p.638)

9. Pour une revue de la littérature sur les questions d’identification par instrumentation, voir Stock et Yogo (2002), Murray (2006) et Mikusheva (2013).

“Judging by current practice, many researchers do not fully appreciate that popular implementations of [System-GMM] estimators can by default generate results that simultaneously are invalid yet appear valid. The potential for type-I errors—false positives—is therefore substantial.”

(Roodman, 2009a, p.1)

“Concern has intensified in recent years that many instrumental variables used in widely-cited growth regressions may be invalid, weak, or both.”

(Bazzi et Clemens, 2013, p.1)

Ce premier chapitre entend donc faire le point sur ces techniques à variables instrumentales internes en abordant une conclusion centrale de la littérature sur la relation finance-croissance : la présence d’un effet de seuil.

Stratégie empirique La stratégie développée dans ce premier chapitre repose sur deux volets : exploiter de nouvelles données et conduire une réflexion méthodologique sur le cadre d’analyse de l’expansion financière.

Tout d’abord, nous reconstituons une base de données couvrant, au plus, 140 pays entre 1960 et 2015. Nous mobilisons, dans l’optique de comparaison, une base plus ancienne ayant servi pour le travail central de Arcand, Berkes et Panizza (2015) concluant à un effet de seuil entre finance et croissance, sur une base de données allant de 1960 à 2010. Pour assurer la comparabilité des deux bases, nous collectons les données auprès des mêmes sources. Notre nouvelle base permet ainsi d’ajouter quelques pays supplémentaires, mais surtout d’allonger la dimension temporelle jusqu’en 2015, permettant de tenir compte de la crise financière de 2007-2008 dans le cadre d’analyse. Outre l’extension de la longueur de l’échantillon, il convient de noter que le nouveau jeu de données ne correspond pas exactement à l’ancien. Il y a des révisions de données inévitables, où les valeurs aberrantes sont reclassées comme manquantes, et certaines deviennent disponibles.

En mobilisant ces deux jeux de données, nous conduisons une analyse approfondie de la méthodologie qui guide une grande partie de la littérature empirique sur la relation finance-croissance. En ouvrant la “boîte noire” de plusieurs estimateurs à variables instrumentales internes, nous mettons au jour un certain nombre de problèmes qui re-

mettent en question la qualité des résultats selon lesquels la relation entre expansion financière et croissance économique serait non-linéaire. Cette analyse se construit autour de deux axes.

Premièrement, ce travail conduit une réflexion autour de la prolifération d'instruments. Une implémentation par défaut de l'estimateur System-GMM conduit ainsi à un nombre d'instruments qui s'accroît de manière exponentielle par rapport à la dimension temporelle du panel. En ce sens nous étudions un ensemble de techniques visant à limiter ce problème, et évaluons la sensibilité des conclusions.

Deuxièmement, nous proposons l'application de deux méthodes transparentes pour apprécier la qualité des instruments. Une première méthode, déployée en amont du processus de recherche, permet d'obtenir une première image très informative sur la qualité des instruments internes, et ce sans nécessiter la mise en place d'une estimation de type System-GMM. Cette méthode minimaliste, présentée par Blundell et Bond (2000) est ainsi très accessible. Nous complétons cette approche par une technique développée par Bazzi et Clemens (2013), en aval du processus d'estimation. Cette méthode n'a fait l'objet, à ce jour, d'aucune application dans cette ligne de recherche pourtant très dépendante des instruments internes. Elle a l'avantage d'apporter un éclairage probabiliste sur la qualité des instruments internes, en contournant l'absence de tests standards dans le cadre du System-GMM.

Enfin, pour prolonger ces considérations méthodologiques, ce premier chapitre conduit une réévaluation de la relation entre développement financier et croissance, en tenant compte au mieux des différentes limites soulevées précédemment.

Résultats principaux et implications Notre première conclusion est que les panels macroéconomiques sont particulièrement exposés aux instruments faibles. L'utilisation d'estimateurs à variables instrumentales internes ne peut s'abstraire d'une évaluation de la qualité de l'instrumentation. Les différents tests sur la qualité des instruments appliqués aux panels macroéconomiques conduisent systématiquement à révéler la faiblesse des instruments. Ainsi, ne pas traiter ce problème sur le fondement qu'aucun test standard n'existe conduit à retenir des résultats pouvant être substantiellement erronés.

Deuxièmement, le présent travail démontre que les effets de seuils dans la relation croissance-finance sont fallacieux. La conclusion d'une finance vertueuse jusqu'à un certain seuil de développement financier repose sur la prolifération d'instruments faibles. De plus, la structure même de l'estimation, par l'ajout d'un terme quadratique pour estimer une relation non-linéaire, expose le résultat à un biais lié à la présence de valeurs aberrantes. Ce chapitre résonne avec le travail de Karagiannis et Kvedaras (2016) qui souligne que l'effet de seuil disparaît lorsque l'on restreint l'échantillon aux pays de l'OCDE ou de l'Union européenne. Nos conclusions vont également dans le sens de Botev, Égert et Jawadi (2019) qui concluent à l'absence d'effet de seuil entre l'expansion financière et la croissance en mobilisant une technique sans instruments.

Enfin, ce chapitre, en proposant une estimation alternative tenant compte au mieux des différentes limites, conclut à un effet globalement négatif de l'expansion de crédit au secteur privé sur la croissance, bien que reposant sur des instruments faibles. Notre analyse souligne par ailleurs que cet effet négatif a eu tendance à s'intensifier dans le temps, et apparaît comme plus prononcé dans les économies avancées. En revanche, un effet ambivalent, possiblement bénéfique pour la croissance semble se dessiner aux premiers stades du développement d'une économie.

Ainsi, ce premier chapitre n'entend pas écarter totalement la possibilité d'un effet non-linéaire. Cependant, celui-ci ne serait pas basé sur le degré d'expansion financière, mais dépendrait plutôt du stade de développement atteint par une économie. Toutefois, déterminer avec précision un seuil s'avère un exercice délicat au regard de la sensibilité des estimations. Sur ce point, le premier chapitre souligne que la relation est bien plus complexe qu'une simple relation en cloche qui dépendrait du niveau de développement financier.

Cet approfondissement méthodologique offre par la même occasion un éclairage pour une variété de sujets reposant sur ces méthodes d'identification, au-delà de la seule littérature financière. Ce travail peut contribuer à réduire la marge de désaccord quant aux effets de l'expansion financière, non seulement en écartant la présence d'effets de seuil, mais surtout en proposant un cadre d'évaluation de la méthodologie employée jusqu'ici.

À partir de cette première étude qui analyse notre capacité à mesurer les conséquences macroéconomiques de l'expansion financière, nous avons voulu approfondir la question en tenant compte des limites exposées ici. Cette approche fait l'objet des chapitre 2 et 3, offrant chacun un éclairage différent sur les conséquences de l'expansion financière.

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Chapitre 2

The Interplay of Private and Public Debts

Motivation et contexte Le chapitre 2 de cette thèse s'inscrit dans le prolongement du chapitre 1 en analysant sous un angle différent les conséquences de l'expansion financière. Nous l'avons vu (figure .2), l'expansion financière a conduit l'endettement public et le crédit alloué au secteur privé à des niveaux encore jamais atteints. Cela soulève trois questions : ce niveau d'endettement est-il dommageable pour l'activité économique ? Dans l'affirmative, peut-on déterminer parmi l'endettement privé ou public celui qui est le plus dommageable ? Enfin, l'endettement privé et public interagissent-ils au cours des cycles et sur le moyen terme ?

Nous proposons de répondre à ces questionnements en comblant trois limites des travaux antérieurs. Premièrement, ce chapitre se construit au regard de la limite de l'estimation précédemment soulevée. Nous faisons le choix de recourir à une stratégie d'identification simple et transparente.

Deuxièmement, nous abordons la question de l'hétérogénéité. L'analyse des interactions entre endettement public et crédit au secteur privé prolonge le travail de Jordà, Schularick et Taylor (2016), portant sur une base historique remontant à 1870 avec un nombre restreint de pays. Plutôt que d'analyser la relation sur une centaine de pays ou sur une période temporelle couvrant plusieurs siècles, nous choisissons un panel plus restreint composé d'une trentaine de pays, observés sur une période plus récente.

En effet, fixer le champ d'analyse sur un groupe plus restreint de pays, sur une dimen-

sion temporelle relativement courte, permet de tenir compte de changements structurels. Si les études historiques peuvent apporter un éclairage essentiel sur les tendances et ruptures, en déduire des relations économiques stables semble délicat. Si tant est que nous puissions parler de lois économiques, leur existence est nécessairement circonscrite aussi bien dans le temps que dans l'espace. Ainsi, rejoignant le propos de cette introduction, les travaux de Jordà, Schularick et Taylor (2016) et plus récemment ceux de Eichengreen et al. (2019) montrent qu'une rupture s'est bien opérée dans la dynamique financière à partir de 1970, rupture que nous prenons comme point de départ. La fonction exercée par l'endettement public s'est depuis lors renversée, d'une finance des activités régaliennes vers le financement de biens publics. Les niveaux actuels d'endettement public sont ainsi sans précédent, en considération des fonctions exercées par cette dette.

Troisièmement, nous proposons une analyse jointe de l'endettement privé et public pour appréhender plus pleinement l'expansion financière, jusqu'alors étudiée soit sous l'angle de crédit au secteur privé, soit de l'endettement public. Le changement structurel des années 1970 a également conduit à un rapprochement entre crédit au secteur privé et endettement public, qui concourent tous deux à l'activité économique. D'un pays à l'autre, le périmètre du financement public/privé varie selon que l'activité repose sur l'initiative privée ou collective. Ainsi, le crédit alloué au système éducatif, au système de santé ou encore à la fourniture de certains services (infrastructure de transports ou de télécommunications par exemple) viendra gonfler le crédit au secteur privé ou l'endettement public, selon le pays.

Ne s'intéresser à l'expansion financière que sous l'angle du crédit au secteur privé conduit à ne retenir qu'une vision parcellaire de cette expansion. Nous retrouvons une trace de ce questionnement dans les premiers travaux sur le sujet :

“Financial systems that primarily fund private firms probably [sic.] provide more services than financial systems that simply funnel credit to the government or state enterprises.” (King et Levine, 1993, p.718)

“The main advantage of credit over other monetary aggregates is that it excludes credit to the public sector.” (De Gregorio et Guidotti, 1995, p.438)

Le financement de l'action publique a de ce fait été écarté dès l'origine, sur ce qui

ressemble plus à une opinion qu'à un réel fondement empirique. Depuis, l'étude de l'expansion financière a maintenu un cloisonnement sans porosité entre crédit au secteur privé et dette publique, que nous proposons de lever. En proposant une analyse jointe de l'endettement public et privé, ce chapitre contribue à réunir deux branches de la littérature ayant évolué séparément.

Stratégie empirique Ce second chapitre élargit l'approche financière en incluant des caractéristiques structurelles au travers du poids des différents bénéficiaires des fonds alloués par le système financier. Notre objectif est d'évaluer l'effet global de l'expansion financière, ainsi que l'effet de chaque composante, et *in fine* mettre au jour d'éventuels effets d'interaction. Pour ce faire, ce chapitre repose sur trois éléments.

Tout d'abord, pour mener à bien notre objectif, nous avons besoin de données détaillées sur les différents bénéficiaires de l'expansion financière. Depuis 2015, la Banque des Règlements Internationaux publie une répartition détaillée de l'activité d'emprunt du secteur public et du secteur privé non financier. Cette base offre plusieurs avantages. Le premier est la possibilité de décomposer l'endettement total d'une économie entre crédit au secteur public et crédit au secteur privé non financier. Deuxièmement, la construction de la base de données garantit l'absence de chevauchement entre les agents économiques et assure la cohérence des définitions entre les pays dans le temps, permettant des comparaisons fiables. Troisièmement, le secteur public regroupe les gouvernements central et local, contrairement aux travaux de Reinhart et Rogoff (2010) qui n'utilisent que la dette du gouvernement central. Par ailleurs, la définition des instruments financiers reste large : l'ensemble des passifs qui nécessitent une forme de paiement d'intérêts et/ou de principal par le débiteur au créancier, que celui-ci soit national ou international, à une date ultérieure (Dippelsman, Dziobek et Gutierrez Mangas, 2012). Enfin, en allant jusqu'à 2018, notre étude intègre la crise financière de 2007/2008 dans son champ d'étude.

D'autre part, notre étude porte sur deux horizons temporels étudiés successivement. Une analyse sur le moyen terme est complétée par l'analyse des cycles (récessions et crises financières). Le moyen terme est propre aux études sur la croissance qui cherchent à dépasser les effets de cycle. Nous nous tournons ensuite vers l'analyse des cycles (récessions

et crises financières) fournissant un éclairage sur les canaux expliquant les mécanismes observés à moyen terme. Cette deuxième partie reprend l'approche de Jordà, Schularick et Taylor (2016) au travers de projections locales développées par Jordà (2005).

Enfin, nous adoptons une stratégie d'identification simple et transparente pour les effets de moyen terme. Au regard des conclusions du chapitre 1 sur les instruments faibles, nous mobilisons un premier test en amont qui confirme que les instruments internes dans notre panel sont bien trop faibles pour traiter avec succès l'endogénéité. Les estimations fondées sur une stratégie mobilisant ces instruments seraient alors sévèrement biaisées (Stock, Wright et Yogo, 2002 ; Murray, 2006 ; Mikusheva, 2013).

Des simulations de Monte-Carlo montrent que pour un échantillon de taille modérée de l'ordre de $T = 30$ et $N = 20$, l'estimateur MCO à effets fixes performe correctement vis-à-vis du biais de Nickell (1981) ; au moins aussi bien si ce n'est mieux que les estimateurs à variables instrumentales (Judson et Owen, 1999 ; Cecchetti, Mohanty et Zampolli, 2011). Considérant la taille de notre panel, le présent article recourt à l'estimateur MCO à effets fixes. Un dernier avantage de cet estimateur, comparé aux autres estimateurs à variables instrumentales internes, est sa transparence.

Comme il est maintenant courant dans ce domaine de recherche, les variables explicatives entrent dans le modèle avec un décalage temporel pour éviter l'endogénéité résultant de relations simultanées. Ainsi, nous ne mobilisons pas une estimation à variables instrumentales, mais plus simplement le concept de causalité au sens de Granger, reposant sur la prévisibilité statistique du phénomène considéré (*Post hoc ergo propter hoc*). Si la variable X permet de prédire la valeur future de Y , on peut alors considérer que X cause Y au sens de Granger.

Bien entendu, cela ne suffit pas à obtenir une causalité au sens strict. Une variable qui sert à prédire n'est pas nécessairement la cause du phénomène : il se peut qu'une tierce variable Z cause Y et X et que X réponde avec anticipation par rapport à Y . Cependant, nous ne pensons pas que cet aspect soit une limite. Le système financier est au mieux un catalyseur qui peut faciliter ou freiner la croissance, plus qu'une cause intrinsèque de celle-ci. La cause réelle de la croissance se situe certainement quelque part dans l'économie réelle : émergence de nouvelles idées, innovations, progrès techniques,

découvertes de ressources naturelles, etc. Le système financier peut ainsi jouer un rôle dans la traduction de l'innovation en production, aider à concrétiser une idée. Ainsi, la causalité au sens de Granger est ici ce qui nous intéresse.

Toutefois, cela n'exclut pas entièrement le biais d'anticipation. Les attentes concernant les conditions de croissance future peuvent affecter les décisions de financement actuelles. Le traitement avec décalage temporel des variables explicatives est destiné à atténuer autant que possible ce biais. Comme pour les études empiriques précédentes, la lecture des résultats appelle à la prudence. Bien que la méthodologie puisse atténuer les problèmes de causalité inverse, une affirmation de causalité ferme reste au-delà de la portée de ce chapitre.

Résultats principaux et implications Dans l'ensemble, les conclusions de ce chapitre suggèrent de contenir le niveau des dettes privées et publiques, cependant pour des raisons différentes.

Tout d'abord, nos estimations soulignent que tous les types de finances ne se valent pas, certains étant nettement plus préjudiciables à la croissance que d'autres. Ainsi, l'approfondissement du crédit privé exerce un effet négatif, robuste et important sur la croissance future, corroborant une littérature maintenant abondante¹⁰. Le travail conduit dans le chapitre 1 montre que la relation entre finance et croissance s'est dégradée au cours du temps, avec un effet négatif qui s'est intensifié. Deux éléments, étudiés dans le chapitre 2, semblent pouvoir éclairer ce phénomène.

Premièrement, les estimations montrent que l'occurrence de crises financières, qui trouvent leurs racines dans l'expansion du secteur financier, expliquent une part importante de cet effet négatif. La période récente, avec la survenue de crises financières de très grande ampleur, sans précédent dans l'histoire contemporaine, ne peut que renforcer l'effet négatif observé (Romer et Romer, 2017).

Ensuite, l'étude de la structure financière montre qu'au sein du crédit au secteur privé, le crédit alloué aux ménages est de loin le plus dommageable, rejoignant le travail de

10. Voir, parmi d'autres, les travaux de De Gregorio et Guidotti (1995), Jappelli et Pagano (1994), Luintel et Khan (1999), Eichengreen et Leblang (2003), Beck et al. (2012), Cecchetti et Kharroubi (2015), Cournède et Denk (2015), Karagiannis et Kvedaras (2016), Demetriades, Rousseau et Rewilak (2017) et Benzur, Karagiannis et Kvedaras (2019).

Beck et al. (2012). En effet, le crédit aux ménages limiterait la consommation future et favoriserait l'émergence de bulles immobilières (Mian et Sufi, 2011 ; Mian, Sufi et Verner, 2017 ; Mian, Straub et Sufi, 2020). L'effet négatif du crédit au secteur privé a ainsi pu se renforcer dans le temps à mesure que la structure financière a connu une modification profonde avec l'essor du crédit destiné aux ménages.

Par contraste, la dette publique n'est généralement pas associée significativement à la croissance future¹¹. Elle peut néanmoins exercer une influence positive à des niveaux faibles. En revanche, les estimations sur les effets de moyen-terme suggèrent que des niveaux plus élevés de dette publique renforcent l'effet néfaste du crédit alloué au secteur privé.

L'analyse des cycles d'affaires permet d'étudier plus finement l'interaction entre le crédit privé et la dette publique. La conjonction des différents horizons produit ainsi une meilleure compréhension des mécanismes sous-jacents. Dans l'ensemble, les différentes estimations sur les cycles corroborent les conclusions de Jordà, Schularick et Taylor (2016). Les crises financières sont beaucoup plus dramatiques car elles entraînent un ralentissement plus marqué et une reprise plus lente par rapport aux récessions normales. Une expansion beaucoup plus importante du crédit au secteur privé précède et caractérise les crises financières. De plus, les estimations soulignent que la dette publique tend à freiner la croissance, non pas initialement, mais à la suite d'une crise financière, si le gouvernement est incapable de mener une politique budgétaire contra-cyclique. Un niveau élevé d'endettement public au début de la crise accentue les conséquences de l'expansion du crédit privé. Ainsi, l'intensification de l'effet négatif du crédit au secteur privé sur la croissance, constatée dans le chapitre 1, peut également s'expliquer au regard des difficultés rencontrées par de nombreux gouvernements pour conduire une politique budgétaire contra-cyclique à la suite de la crise de 2007/2008.

En préconisant de maintenir les niveaux d'endettement bas en période d'expansion, ce chapitre ne rejoint que très partiellement les conclusions de Reinhart, Rogoff et Savastano (2003) et Reinhart et Rogoff (2010). En effet, là où leurs travaux considèrent

11. Nos résultats ne corroborent pas l'effet négatif obtenu par certaines études sur l'endettement public, ce qui peut être dû à la non-prise en compte du vieillissement démographique par celles-ci.

qu'il existe un "seuil d'intolérance" pour la dette publique posant une limite structurelle à l'endettement, le présent chapitre ne fait pas tant état d'un seuil que de la nécessité de préserver un espace fiscal. La conclusion est ici plutôt que les gouvernements doivent veiller à préserver leur capacité à conduire une politique budgétaire contra-cyclique lors d'éventuels chocs économiques, dans une logique d'essence keynésienne. Cela nécessite généralement de reconstituer des capacités d'endettement lors des phases ascendantes du cycle économique, afin d'éviter l'émergence d'une dynamique insoutenable de la dette publique.

Ainsi, la conclusion de ce chapitre rejoint les travaux récents de Pescatori, Sandri et Simon (2014), Chudik et al. (2016) et Bernardini et Forni (2017), en soulignant que la trajectoire de l'endettement public est ici l'élément déterminant, plus encore que le niveau d'endettement. Aussi, au regard du contexte actuel, nos conclusions viennent en soutien des préconisations de Blanchard (2019) et plus récemment Blanchard et Summers (2020) qui invitent les décideurs publics à s'engager dans des politiques de relance contra-cyclique dans l'environnement actuel de taux bas.

Le cloisonnement entre le crédit privé et la dette publique dans la littérature masque ainsi des interactions significatives et substantielles, appelant à des recherches supplémentaires sur ce sujet.

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Chapitre 3

Le fonds de sortie des emprunts toxiques : une alternative au contentieux ?

Motivation et contexte Dans ce troisième chapitre, nous proposons une évaluation relevant de l'analyse microéconomique. Si, de prime abord, il peut sembler étrange de procéder à un tel changement méthodologique, nous pensons que ce travail a toute sa place dans cette thèse. Ce chapitre permet de dépasser les difficultés de la mesure d'un effet macroéconomique en adoptant un prisme différent. Il constitue une analyse sur un cas concret illustrant une dérive du développement financier, induite par la dérégulation financière des années 1980 en France : la crise Dexia. Celle-ci est ainsi la conséquence d'une volonté de développement financier.

En effet, jusqu'à la loi bancaire de 1984 et la loi de dérégulation financière de 1986, le financement des collectivités locales était sous contrôle public. En 1987, le *Crédit Local de France (CLF)* succède à la *Caisse d'Aide à l'Équipement des Collectivités Locales*, puis sera coté en bourse dès 1991, pour être privatisé en 1993. Issue de la fusion du *CLF* avec le *Crédit Communal de Belgique*, la banque Dexia devient alors une source majeure de financement du secteur public, à tel point qu'en 2010 un tiers de la dette publique locale est détenue par celle-ci (Cour des comptes, 2011).

Cette transformation du paysage financier des collectivités locales a eu pour objectif d'assurer l'expansion et l'innovation financière (Bertrand, Schoar et Thesmar, 2007). C'est ainsi qu'à partir du début des années 2000, un nouveau type de prêt voit le jour : le

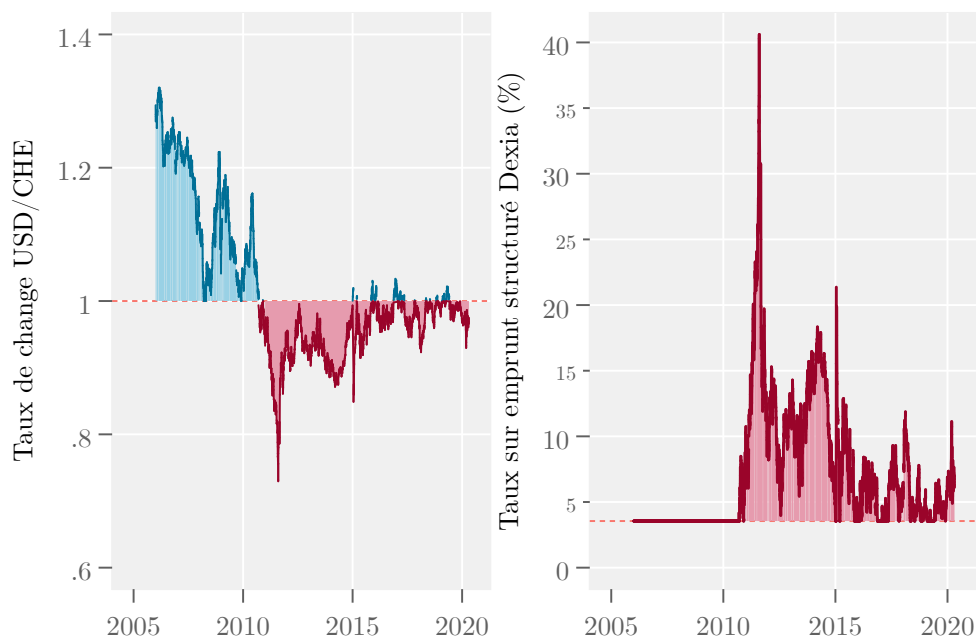


Figure .5 – Variation du taux sur emprunt structuré Dexia USD/CHE. Calculs de l’auteur sur la base d’un contrat structuré Dexia fondé sur une parité USD/CHE, présenté par Pérignon et Vallée (2017). Données : Federal Reserve Bank of St. Louis.

prêt structuré. Celui-ci consiste à adosser une option à un prêt classique, moyennant quoi la commune bénéficie d’un prêt à taux fixe bonifié pour les premières années, avant de basculer sur un taux d’intérêt variable, indexé sur un indicateur monétaire ou financier (comme un taux de change ou un écart de taux d’intérêt), avec éventuellement des effets d’échelle ou des multiplicateurs. Cette innovation financière devait permettre une allocation du capital plus efficiente ainsi qu’une meilleure répartition des risques.

Ces produits financiers structurés connaissent un essor substantiel jusqu’à ce qu’éclate en 2010 un scandale autour de ces prêts devenus toxiques pour les communes. En effet, les communes ayant contracté des emprunts structurés voient leur taux exploser. La crise financière mondiale de 2007/2008 a provoqué une dégradation de nombreux indices sous-jacents aux produits structurés. À partir de 2010, de nombreuses communes supporteront des surcoûts allant jusqu’à doubler le coût initial du prêt. La figure .5 illustre cette évolution sur la base d’un emprunt structuré Dexia, fondé sur la parité Dollar/Franc suisse, où le taux d’intérêt supporté par la commune s’envole pour atteindre les 40% en 2011.

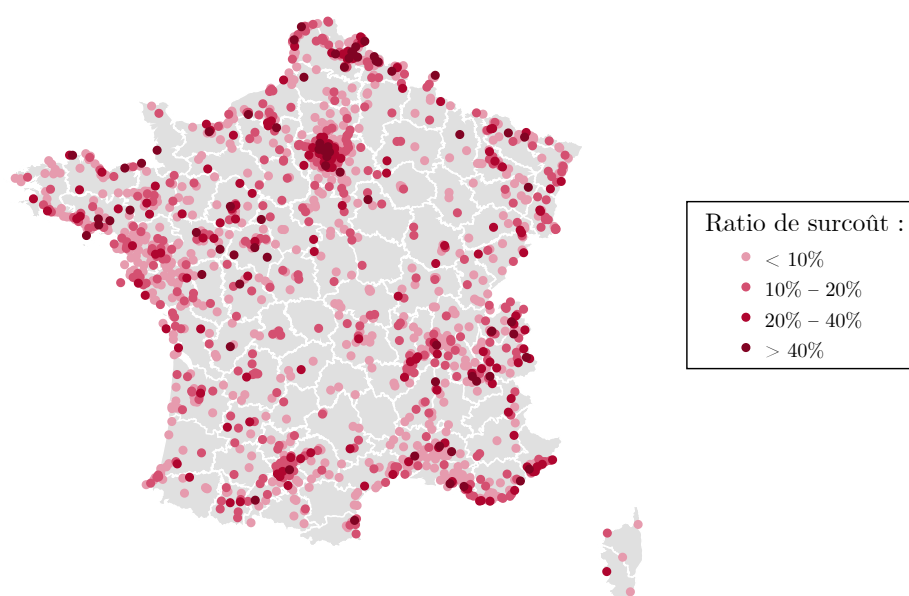


Figure .6 – Les communes touchées par les emprunts toxiques Dexia. Le ratio de surcoût sur les emprunts structurés Dexia mesure la valeur de marché du produit structuré en 2010 rapportée à la valeur initiale du produit au moment de la signature. C’est une mesure relative du coût supplémentaire supporté par la commune.

La crise Dexia est de grande ampleur. La banque Dexia était le principal pourvoyeur de ces contrats structurés, avec plus de 70% de ce marché, soit 9 milliards d’euros de prêts avant surcoûts (Bartolone et Gorges, 2011). Cette crise représente plus de 3000 emprunts toxiques, ayant touché près de 1600 communes. La figure .6 illustre ainsi l’étendue de la crise Dexia, qui a abouti en 2014 à la création d’un fonds de soutien par l’État pour venir en aide aux collectivités locales touchées par ces emprunts toxiques.

Face à cette situation, la réponse de l’État français sera séquentielle. Dès 2009, une première tentative de médiation entre les communes et les établissements de crédits sera menée, sans succès (moins de 50 accords de médiation signés). Dans le même temps, des communes vont opter pour la voie du recours judiciaire et obtiendront gain de cause devant le Tribunal de grande instance de Nanterre en 2013. Le tribunal décidera d’invalider le mécanisme de taux variable des contrats signés avec Dexia pour défaut d’information.

L’application de cette jurisprudence, très favorable aux communes (et aux finances

publiques locales), aurait pourtant posé un problème majeur en raison de la recapitalisation de la banque Dexia par la France, la Belgique et le Luxembourg. La perte de capital aurait représenté un coût potentiel de 17 milliards d'euros pour l'État français¹². C'est la raison pour laquelle, dans un second temps, le gouvernement va faire adopter une loi en 2014 pour invalider rétroactivement cette jurisprudence. En contrepartie, l'État décide de créer un fonds de sortie des emprunts structurés permettant de venir en aide aux communes contraintes de respecter un contrat devenu insoutenable pour les finances publiques locales.

Le fonds d'aide aux collectivités est donc un mécanisme d'État créé en 2014. Les communes devaient déposer un dossier de demande d'aide et recevaient (ou non) ensuite une proposition de refinancement. Enfin, l'acceptation de l'aide était conditionnée par l'abandon de toute poursuite devant les juridictions judiciaires. L'application de la jurisprudence du TGI de Nanterre aurait permis d'épargner aux communes la totalité du poids des intérêts devenus toxiques, alors que dans l'hypothèse d'une aide maximale par le fonds de soutien, celui-ci laisse au mieux 55% des indemnités de remboursement anticipé – très élevées – à la charge des communes.

Dans ce contexte, la finalité réelle du fonds peut être questionnée au regard de la chronologie de la gestion de cette crise (la volonté de l'État de ne pas laisser la justice venir en aide aux communes touchées par la crise de la dette toxique), d'une part, et sur la difficile lisibilité des critères d'attribution de l'aide du fonds. Par ailleurs, le fait que des communes aient finalement opté pour le contentieux et aient obtenu gain de cause interroge sur la stratégie du fonds : était-il destiné à venir en aide aux communes, ou bien simplement à limiter l'impact des contentieux sur la banque Dexia ?

Stratégie empirique Ce chapitre contribue essentiellement à deux littératures distinctes. Tout d'abord, il s'inscrit globalement dans la littérature sur les sauvetages financiers qui s'est saisie de l'étude des fonds de soutien en période de crise¹³. Cette

12. Selon les estimations de la Direction Générale du Trésor, présentées dans le rapport annuel de la Cour des comptes (2018).

13. Voir par exemple les contributions de Hagen et Dahlberg (2004), Poole (2009), Blau, Brough et Thomas (2013), Eichler et Hofmann (2013), Grossman et Woll (2014), Cabrera, Dwyer et Samartín-Saénz (2016) et Cooper et Nikolov (2018).

littérature, porte aussi bien sur les pays que sur les gouvernements locaux, ces derniers ayant fait l'objet de nombreux travaux (Asonuma et Trebesch, 2016 ; Bi, Chamon et Zettelmeyer, 2016). Plus spécifiquement, nous positionnons la contribution sur l'analyse des choix stratégiques en amont du sauvetage, là où de nombreux papiers étudient les conséquences en aval au travers du comportement des agents à la suite du sauvetage (Favara, 2003 ; Sorribas-Navarro, 2011 ; Dietrichson et Ellegård, 2015). Naturellement, considérer les choix stratégiques en amont nous amène à étudier une littérature d'économie des conflits et d'économie du droit, approche dictée ici par le contexte de la mise en place du fonds.

Ce chapitre s'inscrit donc également dans la littérature de l'économie des conflits, à laquelle nous empruntons le cadre d'analyse de la seconde proposition testable¹⁴. Il s'agit donc dans notre cas de la confrontation assez classique entre deux agents, avec deux solutions possibles : le recours contentieux ou le bénéfice d'une aide du fonds, ce dernier correspondant ici à une solution amiable (Alexander, 1991 ; Lederman, 1999 ; Kaplow et Shavell, 2002). La spécificité par rapport aux études précédentes est que nous considérons deux agents n'ayant jamais fait l'objet de travaux en économie des conflits : l'État, au travers des garanties offertes à la banque Dexia, et les communes. Cette approche apporte un regard nouveau à cette littérature qui s'est construite sur des litiges entre agents privés, consacrés principalement aux brevets et aux actions de groupe¹⁵.

Ce chapitre offre ainsi l'opportunité d'étudier non seulement un cas de contentieux entre des acteurs publics, mais aussi d'éclairer les critères déterminant la résolution d'un conflit par une solution amiable. Ces critères sont généralement inaccessibles car tenus secrets entre les agents privés (Boyd et Hoffman, 2013).

Nous élaborons donc deux propositions testables. Premièrement, nous considérons une stratégie de sauvetage financier tout à fait classique au regard de la littérature. Le sauvetage financier est la fonction annoncée par les critères réglementaires encadrant le

14. Voir notamment les contributions de Priest et Klein (1984), Cooter et Rubinfeld (1989), Cooter (1991), Kennan et Wilson (1993), Shavell (1997), Baumol (2000) et Lee et Klerman (2016).

15. Pour une littérature sur les brevets et les actions de groupe, voir Powell (1996), Crampes et Langinier (2002), Lemley et Shapiro (2005), Dickey, Orszag et Tyson (2010) et Lerner (2010).

fonds dans le cas étudié¹⁶. Cette stratégie est ensuite nuancée par une deuxième stratégie identifiée au regard du contexte ayant conduit à la création du fonds. Cette approche consiste à considérer que le fonds serait en réalité une alternative au contentieux.

Afin de répondre à notre question de recherche et de tester ces deux propositions, nous cherchons à déterminer les caractéristiques des communes ayant bénéficié de l'aide financière de l'État pour faire face aux emprunts toxiques. Nous mobilisons des données communales pour les 1542 communes ayant contracté des emprunts structurés Dexia au travers d'un logit qui associe à la décision d'octroi d'une aide du fonds, un ensemble de variables explicatives. Notre travail se distingue par le recours à une méthodologie économétrique spécifique, dite de l'AUROC (Schularick et Taylor, 2012), encore jamais mobilisée pour l'étude des sauvetages financiers d'agents économiques. Cette méthode permet de dissocier les vrais signaux positifs des faux signaux positifs, afin d'établir précisément les critères de choix du fonds, et notamment d'établir l'importance relative des stratégies considérées.

Résultats principaux et implications Nos résultats principaux indiquent que la stratégie d'évitement du contentieux a été privilégiée par le fonds. La prégnance du ratio de surcoût comme prédicteur du bénéfice du fonds dans nos estimations coïncide avec sa mise en exergue par les juridictions judiciaires pour sanctionner les contrats Dexia. En effet, cet indicateur a été utilisé par le TGI de Nanterre pour faire droit aux demandes des communes ayant opté pour la voie judiciaire après 2014.

La prévalence de ce critère, alors même qu'il ne figure pas parmi les critères réglementaires d'octroi du fonds, permet de comprendre davantage comment le fonds de soutien a choisi de verser une aide aux communes les plus susceptibles de se pourvoir en justice pour peser dans l'arbitrage des communes. Celles-ci ont alors le choix entre opter pour une aide du fonds, ou bien parier sur une victoire contentieuse qui, si elle peut être plus avantageuse, n'en reste pas moins incertaine.

À l'inverse, la plus faible prégnance des critères financiers pour prédire le recours au

16. Les quatre critères énoncés par le décret n° 2014-444 du 29 avril 2014 sont : (i) la dette par habitant, (ii) la part des produits structurés dans l'endettement total, (iii) la capacité de désendettement de la commune et (iv) le coefficient de mobilisation du potentiel fiscal.

fonds suggère que les communes qui ont été aidées n'étaient pas nécessairement celles qui étaient le plus en difficulté financièrement. Le fonds, sur ce point, semble s'être écarté de l'application des critères légaux. En ce sens, l'aide a été distribuée en partie sur une approche discrétionnaire en s'éloignant du cadre légal.

Dans ces conditions, notre travail contribue à la littérature sur les sauvetages financiers en apportant un éclairage sur les choix d'attribution d'une aide en amont du processus de sauvetage. De plus, le comportement d'arbitrage entre l'accord amiable et le recours contentieux ancre cette étude dans la littérature de l'économie des conflits et de l'économie du droit. La prégnance du ratio de surcoût rend compte d'une volonté, d'une part, d'éteindre au plus vite la crise Dexia, afin d'éviter des contentieux judiciaires longs et coûteux ; d'autre part, d'opérer une modification de la charge de ces emprunts toxiques en proposant une répartition entre les collectivités locales et l'État que ne permettait pas l'application de la jurisprudence du TGI de Nanterre qui faisait peser intégralement le coût de la crise sur l'État.

Ces résultats sont donc particulièrement utiles pour comprendre à la fois les différents comportements stratégiques lors des sauvetages financiers d'agents économiques, mais aussi pour éclairer les jeux qui peuvent exister entre les acteurs publics eux-mêmes, ici les gouvernements locaux face au gouvernement central. L'étude des fonds de soutien pose donc des questions essentielles, non seulement sur leurs conséquences, mais également sur leur stratégie de sauvetage au travers du choix des agents à sauver. Au regard de nos résultats, nous pouvons affirmer que les fonds de soutien ne visent pas uniquement le sauvetage des agents touchés par une crise. En effet, nos analyses empiriques montrent qu'un fonds de soutien peut jouer un rôle central de répartition de la charge de la crise entre les acteurs : il sert de catalyseur.

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Panorama du chapitre 1

TOO MUCH FINANCE OR TOO MANY WEAK INSTRUMENTS ?

Motivations

- ▶ L'effet de seuil entre finance et croissance repose sur des méthodes d'identification à variables instrumentales dont la fiabilité n'a pas été étudiée dans la littérature finance-croissance.
- ▶ Question de recherche : La prolifération d'instruments faibles compromet-elle la qualité des résultats suggérant une relation en cloche entre finance et croissance ?

Méthodologie et données

- ▶ Deux bases en panel couvrant 140 pays entre 1960 et 2015.
- ▶ Estimateurs variés : Identification par l'hétéroscedasticité, System-GMM, Difference-GMM, MCO Effets fixes, Hansen Panel Threshold.
- ▶ Tests sur la prolifération d'instruments.
- ▶ Tests sur la présence d'instruments faibles, par les 2SLS-Analogs notamment.

Résultats

- ▶ L'effet de seuil repose sur la prolifération d'instruments faibles, voire non valides.
- ▶ La variable financière est un supprimeur classique qui, combiné à quelques outliers, provoque une estimation quadratique fallacieuse. Cet effet est amplifié par la prolifération d'instruments faibles.
- ▶ La prise en compte de l'hétérogénéité associée à une utilisation plus raisonnable des instruments conduit à la présence d'un effet globalement négatif du développement financier sur la croissance.
- ▶ Cet effet négatif a eu tendance à s'intensifier dans le temps et apparaît comme plus prononcé dans les économies avancées.
- ▶ En revanche, un effet ambivalent, possiblement bénéfique pour la croissance semble se dessiner aux premiers stades du développement d'une économie.

Panorama du chapitre 2

THE INTERPLAY OF PUBLIC & PRIVATE DEBTS

Motivations

- ▶ Le développement financier a conduit la dette publique et le crédit alloué au secteur privé à des niveaux jamais atteints.
- ▶ Si des études ont porté sur la dette publique et le crédit au secteur privé, l'analyse jointe de leurs effets demeure jusqu'ici parcellaire.
- ▶ Questions de recherche : le niveau actuel d'endettement est-il dommageable pour l'activité économique ? Dans l'affirmative, peut-on déterminer parmi l'endettement privé ou public celui qui est le plus dommageable ? Enfin, les endettements privé et public interagissent-ils au cours des cycles et sur le moyen terme ?

Méthodologie et données

- ▶ Données de la Banque des Règlements Internationaux offrant une granularité sur l'endettement total : crédit alloué au secteur public, aux entreprises non financières et aux ménages.
- ▶ Analyse à moyen-terme : MCO effets fixes, Hansen Panel Threshold, ARDL.
- ▶ Analyse des cycles : Local Projections, Panel VAR et Probit.

Résultats

- ▶ Sur le moyen-terme, le crédit au secteur privé exerce un effet négatif. Cet effet négatif est principalement généré par le crédit alloué aux ménages, par essence improductif.
- ▶ Une part substantielle de l'effet mesuré à moyen terme est dû aux effets de cycles. Le crédit au secteur privé a un impact négatif lié au déclenchement de crises financières.
- ▶ L'endettement public et le crédit au secteur privé interagissent essentiellement au cours des cycles associés à des crises financières.
- ▶ Si l'endettement public n'est pas en soi préjudiciable à la croissance, il le devient lorsque, à la suite d'une crise financière, le manque de surface fiscale empêche le gouvernement de conduire une politique de relance.

Panorama du chapitre 3

FONDS DE SORTIE DES EMPRUNTS TOXIQUES : UNE ALTERNATIVE AU CONTENTIEUX ?

Motivations

- ▶ À la suite de la crise Dexia ayant touché les finances publiques locales, l'État a décidé de procéder au sauvetage des communes en difficulté.
- ▶ Loi rétroactive sécurisant les emprunts toxiques et création d'un fonds alors que la jurisprudence du TGI de Nanterre aurait permis d'épargner aux communes la totalité du poids des intérêts toxiques.
- ▶ Question de recherche : le fonds de soutien était-il destiné à aider les communes en difficulté financière, ou simplement à limiter l'impact des contentieux sur la banque Dexia ?

Méthodologie et données

- ▶ Expérience naturelle permettant de mesurer la probabilité d'octroi d'une aide du fonds au regard de plusieurs ensembles de critères (notamment réglementaires).
- ▶ Modèle Logit avec analyse de l'AUC de Schularick et Taylor (2012). Modèle de Poisson, Penalized Maximum Likelihood Logit de Firth (1993) et Bayesian Model Averaging en robustesse.
- ▶ Quatre jeux de données : Bilans détaillés des communes (DGFIP), Base *Comparateur des territoires* (INSEE) pour les dimensions socio-économiques, portefeuille des emprunts structurés Dexia (*Libération*), *Répertoire National des Élus* (Ministère de l'Intérieur).

Résultats

- ▶ Le ratio de surcoût explique davantage l'octroi d'une aide du fonds que les critères légaux ou les indicateurs financiers.
- ▶ La prégnance du ratio de surcoût comme prédicteur du bénéfice du fonds dans nos estimations coïncide avec sa mise en exergue par les juridictions judiciaires pour sanctionner les contrats Dexia.
- ▶ Les communes qui ont été aidées n'étaient pas nécessairement celles qui étaient le plus en difficulté financièrement.
- ▶ Le fonds de soutien a choisi de verser une aide aux communes les plus susceptibles de se pourvoir en justice pour peser dans leur arbitrage.

“Concern has intensified in recent years that many instrumental variables used in widely-cited growth regressions may be invalid, weak, or both”

Samuel Bazzi and Michael A. Clemens (2013)

“The GMM approach [...] is often deemed to be a black box with results which are highly dependent on the set of instruments included in the equation and the way in which researchers decide to treat the various variables [...]. The profession, which until ten year ago considered this approach the Holy Grail of causality, is now more skeptical of causality claims that only rely on internal instruments.”

Ugo Panizza (2013)

Too Much Finance or Too Many Weak Instruments?

Abstract – Since the global financial crisis of 2008, a strand of the literature has documented a threshold beyond which financial development tends to affect growth adversely. The evidence, however, rests heavily on internal instrument identification strategies, whose reliability has received surprisingly little attention so far in the finance-growth literature. Therefore, the present paper conducts a reappraisal of the non-linear conclusion twofold. First, in light of new data, second, by a thorough assessment of the identification strategy. Evidence points out that a series of unaddressed issues affecting the System-GMM setup results in spurious threshold regressions and overfitting of outliers. Simple cross-country analysis still suggests a positive association for low levels of private credit. However, adequately accounting for country heterogeneity, along with a more contained use of instruments, points to an overall damaging influence of financial development on economic growth. This association is stronger for more recent periods.

Keywords Finance · Growth · Non-linearity · System-GMM · Panel Data

JEL classification C52 · E44 · G1 · O11 · O16

1 Introduction

Financial development as a source of growth has been the subject of renewed interest since the wake of the 2007/8 crisis. A decade after the financial crisis, this paper intends to contribute to the debate in light of new data and advances in econometric techniques.

Is financial development a leading factor for growth, and if so, should we stimulate its deepening? No straight answer has emerged. The absence of a consensus is already a defining characteristic of the finance-growth literature, notably on the direction of causality.¹

The finance-growth literature and the banking crises literature have left many researchers with conflicting and contradictory findings. Up to the financial crisis, the literature has been quite confident regarding the growth-enhancing properties of financial sector's expansion (King and Levine, 1993; Levine, Loayza, and Beck, 2000; Rioja and Valev, 2004; Demetriades and Law, 2006). However, considering more recent data, Rousseau and Wachtel (2011) show that the positive relationship between finance and growth is not as strong as it was in previous studies using data prior to 1990. Focusing on an alternative proxy for financial development, Capelle-Blancard and Labonne (2016) show that there is no positive relationship between finance and growth for OECD countries over the past 40 years. Demetriades and Rousseau (2016) also find that financial depth is no longer a significant growth determinant. Together with the evident damaging impact of the financial crisis on subsequent economic growth (Kaminsky and Reinhart, 1999; Jordà, Schularick, and Taylor, 2016; Grjebine and Tripier, 2017), these findings have led several studies to reconsider prior conclusions and investigate potential non-linearities.

To provide a convincing reading through these puzzling conclusions, a strand of the literature has investigated whether there is evidence of a threshold in the finance-growth relationship (see, for instance, the contribution of Cecchetti and Kharroubi, 2012; Arcaud, Berkes, and Panizza, 2015; Benczur, Karagiannis, and Kvedaras, 2019; Swamy and Dharani, 2020). The later studies conclude that financial deepening starts harming

1. For a detailed literature review, see Amable and Chatelain (1995), Levine (2005) or Popov (2018).

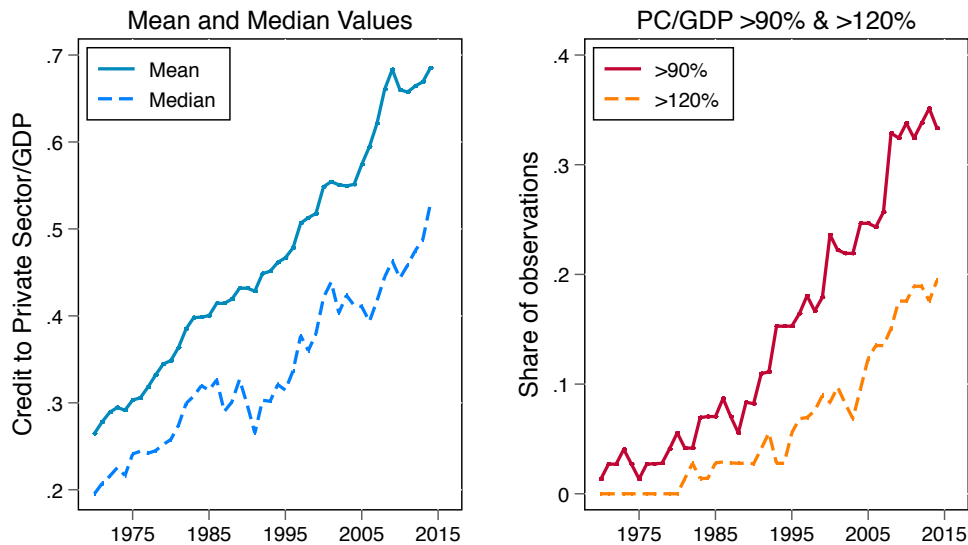


Figure I.1 – Evolution of the ratio of credit to the private sector over GDP as a proxy of financial depth, based on the new expanded dataset for 140 countries over 1970-2015. The left panel plots the mean and median values of private credit. The right panel plots the share of observations for which private credit is above 90% (solid line) and 120% (dashed line).

output growth when credit to the private sector roughly reaches a certain threshold somewhere around 100% of GDP. In other words, the non-linear conclusion implies that the financial sector can grow too large for society’s benefits. Such a finding has tremendous policy implications. If financial expansion can still offer additional growth for developing countries, the level of credit to the private sector of most developed economies is often well beyond this estimated limit (see Figure I.1). Therefore, a decade of expansionist monetary policy, easing private credit, could have an ambivalent outcome.

Far from gaining the full support of the entire economic community, several recent contributions have shed doubt on the robustness of a monotonically positive link between finance and growth. Karagiannis and Kvedaras (2016) challenge earlier results and find that the non-linear conclusion is no longer present when restricting the panel to the OECD or the EU countries. Such evidence emphasizes that the threshold estimates could be a byproduct of unaccounted heterogeneity. Based on various dynamic threshold estimates, Botev, Égert, and Jawadi (2019) also fail to find a non-linear association between finance and growth. It further suggests that the threshold estimates are likewise sensitive to the estimation technique.

In line with this inconclusive literature, the present paper seeks to understand why prior evidence relying on large panels led to non-linear conclusions. The primary contributions of this paper are: i) an in-depth reassessment of the mainstream empirical methodology, ii) the inclusion of data since the global financial crisis, iii) new and comprehensive evidence on the overall finance-growth nexus. The present study reassesses the non-linear evidence twofold. Firstly, by using more data. The new dataset results in additional countries and observations. It extends the scope of the study up to 2015, thereby including additional post-crisis observations. Second, in reexamining the non-linear conclusion in its original methodological environment, this study also sets the focus on the soundness of the econometric methodology.

The finance-growth nexus is no exception to the well known empirical struggle to identify a causal impact. Moving beyond mere statistical association is commonly achieved based on instrumental variables techniques to extract the exogenous component of financial development in a macroeconomic setup. The recent non-linear finance-growth literature heavily relies on internal instrument identification strategies in the spirit of Arellano and Bond (1991) and Arellano and Bover (1995).² However, only limited attention is drawn to the potential fragility of such System-GMM identification strategies (for recent examples, see Cheng, Chien, and Lee, 2020; Swamy and Dharani, 2020). Following advances in econometric research, this study takes a look under the hood of the System-GMM estimator. To do so, it focuses on alternative specifications to avoid the default implementation pitfalls and provides tests to assess the instruments' strength. This paper discusses the assumptions underlying the validity of the identification strategy, and thereby the reliability of the threshold estimates. This study is, therefore, the first to provide a thorough appraisal of the internal instrument identification strategies in the non-linear finance-growth literature.

This study provides a body of evidence that dismisses the relevance of a threshold in the finance-growth nexus. It shows that uncontrolled country-specific factors and a few

2. The influential contribution of La Porta et al. (1997) and La Porta et al. (1998) suggested the predetermined legal origin of a country as an external instrument for identifying the causal impact of finance on growth. The "legal origin" instrument, while widely used for a time, has been recast by Bazzi and Clemens (2013) because its widespread use to instrument a variety of endogenous variables could only lead to valid instrumentation in at most one of the study. And at worst none.

outliers are driving former hump-shaped conclusions. This paper provides evidence calling into question the soundness of the various identification strategies. It demonstrates that the conclusion of a non-monotonic causal impact of finance on growth relies on a very large number of either irrelevant or weak instruments. These problematic instruments prevent reliable causal inferences about the effect of financial depth on growth. Further evidence suggests that the near-multicollinearity of the financial proxies, combined with the weak instrument proliferation issue, fosters spurious regressions overfitting a few outliers.

Finally, this study further contributes to the literature by establishing an overall negative finance-growth nexus, with a stronger emphasis in recent periods. The relationship between finance and growth has degenerated over time, reinforcing the intuition that no economic association is an immutable law (Rousseau and Wachtel, 2011). These estimates are robust to various time coverages, three estimation techniques, two datasets, and several empirical exercises.

Evidence suggests that the negative influence of financial depth is also stronger in high-income countries, with ambivalent yet possibly growth-enhancing effects at the earliest stages of development. Thereby, these new estimates are not ruling out the plausibility of a non-monotonous finance-growth nexus, not based on the level of financial depth, but rather on the development stage of an economy.

Furthermore, the estimates reveal that financial distress is a substantial channel underlying private credit's adverse effect. In contrast, financial reforms have been rather growth-promoting, corroborating the evidence in Demetriades and Rousseau (2016) and Demetriades and Rewilak (2020) calling for sound supervision and regulation of the financial system.

Overall, these empirical findings support the hypothesis that financial deepening has done more harm than good in the long run. This conclusion is in line with recent studies providing similar evidence (Cournède and Denk, 2015; Cecchetti and Kharroubi, 2015; Karagiannis and Kvedaras, 2016; Demetriades, Rousseau, and Rewilak, 2017; Cheng, Chien, and Lee, 2020).

The paper proceeds as follows. Section 2 overviews data and methodology. Section 3

provides some preliminary comments on cross-country regressions. The paper delves into a complete reappraisal of the threshold estimates based on panel data estimates in section 4. Then, section 5 provides alternative estimates unveiling a damaging impact of financial deepening. Finally, section 6 concludes this study.

2 Data and Methodology

2.1 Data and Variables

The dataset is gathered from the usual sources. Throughout the study, the independent variable is economic growth, measured as the log-difference of real GDP per capita (WDI, World Bank, 2018). The study rests on credit to the private sector by deposit money banks and other financial institutions as a ratio of GDP (private credit) as a proxy for financial depth. The amount of credit allocated to the private sector is commonly used by the finance-growth literature as it is likely to be associated with the functions exerted by the financial sector that can impact the growth path of an economy.³

It is undoubtedly an imperfect substitute for the much broader concept of financial development. However, it has two main advantages. First, it has become one of the most commonly used financial depth measures and, thus, facilitates comparison with previous empirical work. Second and most importantly, among all proxies, private credit has the largest time and country coverage, which is essential when conducting a broad cross-country comparison. This variable is provided and actualized by Beck, Demirguc-Kunt, and Ross (2000) and Cihak et al. (2012).

All regressions are conducted based on a set of policy and nonpolicy control variables correlated with growth performance across countries. These control variables are common to growth empiric literature: the logarithm of initial GDP per capita to capture convergence and provide a dynamic feature in the panel estimates (Barro, 1991; Bun and Sarafidis, 2015), average years of education gathered from Barro and Lee (2013) to

3. Levine (2005) reports several financial functions, which boils down to producing ex-ante information about investment opportunities; improving ex-post monitoring of investment and exerting corporate governance; facilitating risk management and diversification; mobilizing and pooling savings; easing the exchange of goods and services.

account for human capital, a measure of trade openness⁴ (Frankel and Romer, 1999), and two measures of macroeconomic stabilization, the log of the inflation rate⁵ (Barro, 1995; Rousseau and Wachtel, 2002) and the log of government consumption normalized by GDP to account for fiscal adjustments (Alesina and Wacziarg, 1998; Sala-i-Martin, Doppelhofer, and Miller, 2004).

For comparison purposes with existing literature, this study also works with an older dataset gathered from Arcand, Berkes, and Panizza (2015). This older dataset ranges from 1960 to 2010. Besides extending the sample length, it is worth noting that the new dataset does not exactly match the former. There are inevitable data revisions, where some values are reclassified as missing, and some become available. The correlations, however, are usually close to 0.98 within the sample (including the proxy for financial depth), except for the government consumption ratio, which is 0.94.

The new dataset results in additional countries and observations. It extends the scope of the study up to 2015, thereby including additional post-crisis observations. The paper focuses on the most extended period range. Indeed, one of the alleged strength of the non-linear estimates is to remain statistically significant in long samples where other linear specifications fail to find a significant association between finance and growth. The number of countries varies slightly depending on data availability and is always displayed in the estimation tables. For more details, see the Data Appendix page 180.

2.2 Empirical Methodology

This study aims to reassess the finance-growth relationship, with a particular focus on the non-linear finding in its original methodological environment. A host of empirical papers have found evidence of a threshold in the finance-growth relationship. From a methodological perspective, they boil down to dynamic panel data estimates based on System-GMM estimator using five-year periods to smooth out business cycle (Cecchetti and Kharroubi, 2012; Arcand, Berkes, and Panizza, 2015; Sahay et al., 2015a; Benczur, Karagiannis, and Kvedaras, 2019; Cheng, Chien, and Lee, 2020).

4. Computed as exports plus imports divided by GDP and gathered from WDI, World Bank (2018).

5. To deal with possible negative value using log of this variable, we apply the inverse hyperbolic sine transformation: $\ln(\text{infl}) \equiv \ln\left(\text{infl} + \sqrt{\text{infl}^2 + 1}\right)$. The data is gathered from WDI, (World Bank, 2018).

The standard estimated model proceeds as follows. Define the logarithmic growth in real GDP per capita for country i between t and $t + k$ as:

$$\Delta y_{i,t+k} = \frac{1}{k} \sum_{j=1}^k (y_{i,t+j} - y_{i,t-1+j}) \quad (\text{I.1})$$

which translates into the average annual growth rate of per capita GDP. For a five-year spell, i.e. $k = 5$, equation (I.1) simplifies as:

$$\Delta y_{i,t+5} = \frac{1}{5} (y_{i,t+5} - y_{i,t})$$

Let's denote $y_{i,t}$ as the initial level of log GDP per capita, and y_i^* the long-run (or steady-state) value. Generic forms of growth estimation equation are usually obtained from a first-order approximation of the neoclassical growth model (Mankiw, 1995), such that one can derive:

$$\Delta y_{i,t+k} = \lambda (y_{i,t} - y_i^*)$$

where λ is the classical conditional convergence parameter. Generally, for practical purposes, the literature implicitly assumes that y_i^* can be modeled as a linear function of several variables that impact the structure of the economy (Bekaert, Harvey, and Lundbald, 2005). The government's spending, inflation, average years of secondary schooling, and many other control variables enter the empirical growth studies on this account. The estimated growth model, non-linear and non-monotonic with respect to financial depth, has the following form:

$$\begin{aligned} \Delta y_{i,t+k} &= \lambda y_{i,t} + \beta_1 PC_{i,t} + \beta_2 PC_{i,t}^2 + \gamma \mathbf{x}_{i,t} + \nu_{it+k} \\ \nu_{it+k} &= \mu_i + \lambda_{t+k} + \varepsilon_{i,t+k} \end{aligned} \quad (\text{I.2})$$

where the subscripts i and t refer to cross-section unit and time period. $PC_{i,t}$ is the ratio of private credit over GDP used as a proxy for financial development. $\mathbf{x}_{i,t}$ is the set of control variables. Finally, ν_{it} follows a two-way error component model where μ_i , λ_t and $\varepsilon_{i,t}$ are respectively the country-specific effect, the period-specific effect and

the error term. The inclusion of time dummies allows capturing period-specific effects, proxying for world economic conditions.

The non-linear and non-monotonic estimations are based on a linear term for private credit, augmented with its quadratic counterpart. The method proposed by Sasabuchi (1980) and developed by Lind and Mehlum (2011), henceforth SLM test, is suited to ascertain the location and relevance of the extremum point. It involves determining whether the marginal effect of finance on growth is significantly different from zero and positive at a low level of finance but negative at a high level, within-sample:

$$H_0 : (\beta_1 + 2\beta_2 PC_{\min} \leq 0) \cup (\beta_1 + 2\beta_2 PC_{\max} \geq 0) \text{ i.e monotone or U-shaped}$$

$$H_1 : (\beta_1 + 2\beta_2 PC_{\min} > 0) \cup (\beta_1 + 2\beta_2 PC_{\max} < 0) \text{ i.e inverted U-shaped.}$$

The estimation method relies on dynamic panel System-GMM estimator, introduced by Arellano and Bover (1995) and Blundell and Bond (1998). This GMM inference method has been applied extensively in economic growth and finance literature. It improves upon pure cross-country work in several respects. First, it deals with the dynamic component of the regression specification. It also fully controls for unobserved time- and country-specific effects. Finally, it accounts for some endogeneity in the variables, thereby allowing for a causal interpretation of the results.

3 Preliminary Comments on Cross-country Regressions

3.1 Simple Cross-country Evidence

Before further delving into the panel estimates, this study first focuses on some cross-country evidence. The setup closely follows the econometric methodology of King and Levine (1993) and the early empirical growth literature (see Barro, 1991). Well aware of the various limitations steaming from endogeneity issues, this exercise is only intended as a preliminary reassessment of the threshold estimates. Naturally, panel data comes as serious help to get around many problems cross-sectional regressions fail to address. Therefore, the panel conclusions of the next sections should be viewed as more reliable.

The number of countries in the following cross-country regressions is substantially smaller than the subsequent panel estimates of sections 4 and 5. This sample difference rests in the cross-country regressions structure, consisting of regressing the average annual growth observed over the sample with a combination of average observation and initial values. The latter requires at least one observation at the beginning of the sample, which is not available for most countries. The selection criteria is the availability of the data. To mitigate the issue and maximize the sample size, I follow Arcand, Berkes, and Panizza (2015) and start a decade later, in 1970, instead of 1960. Once turning to panel estimates, this constraint no longer burdens the sample size.⁶

Table I.1 reports various cross-country regressions. Column (1) provides a benchmark based on the old dataset. The point estimate associated with the linear term of private credit is positive, the quadratic term is negative, and both are statistically significant. It indicates that financial depth starts yielding negative returns as credit to the private sector reaches 86% of GDP. The reliability of this turning point, however, rests solely on the SLM test. With a low p -value of 0.02, the threshold is well identified.

Now focusing on the new dataset.⁷ Using additional available countries and extending the coverage up to 2015, the non-linear finding weakens. The threshold for private credit rises to 100% of GDP with a higher p -value of 0.08 for the SLM test. However, 96% of total observations are below this threshold. Only three countries experience a level of financial depth above the 100% of GDP threshold. None of them reach the 180% threshold above which the marginal effect of financial depth would become both negative and statistically significant.

Figure I.2 plots the quadratic fit between financial depth and growth using the new expanded data. It shows that the curvature is due to only three countries above the threshold, namely: the United-States (USA), Japan (JPN), and Switzerland (CHE). The latter has a high private credit ratio due to the credit extended abroad by two multinational banks (UBS and Cr dit Suisse) not financing directly the Swiss economy.

6. See Appendix A section 1 on page 180 for details on the samples.

7. The correlation for the between dimension of private credit with growth is $\rho(PC, GR) = 0.27$, for its square term with growth $\rho(PC^2, GR) = 0.19$ and for both private credit terms $\rho(PC, PC^2) = 0.95$. Except $\rho(PC^2, GR)$, each of them reject the null hypothesis $H_0 : \rho = 0$ at the 10% level for $N = 74$.

Table I.1 – Cross-country OLS Between regressions.

Data Period Specificity	(1) Old 1970-2010 –	(2) New 1970-2015 –	(3) New 1970-2015 w/o 3 obs.	(4) New 1970-2015 strict OLS-BE
Private Credit	5.608*** (1.738)	4.908*** (1.627)	4.240 (2.871)	4.244** (1.701)
(Private Credit) ²	-3.202*** (1.075)	-2.432** (1.048)	-1.751 (2.591)	-1.770* (0.897)
Log(init. GDP/capita)	-0.611*** (0.173)	-0.752*** (0.152)	-0.716*** (0.156)	-0.735*** (0.161)
Log(school)	1.314** (0.501)	1.460*** (0.362)	1.465*** (0.361)	1.370*** (0.370)
Log(inflation)	-0.165 (0.139)	0.003 (0.153)	-0.005 (0.146)	0.022 (0.250)
Log(trade)	-0.017 (0.257)	0.224 (0.267)	0.249 (0.270)	0.195 (0.262)
Log(gov. cons.)	-0.796 (0.519)	-0.865 (0.559)	-1.032* (0.568)	-0.700 (0.543)
Observations	64	74	71	74
R ²	0.41	0.49	0.50	0.44
dGrowth/dPC=0	86%**	100%*	121%	120%
90% Fieller CI	[74%–111%]	[81%–181%]	[70%–∞]	[91%–308%]
SLM (<i>p</i> -value)	0.02	0.08	0.41	0.18

Notes: This table reports the results of a set of cross-country OLS Between regressions in which the dependent variable is the average real GDP per capita growth rate. While the first column provides a benchmark of the typical non-linear result from the old dataset, the subsequent columns report various exercises based on the new data set expanding the period and country coverage. Column (2) presents a reassessment. Column (3) excludes CHE, JPN, and USA. Column (4) incorporates a slight methodological correction. The SLM test provides *p*-value for the relevance of the estimated threshold. Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Column (3) of Table I.1 performs the same regression, this time without these three peculiar observations.⁸ As expected, the linear and quadratic terms for private credit turn insignificant, and the SLM test indicates that the threshold estimate is no longer statistically relevant. The regression in column (3) emphasizes the dependency of the non-linear conclusion over a long period on a few observations driving the results.

8. Both Japan (JPN) and Switzerland (CHE) display high *Dfbeta* statistics (Belsley, Kuh, and Welsch, 1980). However, as the *Dfbeta* statistic works by dropping one observation at a time, the United-States (USA) does not display an outstanding statistic as it is caught between the other two observations. The *Dfbeta* statistic fails to grasp multiple outliers at once.

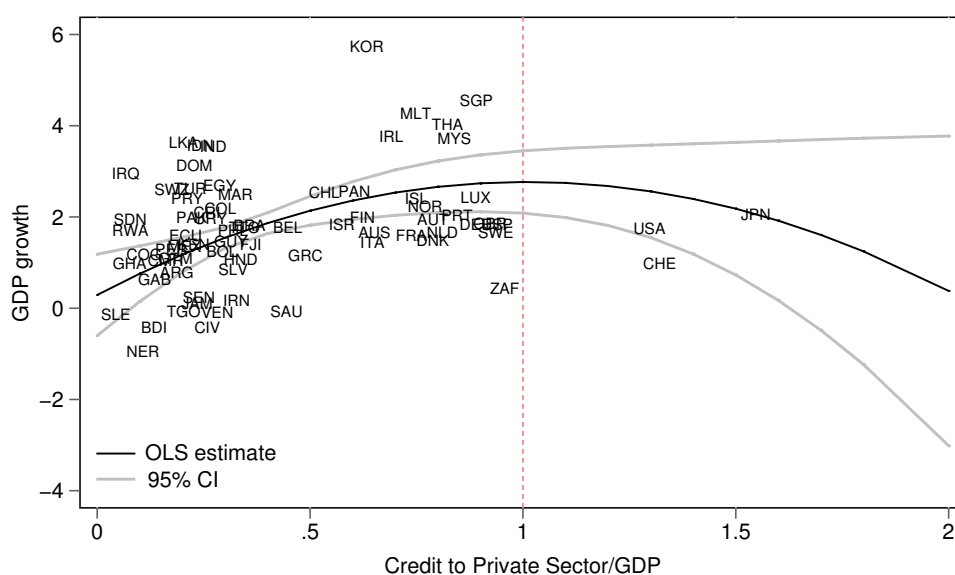


Figure I.2 – Financial depth and growth using the new expanded data for 1970-2015. The solid black line plots the OLS quadratic fit of column (2), Table I.1. The solid light lines are 95% Fieller confidence intervals. The vertical dotted red line marks the threshold estimate at 100%. Point labels are three-letter ISO country codes.

Performing the regression without the quadratic term leads to a positive and statistically significant coefficient for the variable private credit.⁹ In the same spirit, a linear spline regression allowing for different slopes when credit to the private sector is above and below 100% of GDP leads to similar conclusions. Financial depth is positively and significantly associated with economic growth when credit to the private sector is below 100% of GDP, and that it is not significantly correlated above this threshold.¹⁰ Over a long period, the threshold estimation rests solely on three observations.

Finally, these estimates raise a methodological question. Strictly speaking, such cross-sectional regressions, focusing only on the permanent differences in mean levels between countries, corresponding to the “between” dimension, would impose a specific data processing. Columns (1) to (3) follow previous work and handle the data by computing the log and square of the average values of the variables before estimating with OLS. However, for the “between” and “within” dimensions to be orthogonal, one would have

9. The coefficient associated with private credit is 1.42 with a p -value of 0.02.

10. Below 100% of GDP, the coefficient associated with private credit is 2.30 with a p -value of 0.003. Above, the coefficient drops to 0.33 with a p -value of 0.55.

to work with the average of the logs and squares and not the opposite. Column (4) provides estimates with this methodological correction. This “rigorous” cross-country dimension leads to a much higher threshold for private credit at 120% of GDP. Thus, the SLM test now rejects the presence of an inverted U-shape.

These new estimates reduce the confidence one can have in the conclusion that financial depth is detrimental to economic growth when credit to the private sector reaches 100% of GDP. Moreover, the conclusions drawn from cross-country regressions ignore within-country variation, and country-specific characteristics are most likely driving the results.

3.2 Misleading Identification Through Heteroscedasticity

To address the causality issue in these pure cross-sectional country-level regressions, one can use the instrumental variables estimator developed by Rigobon (2003) and Lewbel (2012), which relies on heteroscedasticity-constructed internal instruments (henceforth IH). It allows circumventing the lack of suited external instruments. The downside, as emphasized by Lewbel (2012, p.2), is that “the resulting identification is based on higher moments, and so is likely to provide less reliable estimates than identification based on standard exclusion restrictions.” Moreover, concern regarding potential weak instruments is real and does not boil down to a question of precision but rather of reliability. Precise estimates convey absolutely no information regarding their reliability. Therefore, weak instruments should be tested for. Thus, Table I.2 performs the same regressions as in Table I.1, starting with a benchmark threshold estimate from the old dataset, then with the new dataset up to 2010 and 2015. For each specification, Table I.2 complements the estimates with tests for underidentification and weak instruments.

For the underidentification, Table I.2 reports the p -values for the Kleibergen and Paap (2006) heteroscedasticity robust version of the Lagrange-Multiplier (LM) test. The null hypothesis is that the structural equation is underidentified. A rejection of the null indicates that the smallest canonical correlation between the endogenous variables and the instruments is nonzero. Since the nonzero correlation condition is not enough, Table I.2 also controls for weak-instruments by reporting the weak-instruments Wald statistics based on Cragg and Donald (1993), and its non-*iid* robust analog by Kleibergen

Table I.2 – Misleading cross-country IH regressions.

Data Period	IH-Between			IH-Strict Between	
	(1)	(2)	(3)	(4)	(5)
	Old 1970-2010	New 1970-2010	New 1970-2015	New 1970-2010	New 1970-2015
Private Credit	8.849*** (1.937)	8.883*** (2.577)	9.002*** (2.015)	-0.157 (3.674)	-0.110 (3.191)
(Private Credit) ²	-4.457*** (1.117)	-4.259*** (1.282)	-4.312*** (1.026)	-0.098 (1.497)	-0.048 (1.256)
	<i>Other parameter estimates omitted for clarity</i>				
Observations	64	77	74	77	74
N. instruments	10	10	10	10	10
Kleibergen-Paap LM test (<i>p</i> -value)	0.12	0.05	0.06	0.18	0.13
Cragg-Donald Wald statistic	3.05	2.08	2.33	0.88	0.96
H_0 : <i>t</i> -test size > 10% (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
H_0 : <i>t</i> -test size > 25% (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
H_0 : relative OLS bias > 10% (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
H_0 : relative OLS bias > 30% (<i>p</i> -value)	0.41	0.76	0.67	0.99	0.99
Kleibergen-Paap Wald statistic	5.19	4.28	4.78	1.06	1.11
H_0 : <i>t</i> -test size > 10% (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
H_0 : <i>t</i> -test size > 25% (<i>p</i> -value)	0.81	0.94	0.88	1.00	1.00
H_0 : relative OLS bias > 10% (<i>p</i> -value)	0.91	0.98	0.95	1.00	1.00
H_0 : relative OLS bias > 30% (<i>p</i> -value)	0.03	0.11	0.06	0.98	0.98
Hansen test (<i>p</i> -value)	0.46	0.28	0.46	0.35	0.18
dGrowth/dPC=0	99%***	104%***	104%***	-80%	-115%
90% Fieller CI	[88%–117%]	[92%–121%]	[93%–120%]	–	–
SLM (<i>p</i> -value)	<0.01	<0.01	<0.01	–	–

Notes: This table reports the results of a set of cross-country instrumental variables regressions in which the dependent variable is the average real GDP per capita growth rate. The identification strategy rests on the estimator developed by Rigobon (2003) and Lewbel (2012), and relies on heteroscedasticity-constructed internal instruments: Identification Through Heteroscedasticity (IH). The following variables are included in the regressions but omitted in the table here for clarity: the logarithm of initial Gross Domestic Product per capita, average years of education, a measure of trade openness, the log of the inflation rate, and the log of government consumption normalized by GDP. While the first column provides a benchmark of the typical non-linear result from the old dataset, the subsequent columns report estimates based on the new dataset expanding the period and country coverage. Column (2) is based on the new dataset with the same time coverage as column (1) but with additional countries. Column (3) expands the coverage up to 2015. Columns (4-5) incorporate a slight methodological correction. The SLM test provides *p*-value for the relevance of the estimated threshold. Robust Windmeijer standard errors in parentheses. Table A.12 on page 201 displays a full version of this table with all coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

and Paap (2006). The latter is better suited due to heteroscedasticity-robust standard errors. These tests assess whether the instruments jointly explain enough variation to identify unbiased causal effects.

The additional diagnostics proposed by Stock and Yogo (2002) and Yogo (2004) complement these tests: p -values for the null hypotheses that the bias in the estimates on the endogenous variable is greater than 10% or 30% of the OLS bias, and p -values for the null hypotheses that the actual size of the t -test that the coefficient estimates equal zero at the 5% significance level is greater than 15% or 25%.¹¹ Finally, the table reports the Hansen test of overidentifying restrictions, robust to heteroscedasticity.

Columns (1) to (3) of Table I.2 show that the coefficients associated with private credit are precisely estimated, roughly constant for the various regressions, and yield a threshold around 100% of GDP. However, the various specification tests severely reduce the confidence one should have in these results. In column (1), the Kleibergen-Paap LM test of underidentification fails to reject the null hypothesis that the structural equation is underidentified. For all regressions, the Cragg-Donald and Kleibergen-Paap Wald-type statistics show that the instrumentation is very weak. Moreover, the high p -values for the various levels of relative OLS bias underlines that the instrumentation is far too weak to remove a substantial portion of OLS bias. Large p -values also indicate that the actual size of the t -test at the 5% level is greater than 25%. The precise estimates are a byproduct of either weak or irrelevant instruments.

Columns (4) and (5) deal with the methodological issue mentioned in the previous subsection 3.1. They provide estimates with the methodological correction, based on the exact specification of previous columns (2) and (3). This rigorous cross-country dimension leads to insignificant point estimates for the level of private credit and its squared term, along with a negative threshold for private credit. Thus, the SLM test now trivially rejects the presence of an inverted U-shape. The threshold estimates are highly sensitive to the specific data process.

11. Critical values for the Kleibergen-Paap Wald statistic have not been tabulated, as it depends on the specifics of the *iid* assumption's violation. Therefore, following others in the literature (see for more details Baum, Schaffer, and Stillman, 2007; Bazzi and Clemens, 2013), the critical values tabulated for the Cragg-Donald statistic are applied to the Kleibergen-Paap statistic.

The IH estimations suffer from weak instrumentation. Thus, not surprisingly, the point estimates from Table I.2 are in line with those obtained from the OLS estimator in Table I.1. This proximity does not point toward highly causal results. It would rather be a sign of untreated bias and persistent endogeneity as the instruments are far too weak to address these issues. By looking under the hood of the identification through heteroscedasticity, these simple tests shine brighter lights on its inability to yield a reliable identification of a causal impact from finance depth to economic growth.

Overall, plain vanilla cross-country regressions suffer from various biases “as serious as they are well-known” (Popov, 2018, p.10). First of all, it is almost impossible to account for all plausible growth determinants, raising issues about the omitted variable bias.¹² Parameter heterogeneity is a non-negligible concern. Cross-country regressions pool together countries that differ vastly in their degree of financial and economic development leaving group differences unaccounted.¹³ The misspecification bias is also a notable limitation as the quadratic form is prone to overfitting outliers. Ultimately, cross-country regressions are exposed to simultaneity and endogeneity bias, which could drive the non-monotonic conclusion. For instance, the development level might likely be the driving force of both the average growth and private credit level – recall the famous words of Robinson (1952): “where enterprise leads, finance follows.” The cross-country setup is unsuited to address these limitations.

Determining where each bias precisely stands is unfortunately out of reach. Nevertheless, panel data comes as serious help to get around many of the problems cross-sectional regressions fail to address. Therefore, the panel conclusions are usually considered as more reliable. Indeed, switching from pure cross-country to panel data mobilizing the time-series dimension has significant advantages. Among them, estimates are no longer biased by omitted variables constant over time –the so-called fixed effects. Also, taking advantage of internal instrument techniques allows for consistent estimates of the endogenous models, if carefully and adequately cast.

12. Durlauf, Johnson, and Temple (2005) have listed no less than 145 distinct variables in the growth regressions in the literature.

13. For more consideration on *overcontrolling*, see Appendix A section 3.

4 More Reliable Panel Estimates?

4.1 A Very Influential Starting Point

Now turning to a pooled (cross-country and time-series) data set consisting of at most 140 countries and, for each of them, at most 11 non-overlapping five-year periods over 1960-2015.¹⁴

The five-year spell length is commonly chosen in the literature for several reasons. First, the use of longer periods would significantly reduce the number of degrees of freedom, which is problematic when implementing dynamic panel data procedures. Secondly, five-year periods, as emphasized by Calderon, Chong, and Loayza (2002), follows the endogenous growth literature (e.g. Caselli, Esquivel, and Lefort, 1996; Easterly, Loayza, and Montiel, 1997; Benhabib and Spiegel, 2000; Forbes, 2000) where such period length is believed to purge out business-cycle fluctuations which could induce a negative coefficient on private credit. Indeed, the empirical growth literature usually averages out data over five-year spells in order to measure the steady-state relationship between the variables. Smoothing out data series supposedly removes useless variation from the data, enabling precise parameter estimates. Indeed, Loayza and Ranci ere (2006) find that short-run surges in private credit appear to be a good predictor of both banking crises and slow growth. In the long run, a higher level of private credit is associated with higher economic growth. This tension between short-term and long-term effects justifies the use of low-frequency data to abstract from business-cycles. Finally, this is conveniently suited to the specifics of System-GMM, as it requires a short panel characterized by large N and small T dimensions.

The growth variable is usually computed as the average annual growth rate within the five-year spell. All explanatory variables, however, are systematically based on the first observation of each five-year spell. The absence of averaging implies a substantial informational loss as well as a consistency loss. Excluding 80% of the data would possibly expose the coefficient estimates to bias as it could mismeasure the true explanatory variables. Hence, is the starting point of the five-year spells influencing the results?

14. See Appendix A section 1 on page 180 for details on the samples.

Table I.3 – Sequential anchoring of the five-year spells in dynamic panel regressions (1/2).

	(1)	(2)	(3)	(4)	(5)
Data	Old	Old	Old	Old	Old
Coverage	1960-2010	1961-2011	1962-2007	1963-2008	1964-2009
Number of spells	10	10	9	9	9
Private Credit	3.621**	0.171	0.780	0.084	1.971
	(1.718)	(1.824)	(1.877)	(1.689)	(1.688)
(Private Credit) ²	-2.018***	-0.882	-0.749	-0.523	-1.418*
	(0.727)	(0.774)	(0.889)	(0.782)	(0.852)
	<i>Other parameter estimates omitted for clarity</i>				
N. instruments	318	318	254	254	254
N. countries	133	133	134	133	133
Observations	917	916	811	829	858
AR(2) (<i>p</i> -value)	0.11	0.08	0.23	0.51	0.91
Hansen test (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
dGrowth/dPC=0	90%**	10%	52%	8%	69%
90% Fieller CI	[43%–113%]	–	–	–	[0%–124%]
SLM (<i>p</i> -value)	0.03	0.46	0.40	0.48	0.19

Notes: This table reports the results of a set of panel regressions consisting of non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. The first column reports the best-attempted replication of the typical threshold result from the yearly version of the old dataset. Column (2) provides point estimates with a one-year forward shift for the starting point of each spell. The subsequent columns continue shifting forward by one year the beginning of the five-year spells. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). Robust Windmeijer standard errors in parentheses. Table A.13 on page 205 displays a full version of this table with all coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table I.3 shows regressions for sequential anchoring of the five-year spells based on the old dataset. Column (1) provides a benchmark (typical) non-linear conclusion. Column (2) provides point estimates with a one-year forward shift for the starting point of each spell, with an identical sample of countries, and one fewer observation (916 against 917 previously) due to data availability. The coefficients associated with the linear and quadratic term of private credit lose magnitude, and neither of them is statistically significant. The SLM test discards the inverted U-shape with a high p -value of 0.46. Through columns (3) to (5), the same exercise goes on by shifting forward by one year the beginning of the five-year spells. In the end, out of the five possible starting points presented in columns (1-5), only one supports the existence of a threshold.

Table I.4 – Sequential anchoring of the five-year spells in dynamic panel regressions (2/2).

	(1)	(2)	(3)	(4)	(5)
Data	New	New	New	New	New
Coverage	1960-2015	1961-2016	1962-2012	1963-2013	1964-2015
Number of spells	11	11	10	10	10
Private Credit	-0.170 (1.480)	2.851* (1.465)	1.085 (1.408)	0.940 (1.610)	1.027 (1.800)
(Private Credit) ²	-0.256 (0.703)	-1.217* (0.699)	-1.062* (0.612)	-0.901 (0.679)	-0.698 (0.721)
<i>Other parameter estimates omitted for clarity</i>					
N. instruments	388	388	318	318	318
N. countries	140	140	138	138	138
Observations	1,055	1,085	965	970	987
AR(2) (<i>p</i> -value)	0.40	0.01	0.11	0.56	0.85
Hansen test (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
dGrowth/dPC=0	–	117%*	51%	52%	73%
90% Fieller CI	–	[86%–426%]	[0%–95%]	–	–
SLM (<i>p</i> -value)	–	0.06	0.34	0.37	0.33

Notes: This table reports the results of a set of panel regressions consisting of non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. Each column presents one possible anchoring for the five-year spells in the new dataset. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). Robust Windmeijer standard errors in parentheses. Table A.14 on page 206 displays a full version of this table with all coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table I.4 conducts the same exercise, this time based on the new dataset. The number of countries in the new dataset varies slightly from the old one.¹⁵ The selection criteria is the availability of the data. Each column shifts forward by one year the beginning of the five-year spells. Very similar conclusions arise, as only one estimate out of the five possible anchors supports the presence of a significant threshold. Analogous conclusions are drawn from a restricted sample of the new dataset to match the country coverage of Table I.3.

From these various anchoring exercises, a clear recommendation emerges. Averaging the explanatory variables within the five-year spell should be favored over initial values, except for the convergence variable.¹⁶ It is preferable to keep more observa-

15. See Appendix A section 1 on page 180 for details on the samples.

16. The work of Caselli, Esquivel, and Lefort (1996) is among the first attempts to use the GMM framework to estimate a Solow growth model. They make use of the Barro (1991) method, initially

tions through data averaged over sub-periods, while controlling for endogeneity biases by properly instrumenting the explanatory variables.¹⁷ Otherwise, coefficient estimates remain exposed to mismeasured true explanatory variables.

4.2 Abundant Weak Instruments

4.2.1 An Instruments Proliferation Issue

The dynamic panel System-GMM estimator introduced by Arellano and Bover (1995) and Blundell and Bond (1998) comes in handy to work toward a causal reading of the estimates as no suited external instruments have emerged. However, the default implementation of this estimator generates a set of internal instruments whose number increases particularly quickly with the time dimension of the panel. The dramatic increase (somehow pandemic) in the instrument count is often referred to as *instruments proliferation*. The literature has documented several problems arising with excessive proliferation: overfitting of endogenous variable, weakened Hansen test for over-identifying restrictions, biased two-step variance estimators and imprecise estimates of the optimal weighting matrix.¹⁸ Fortunately, there are two usual telltale signs: a number of instruments greater than the number of cross-sectional units (the number of countries), and a perfect Hansen test p -value of 1.00. The non-linear conclusions systematically meet both telltale signs.

Column (1) of Table I.5 presents the typical non-linear conclusion based on the old dataset. The coefficient estimates on private credit are significant, and the SLM test corroborates the presence of an inverted U-shape relationship. It indicates that financial

created for cross-sectional data, by adapting it for a panel framework. Already at this early stage, the problem related to extensive use of initial value was raised. They chose to work with the averaged annual growth rate of per capita GDP, but distinguished between state and control variables for the explanatory variables. Controls are averaged over the five-year intervals (government consumption, inflation rate, trade openness). In contrast, only states variables are taken at their initial value (initial level of per capita GDP, the average number of years of schooling). Therefore all variables do not enter with the same treatment.

17. Some papers have taken this path, see for example Benhabib and Spiegel (2000), Beck and Levine (2004), Rioja and Valev (2004), Beck, Degryse, and Kneer (2014), and Law and Singh (2014).

18. For more details, see Andersen and Sorensen (1996), Ziliak (1997), Alonso-Borrego and Arellano (1999), Koenker and Machado (1999), Hayashi (2000), Calderon, Chong, and Loayza (2002), Bowsher (2002), Alvarez and Arellano (2003), Han and Phillips (2006), Hayakawa (2007), Roodman (2009a), and Baltagi (2013).

Table I.5 – Instrument proliferation in System-GMM panel regressions for 1960-2010.

	(1)	(2)	(3)	(4)	(5)
Instrument matrix:	GMM-type	GMM-type	COLLAPSED	GMM-type	COLLAPSED
Number of lags	ALL	1	ALL	ALL (PCA)	ALL (PCA)
Number of instruments	318	122	73	51	19
Private Credit	3.628** (1.726)	2.694 (2.025)	0.689 (2.972)	-3.267 (2.107)	-15.834 (10.411)
(Private Credit) ²	-2.021*** (1.726)	-1.970** (0.952)	-0.882 (1.390)	0.924 (0.964)	4.660 (3.357)
Log(init. GDP/cap.)	-0.728** (0.310)	-0.317 (0.305)	-0.957* (0.525)	-0.853 (0.541)	1.591 (1.917)
Log(school)	2.270*** (0.615)	2.016*** (0.745)	3.738*** (1.040)	5.568*** (1.438)	4.872** (1.910)
Log(inflation)	-0.273 (0.210)	-0.393** (0.198)	-0.875** (0.377)	-1.024*** (0.394)	-1.819*** (0.561)
Log(trade)	1.087** (0.511)	1.291* (0.759)	3.532** (1.437)	1.235 (0.876)	3.370 (2.977)
Log(gov. cons.)	-1.461** (0.742)	-2.474*** (0.594)	-1.452 (1.227)	-2.242** (0.995)	-1.652 (7.501)
Number of countries	133	133	133	133	133
Observations	917	917	917	917	917
AR(2) (<i>p</i> -value)	0.11	0.10	0.02	0.04	0.02
Hansen test (<i>p</i> -value)	1.00	0.51	0.19	0.07	<0.01
dGrowth/dPC=0	90%**	68%	39%	–	–
90% Fieller CI	[42%–113%]	[–∞–93%]	–	–	–
SLM (<i>p</i> -value)	0.03	0.16	0.47	0.34	0.14
PCA <i>R</i> ²	–	–	–	0.86	0.83

Notes: This table reports the results of a set of panel regressions consisting of ten non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. While the first column reports a replication of the typical threshold result from the old dataset for 1960-2010, the subsequent columns report various instrument reduction exercises. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). The SLM test provides *p*-value for the relevance of the estimated threshold. Windmeijer standard errors in parentheses. Table A.15 to A.18 in the appendix display extended versions of this table with various sub-periods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

depth starts yielding negative returns as credit to the private sector reaches 90% of GDP. However, there are no less than 318 instruments in this default implementation of the System-GMM estimator (for only 130 cross-sectional units). Along with the perfect value of 1.00 for the Hansen test, this casts doubts on the reliability of the result, with possible overfitting and failure to expunge the endogenous part as the tests would be weakened in this setup. Moreover, the AR(2) test for autocorrelation display a p -value of 0.11, which is too low to be considered safe. These tests are conservative, a value close to conventional thresholds should be viewed with a fair degree of caution.

Roodman (2009a, p. 156) stresses that “results and specification tests should be aggressively tested for sensitivity to reduction in the number of instruments.” The remaining columns of Table I.5 present the various instrument count reductions implemented as minimally arbitrary robustness checks to examine the behavior of the coefficient estimates and various specification tests.

Column (2) provides the first step of the robustness check strategy to reduce the number of instruments. Alonso-Borrego and Arellano (1999) states that the most distant instruments are generally those which offer the weakest correlation and are therefore the least relevant. Following others in the finance-growth literature, column (2) restricts the instrument matrix to a single lag (see for examples Levine, Loayza, and Beck, 2000; Beck, Levine, and Loayza, 2000; Baltagi, Demetriades, and Law, 2009; Kose, Prasad, and Taylor, 2009; Law and Singh, 2014). This brings the instruments count down to 122 instruments, below the usual rule of thumbs based on the number of cross-country observations. This time, the coefficient estimate for private credit in level loses significance, and the SLM test becomes inconclusive, rejecting the presence of an inverted U-shape. The usual specification tests now systematically reject at lower p -values, displaying a serious sign of second-order autocorrelation. The Hansen test now returns a lower p -value of 0.51, much lower than the initial 1.00.

Collapsing the instrument matrix further reduces the instrument count. Whereas limiting the lag depth still relies on different sets of instruments for each time period, the collapsing works around with moment conditions applied such that each of them corresponds to all available periods (Calderon, Chong, and Loayza, 2002). It maintains

the same amount of information from the original 318 columns instrument matrix, yet combined into a smaller set.¹⁹ The number of instruments now falls to 73. Column (3) displays results for this exercise. Both coefficient estimates for private credit in level and squared are no longer significant. Once again, the SLM test rejects the presence of an inverted U-shape between finance and growth. Moreover, the p -value of the AR(2) test now dips down to 0.02, confirming the previously suspected autocorrelation issue. The Hansen test's p -value falls to 0.18, as compared to the 1.00 for the default implementation.

The penultimate technique to reduce the instrument count without either cutting into the lag depth or the GMM-style construction of the instrument matrix is to replace the prolific instruments by their principal components (Kapetanios and Marcellino, 2010; Bai and Ng, 2010; Bontempi and Mammi, 2012). Column (4) presents results for this principal components analysis (PCA) technique, which enables to maintain a substantial amount of the information in the instruments into less extensive moment conditions. The identification now rests on 51 instruments. The coefficient estimates for private credit are insignificant, and of opposite sign as compared to the default implementation of column (1). Once again, the SLM test confirms the absence of an inverted U-shape. Other coefficients remain roughly in line with the default implementation, with slightly higher absolute values. Both the AR(2) test and Hansen test return very low p -values of 0.04 and 0.07 respectively, discarding the reliability of the results.

Finally, the last column combines PCA and collapse techniques, as Mehrhoff (2009) concludes that PCA performs reasonably well when the instrument matrix is collapsed prior to factorization (see for example Beck, Georgiadis, and Straub, 2014). Column (5) displays this ultimate reduction to 19 instruments. The point estimate and standard errors for private credit are more than four times higher in absolute value than in the baseline regression from column (1). Just as in column (4), private credit and its square term switch signs. The SLM test discards once again the presence of an inverted U-shape. The main specification tests now display extremely low p -values, discarding the

19. For an application to the finance-growth setup, see the work of Beck and Levine (2004) or Carkovic and Levine (2005).

adequacy of the model: 0.02 and 0.00 for the AR(2) and the Hansen test, respectively.

Overall, there is a substantial and systematic decrease in the p -values of both the Hansen test and the AR(2) test as the number of instruments falls. Given the overall dependence of the non-linear conclusion on a very high instrument count, these straightforward techniques highlight a strong possibility of overfitting and concerns of third-variable or reversed causation. The general dependence of the results on a specific instrument matrix also gives hints toward a weak instrument problem.

4.2.2 Far Too Weak Instruments

A reliable causal inference of financial depth on growth requires the instruments to display a strong relationship with the endogenous explanatory variables. When this relationship is only weak, instrumental variable estimators are severely biased (see for a survey Murray, 2006; Mikusheva, 2013). The System-GMM estimator is far from immune to the weak instruments' problem (Hayakawa, 2009; Bun and Windmeijer, 2010).

Measuring how much of the variation in the endogenous variables is explained by the internal instruments is crucial, and often remains unexplored. Most applications of the System-GMM assume that instruments are strong. The issue goes far beyond the finance-growth literature. Indeed, testing for weak instruments is not straightforward in dynamic panel GMM regressions due to the absence of standardized tests.

To circumvent this issue, Bazzi and Clemens (2013) have come up with a simple "2SLS analog" technique. Since weak instrument tests are available within the 2SLS setup, carrying out the equivalent regression using 2SLS with the identical GMM-type instrument matrix provides "simple and transparent tests of instrument strength in a closely related setting" (Bazzi and Clemens, 2013, p. 167). This exercise requires to split the System-GMM in two: the difference part and the level part.

Table I.6 reports point estimates for this exercise along with various specification tests for the typical threshold from the old dataset. Once again, the table displays tests for underidentification (Kleibergen-Paap LM test) and weak instruments (Cragg-Donald

Table I.6 – Weak instruments in dynamic panel regressions.

Estimator	GMM-SYS	Difference equation		Levels equation	
		No	Yes	No	Yes
Collapsed IV matrix	(1)	(2)	(3)	(4)	(5)
Private Credit	3.628** (1.726)	-5.110** (2.161)	1.380 (4.020)	4.247** (2.028)	16.220 (121.16)
(Private Credit) ²	-2.021*** (1.726)	0.536 (0.825)	-2.278 (1.896)	-2.765*** (0.996)	-11.390 (81.01)
<i>Other parameter estimates omitted for clarity</i>					
Observations	917	780	780	917	917
N. countries	133	130	130	133	133
N. instruments	318	261	57	65	16
IV: Lagged levels	Yes	Yes	Yes	No	No
IV: Lagged differences	Yes	No	No	Yes	Yes
Hansen test (<i>p</i> -value)		0.67	0.12	0.01	–
Kleibergen-Paap LM test (<i>p</i> -value)		0.286	0.465	0.518	0.894
Cragg-Donald Wald statistic		0.89	0.68	0.83	0.002
<i>H</i> ₀ : <i>t</i> -test size > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
<i>H</i> ₀ : <i>t</i> -test size > 25% (<i>p</i> -value)		1.000	1.000	1.000	1.000
<i>H</i> ₀ : relative OLS bias > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
<i>H</i> ₀ : relative OLS bias > 30% (<i>p</i> -value)		1.000	1.000	1.000	0.999
Kleibergen-Paap Wald statistic		3.17	0.85	1.15	0.002
<i>H</i> ₀ : <i>t</i> -test size > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
<i>H</i> ₀ : <i>t</i> -test size > 25% (<i>p</i> -value)		1.000	1.000	1.000	1.000
<i>H</i> ₀ : relative OLS bias > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
<i>H</i> ₀ : relative OLS bias > 30% (<i>p</i> -value)		0.614	1.000	1.000	0.999

Notes: This table reports the results of a set of minimally arbitrary weak instrument test opening the “black box” of the System-GMM estimator. The panel regressions are based on ten non-overlapping five-year spells and contain time fixed effects. The dependent variable is the average real GDP per capita growth rate. While the first column simply reproduce the baseline result from the old dataset for 1960-2010 (see Table I.5, column (1)), the subsequent columns report the decomposition of the System-GMM following the “2SLS analogs” of Bazzi and Clemens (2013). Windmeijer standard errors in parentheses. Table A.20 on page 239 displays a full version of this table with all coefficients. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

and Kleibergen-Paap Wald tests).²⁰

Column (1) provides the benchmark System-GMM estimates. Column (2) presents 2SLS regressions of difference growth on differenced regressors, instrumented by lagged levels, analogous to the difference part of the System-GMM estimator. Similarly, column (3) reproduces the exercise, this time with a collapsed instrument matrix. To complete the picture, columns (4) and (5) present a parallel exercise examining the level part of the System-GMM estimator, in the same manner as the difference part. The level of growth is regressed on the level of explanatory variables, instrumented by lagged differences identical to the levels part of the System-GMM estimator.

Each time, both the LM test of underidentification and the Wald-type statistics show that instrumentation is far too weak to remove a substantial portion of OLS bias. Large p -values also indicates that the actual size of the t -test at the 5% level is greater than 25%. These extremely high p -values, denoting a failure to reject the null of weak instruments, are not indicative of under-powered or biased tests as would a p -value of 1.00 for the Hansen test with instrument proliferation. The precise estimates are a byproduct of either weak or irrelevant instruments.

These simple 2SLS analogs open the “black box” surrounding the estimation strategy. They demonstrate the pervasiveness of abundant weak instruments in the System-GMM setup underlying the non-linear conclusion, thereby casting severe doubts on its ability to yield any identification of a causal impact.

4.3 Near-Multicollinearity and Outliers’ Driven Threshold

Where is this inverted U-shape emerging from? Assessing the underlying mechanism driving the thresholds estimates requires to focus on a near-multicollinearity issue.

First, consider a classical suppressor, which refers to a regressor whose simple correlation coefficient with the dependent variable is below 0.1 in absolute value.²¹ The presence of a classical suppressor induces a parameter identification issue. As previously emphasized in column (4) of Table I.6, the level part of the System-GMM estimates

20. For further details on these tests, see the previous subsection 3.2 on page 49.

21. The 0.1 threshold for simple correlation implies that the explanatory variable would account for 1% of the variance of the dependent variable in a simple regression (Chatelain and Ralf, 2014).

almost exclusively contributes to the identification of the non-linear conclusion. Moreover, the explanatory variable *Private Credit* is a classical suppressor in the level part of the System-GMM estimate. It displays a coefficient of correlation with growth of $\rho(PC, GR) = 0.007$, far below the 0.1 threshold.²²

Chatelain and Ralf (2014) have documented that including an additional classical suppressor, highly correlated with the first one, may lead to very large and statistically significant point estimates. Unfortunately, these results are spurious and outliers driven.

The typical additional classical suppressor in dynamic panel setup is the square term of the first one. The thresholds estimates fit the scenario of a highly correlated pair of classical suppressors. The *Private Credit* variable and its square counterpart are highly correlated with one another, $\rho(PC, PC^2) = 0.93$. And they both display a near-zero correlation with the dependent variable, $\rho(PC^2, GR) = -0.03$. Chatelain and Ralf (2014, p. 91) emphasize that “the spurious effect can be identified because its statistical significance is not robust to outliers.”

Figure I.3 plots the quadratic fit between financial depth and growth in levels from the first column of Table I.5. As only the level part of the System-GMM estimator is exposed to the near-multicollinearity issue, and since it bears the weight of deriving the non-linear result, the scatter plot focuses on levels rather than on first-differences. Figure I.3 provides visual support for the presence of several outliers. The most obvious ones are Liberia-1986 (LBR6), Saudi Arabia-1981 (SAU5), and Iceland-2006 (ISL10). The latter represents the tremendous expansion of three major Icelandic banks (Kaupthing, Landsbanki, and Glitnir) driven by the provision of credit in international financial markets. These banks defaulted in the wake of the 2007/8 financial crisis, which explains the negative average growth over the subsequent five years.

Furthermore, based on outstanding normalized residual squared, leverage, and *Dfbeta*, there are three additional outliers: Gabon-1971 (GAB3), China-1991 (CHN7) and China-

22. Which do not reject the null hypothesis $H_0 : \rho(PC, GR) = 0$ at the 10% level for $N = 917$ observations. The coefficient of correlation of private credit with growth for the first difference part of the System-GMM is $\rho(\Delta PC, \Delta GR) = -0.22$, for the square of private credit with growth $\rho(\Delta PC^2, \Delta GR) = -0.16$ and for both private credit terms $\rho(\Delta PC, \Delta PC^2) = 0.86$. Each of them rejects the null hypothesis $H_0 : \rho = 0$ at the 10% level for $N = 799$ observations. *Private Credit* is a classical suppressor only in the level part of the System-GMM.

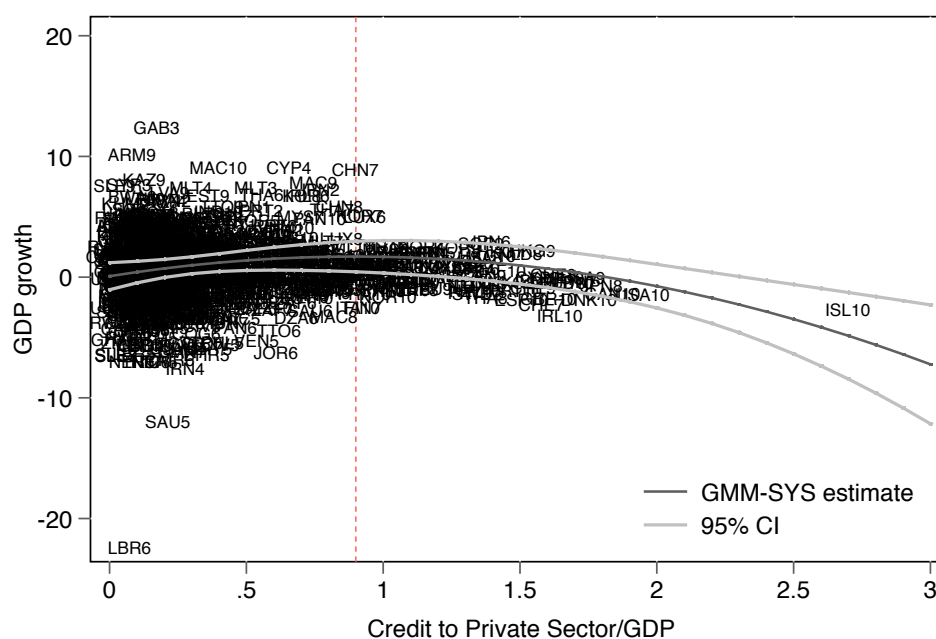


Figure I.3 – Financial depth and growth for 1960-2010 in the old dataset. The solid black line plots the System-GMM estimate of Table I.5, column (1). The solid light lines are 95% Fieller confidence intervals. The vertical dotted red line marks the threshold estimated at a ratio of private credit over GDP of 90%. Point labels are three-letter ISO country codes followed by a time period digit (2 = 1965-1969, 3 = 1970-1974, etc.).

1996 (CHN8). The latter two are the sole China observations in the sample. Their position over the top of the bell-shaped curve induces high leverage on the curvature.

In Table I.7, columns (1) and (2) provide outlier-free estimates of the baseline non-linear result (still suffering from weak instrument proliferation). Whether three or six outliers are dropped, each time, *Private Credit* is no longer statistically significant and loses in magnitude. The SLM test discards the relevance of a threshold. Note that in Tables I.3 and I.4, out of the five possible starting points presented through columns (1-5), only one supports the non-linear conclusion. The other four anchors do not include these outliers, which are specific to the chosen starting point. This evidence emphasizes the general dependence of the results on a set of outliers.

The near-multicollinearity creates instability on the parameters and increases the weight of the outliers. The two issues are enhanced by the overfitting due to weak instrument proliferation (see section 4.2.2), which generates misleading estimates.

Instead of overcoming the endogeneity bias of cross-country regressions with mislead-

Table I.7 – Near-multicollinearity, outliers and preferred dynamic panel regressions.

	GMM-SYS		OLS-FE		
	(1)	(2)	(3)	(4)	(5)
Data	Old	Old	Old	Old	New
Specificity	w/o 3 outliers	w/o 6 outliers	–	w/o 6 outliers	–
Period	1960-2010	1960-2010	1960-2010	1960-2010	1960-2010
Private Credit	2.533	2.350	-0.531	-0.506	-0.455
	(1.929)	(1.688)	(1.033)	(1.017)	(1.001)
(Private Credit) ²	-1.784*	-1.623*	-0.660	-0.863*	-0.621
	(0.937)	(0.826)	(0.469)	(0.462)	(0.517)
	<i>Other parameter estimates omitted for clarity</i>				
N. instruments	318	318	–	–	–
N. countries	133	132	133	132	138
Observations	914	911	917	911	956
AR(2) (<i>p</i> -value)	0.16	0.04	–	–	–
Hansen test (<i>p</i> -value)	1.00	1.00	–	–	–
dGrowth/dPC=0	71%	72%	–	–	–
90% Fieller CI	[0%–101%]	[0%–109%]	–	–	–
SLM <i>p</i> -value	0.15	0.14	–	–	–

Notes: This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. While the first column presents the baseline result from Table I.5, column (1), dropping ISL10, LBR6, SAU5. Column (2) further drops GAB3, CHN7, and CHN8 from the sample. The subsequent columns report various OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). The SLM test provides *p*-value for the relevance of the estimated threshold. The absence of *p*-value for columns (3) to (5) is due to a trivial rejection of the inverted U-shape. Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

ing System-GMM estimates, column (3) to (5) favor OLS fixed effect estimates. They are more reliable, in this setup, for several reasons. First, they adequately deal with the endogeneity stemming from time-invariant country's specifics. In the System-GMM setup, only the difference equation controls for country fixed effect. The level equation, bearing most of the identification, does not control for such invariant country's characteristics. Second, the absence of instrument proliferation reduces the overfitting issue, thereby limiting the point estimate's sensitivity to outliers. Finally, as the GMM instruments are weak, the remaining endogeneity bias indeed remains unaddressed.

Column (3) shows OLS fixed effects estimates of the same model as the baseline results in column (1). Column (4) displays the OLS fixed effect estimates similar to column

(2). Finally, column (5) presents the same regressions using this time the new dataset. Each time, the various point estimates for *Private Credit* and its square counterpart loose magnitude and are no longer statistically significant. Due to their signs, the SLM test trivially discards the relevance of a threshold. The near-multicollinearity of the financial proxies, combined with the weak instrument proliferation issue, fosters spurious regressions overfitting outliers.

5 The Damaging Effect of Financial Deepening

This final section investigates the overall average effect of finance on growth and some non-linearities as a reassessment of the nexus. The *Great Recession* has spotlighted already flourishing literature documenting the adverse effects of financial depth. There are both pros and cons to financial development. The early work of Amable and Chatelain (1995) is among the first to document the ambivalence of financial deepening. Overall, the lack of a consensus is a defining characteristic of the finance-growth literature. One way to consider this issue and bridge these divergences is to consider that both strands document mutually compatible effects. On the one hand, financial development can funnel the growth process by enabling investments in physical and human capital (Galor and Zeira, 1993; Galor and Moav, 2004; Aghion, Howitt, and Mayer-Foulkes, 2005). On the other hand, financial development can drain talents from the real sector (Tobin, 1984; Philippon and Reshef, 2013), stimulate unproductive allocation of resources, feed speculative bubble (e.g. mortgage credit, Beck et al. (2012), encourage risk-taking and exacerbate fragility (Minsky, 1974) fostering “catastrophic meltdown” (Rajan, 2005).

These opposing strands of the literature give support to investigating the overall net effect. The average effect provides an answer to the following questions: what is the prevailing outcome of financial depth overtime? Has financial development done “more harm than good”? What is the overall net effect of financial development on growth? This section intends to contribute to the literature in this regard. In light of previous evidence, is it possible to draw any conclusion regarding the finance-growth relationship based on such large macro-finance panels?

As mentioned in the first Chapter, the level part of the System-GMM estimates misleadingly bears the load of the inverted-U curve. The level part does not account for cross-country heterogeneity. Therefore, System-GMM estimates can be driven by unaccounted permanent differences between countries that have little to do with financial development. Moreover, the explanatory variable *Private Credit* is a classical suppressor in the level part, thereby producing a spurious effect. This first Chapter draws a clear cut recommendation to favor an estimation strategy that fully accounts for cross-country heterogeneity. Otherwise, the results might be uninformative concerning the growth-enhancing or damaging properties of financial development.

The Chapter aims to determine whether any conclusion regarding the finance-growth relationship can be drawn from large macro-finance panels. The Difference-GMM estimator is a suited—albeit not ideal—candidate to address the previous limitations. It controls both the country- and period-specific effects while providing a setup related closely to the System-GMM. Well aware of the limitations arising from the various estimates, this section offers grounds for future research.²³ It follows the idea that economists should not refrain from “pursuing pressing research questions until the perfect methods arrive” (Bazzi and Clemens, 2013, p.31).

5.1 An Overall Adverse Effect

Table I.8 provides Difference-GMM estimates throughout columns (1-4). Careful attention is paid to limit the instrument count in order to avoid problems stemming from excessive instrument proliferation. To this end, the estimations rely on only one lag of instruments, allowing a manageable number of instruments, mitigating concerns that excessive instrument proliferation could compromise the reliability of the estimation results.²⁴ Whether it is the new or the old dataset, the estimations display an overall negative association between finance and growth.

Focusing on post-1980s observations, in column (2) and (4), magnifies the coefficient

23. Chapter 2 provides a change of scenery, further addressing the present estimation method limitations by relying on other techniques.

24. The estimates from Table I.8 are robust to collapsing the instrument set. See Table A.22 on page 243 in the Appendix.

Table I.8 – Alternative estimates: the damaging impact of financial deepening.

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Old 1960-2010	Old 1980-2010	New 1960-2015	New 1980-2015	Old 1960-2010	Old 1980-2010	New 1980-2015	New 1990-2015
Private Credit	-4.904*** (1.607)	-6.890*** (2.247)	-6.502*** (1.153)	-8.540*** (2.035)	-1.795*** (0.442)	-2.474*** (0.581)	-1.485*** (0.470)	-2.051*** (0.506)
			<i>Other parameter estimates omitted for clarity</i>					
N. instruments	99	47	112	60	–	–	–	–
N. countries	130	130	137	136	133	133	140	140
Observations	784	527	915	658	917	660	824	637
AR(2) (<i>p</i> -value)	0.29	0.06	0.99	0.36	–	–	–	–
Hansen test (<i>p</i> -value)	0.39	0.23	0.47	0.10	–	–	–	–
<i>R</i> ²	–	–	–	–	0.27	0.32	0.28	0.26

Notes: This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. While the first four columns present difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

size. Moreover, including the Great Financial Crisis within the scope of the sample lead to an even higher coefficient (columns 3 and 4). The relationship between finance and growth has degenerated over time, reinforcing the intuition that no economic association is an immutable law (Rousseau and Wachtel, 2011).

As the weak instrument issue is still of serious concern with this identification strategy, one should handle these estimates with caution. Columns (4-8) favor the OLS fixed effect estimator. The latter also deals with the endogeneity stemming from time-invariant country's specifics. The absence of instruments, and thereby weak instrument proliferation, reduces the overfitting issue. Finally, as the GMM instruments are weak, the remaining endogeneity bias certainly remains unaddressed. Whereas in Table I.7, the OLS fixed effect estimates failed to support the existence of a threshold, the estimates in Table I.8 provide support for an overall negative association. A similar pattern emerges with a stronger association for more recent periods.

Altogether, the various estimates displayed in Table I.8 provide evidence of a rather damaging influence of financial development on economic growth. This finding is in line with a recent strand of the literature, unveiling similar conclusions on various sample

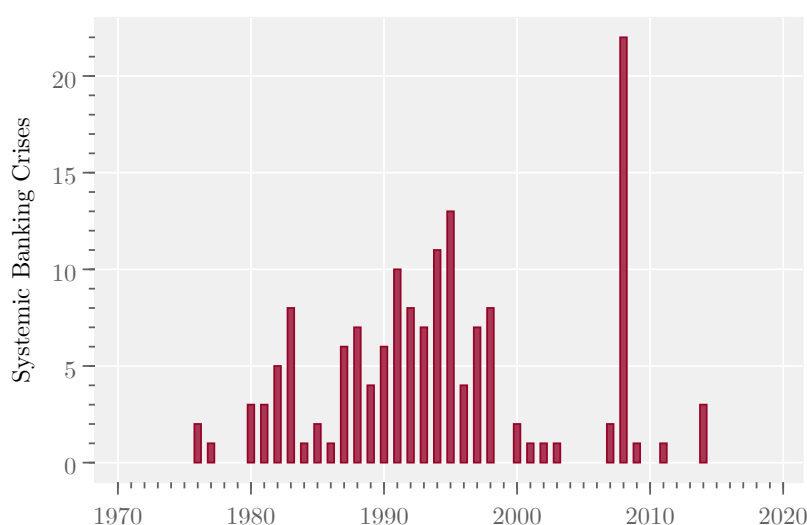


Figure I.4 – Occurrence of 151 systemic financial crises in the world since 1970. Source: *Systemic Banking Crisis Database* (Laeven and Valencia, 2018).

sizes. Cournède and Denk (2015) find a comparable negative relationship between intermediated credit and GDP growth on a smaller sample of 34 OECD economies from 1961 to 2011. With a shorter time coverage on a comparably large sample of 126 economies, Demetriades, Rousseau, and Rewilak (2017) also find that private credit has a negative impact on GDP growth. Their results are robust to controlling for systemic crises. Karagiannis and Kvedaras (2016) emphasize that there is no evidence of a threshold when restricting the panel to the OECD or the EU countries, but conclude to a robust negative impact holding for a range of samples and identification strategies. Cecchetti and Kharroubi (2015) reexamine the relationship between financial depth and growth and provide evidence of a negative association between finance and total factor productivity growth at both the country and the industry level. The recent work of Benczur, Karagiannis, and Kvedaras (2019) provides evidence of a negative association between credit to the private sector and growth. Their conclusion is robust to various specifications and subsamples (OECD, EU members, EMU). Finally, the present study's findings resonate with the recent evidence in Cheng, Chien, and Lee (2020), where financial development is unfavorable for economic growth. These first estimates constitute a starting point on which the analysis must presently be refined.

Table I.9 – Growth effects of financial development controlling for systemic banking crises (Laeven and Valencia, 2018).

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Old 1960-2010	Old 1980-2010	New 1960-2015	New 1980-2015	Old 1960-2010	Old 1980-2010	New 1980-2015	New 1990-2015
Private Credit	-2.874** (1.182)	-3.049* (1.586)	-4.764*** (1.121)	-5.253*** (1.972)	-1.355*** (0.433)	-1.870*** (0.591)	-1.678*** (0.525)	-1.851*** (0.689)
Banking Crisis	-1.906*** (0.651)	-1.935** (0.882)	-2.534*** (0.485)	-1.917*** (0.726)	-1.232*** (0.238)	-1.201*** (0.273)	-1.285*** (0.258)	-0.832*** (0.257)
<i>Other parameter estimates omitted for clarity</i>								
Observations	739	527	915	658	872	660	666	487
N. Countries	130	130	137	136	133	133	136	135
R^2	–	–	–	–	0.29	0.35	0.37	0.32
N. Instruments	107	54	122	69	–	–	–	–
AR(2) test p -value	0.301	0.173	0.753	0.200	–	–	–	–
Hansen test p -value	0.441	0.060	0.504	0.062	–	–	–	–

Notes: This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. See complete Table A.35 in the appendix for omitted parameters. While the first four columns present difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5.2 An Adverse Effect Beyond Systemic Banking Crises

A body of research indicates that financial crises have long-lasting negative influence on subsequent economic activity (Kaminsky and Reinhart, 1999; Jordà, Schularick, and Taylor, 2016; Grjebine and Tripier, 2017). Figure I.4 underlines that systemic banking crises are legions since the 1970s, with a strong emphasis on the Great Financial Crisis of 2007-2008. As emphasized in the previous section, focusing on post-1980s observations in Table I.8 magnifies the coefficient size. Moreover, including the Great Financial Crisis within the sample's scope systematically lead to a higher coefficient (columns 3 and 4).

At this stage, the natural question is to wonder if the occurrence of financial crises is possibly driving the overall adverse result from Table I.8? To provide an answer to this question, Table I.9 provides the same estimates, this time controlling for systemic banking crises based on Laeven and Valencia (2018). Banking crises are negatively related to GDP growth, which corroborates previous evidence on the matter.

The estimates reveal that financial distress is a substantial channel underlying the adverse effect of private credit. This impact is certainly undersized due to the time fixed effects capturing part of the Global Financial Crisis. The size of the coefficients is sensitive to the inclusion of a crisis dummy. The coefficient falls by about one third or a half when financial crises are controlled for. However, point estimates for the financial depth proxy remain statistically significant. Banking crises are not the sole driver of the negative association between finance and growth. Nonetheless, it can partially account for the overall negative association between finance and growth.

5.3 Does One Size Fit All?

The demonstration presented throughout the first Chapter provides statistical evidence against a non-monotonic estimation based on the level of private credit. However, as the literature documents ambivalent effects, the relationship between finance and growth may vary according to the general level of development. A body of empirical research has documented that the effect of finance on growth differs across advanced and developing countries. While some studies find that the positive effect of financial development is stronger in middle-income countries (Rioja and Valev, 2004), other empirical evidence suggests that the finance-growth nexus is more pronounced in low and middle-income countries (De Gregorio and Guidotti, 1995). Is the average overall effect documented in this Chapter masking some disparities?

To provide an answer to this question, I also explore the differential effects of private credit on growth across countries at various development stages. Focusing on smaller subgroups of countries to investigate cross-country heterogeneity could reduce the degrees of freedom and thereby the estimator's reliability. Therefore, I follow Rioja and Valev (2004) and account for differentiated effect based on income group dummies, instead of dissociated estimations. To this purpose, the following model is estimated:

$$\begin{aligned}\Delta y_{i,t+k} &= \lambda y_{i,t} + \beta_1 PC_{i,t} + \beta_2 (PC_{i,t} \times MH_i) + \beta_3 (PC_{i,t} \times ML_i) + \gamma \mathbf{x}_{i,t} + \nu_{it+k} \\ \nu_{it+k} &= \mu_i + \lambda_{t+k} + \varepsilon_{i,t+k}\end{aligned}$$

Table I.10 – Financial development growth effects with regions.

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Old 1960-2010	Old 1980-2010	New 1960-2015	New 1980-2015	Old 1960-2010	Old 1980-2010	New 1980-2015	New 1990-2015
Private Credit	-4.941*** (1.259)	-7.872*** (2.016)	-5.542*** (1.206)	-8.394*** (2.081)	-2.280*** (0.477)	-2.894*** (0.692)	-2.125*** (0.670)	-3.132*** (0.756)
Private Credit × Medium-high inc.	1.807 (2.311)	-0.274 (5.094)	1.733 (2.064)	2.810 (2.472)	1.613** (0.794)	0.829 (1.029)	2.495** (0.978)	1.726 (1.853)
Private Credit × Medium-low & Low inc.	12.515** (5.731)	15.961*** (6.130)	3.037 (3.374)	6.896 (4.756)	4.169*** (1.528)	5.375*** (1.292)	5.868*** (1.745)	6.388*** (2.419)
<i>Other parameter estimates omitted for clarity</i>								
Observations	784	527	915	658	917	660	666	487
N. Countries	130	130	137	136	133	133	136	135
R^2	–	–	–	–	0.28	0.35	0.33	0.32
N. Instruments	129	61	132	78				
AR(2) test (p -value)	0.264	0.078	0.864	0.195	–	–	–	–
Hansen test (p -value)	0.262	0.693	0.511	0.076	–	–	–	–
LINEAR COMBINATIONS OF PARAMETERS FOR PRIVATE CREDIT								
High income	-4.941*** (1.259)	-7.872*** (2.016)	-5.542*** (1.206)	-8.394*** (2.081)	-2.280*** (0.477)	-2.894*** (0.692)	-2.125*** (0.670)	-3.132*** (0.756)
Medium-high income	-3.133 (-2.159)	-8.145* (4.625)	-3.809* (2.106)	-5.583** (2.401)	-0.667 (0.769)	-2.064** (0.983)	0.370 (0.959)	-1.405 (1.798)
Medium-low & Low income	7.574 (5.213)	8.088 (6.189)	-2.505 (3.318)	-1.498 (4.802)	1.889 (1.471)	2.480** (1.220)	3.742** (1.701)	3.256 (2.457)

Notes: This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. See complete Table A.34 in the appendix for omitted parameters. While the first four columns present difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

This specification returns three coefficients estimates for *Private Credit*. The effect of financial development on growth in high-income economies is β_1 , in the medium-high income economies (MH_i), it is $\beta_1 + \beta_2$, and in the medium-low and low-income economies (ML_i), it is $\beta_1 + \beta_3$. Table I.10 reports the estimates for this interaction model along with the linear combinations of coefficients estimates with significance tests in the bottom panel.

Consistent patterns emerge from high-income economies, in line with findings from Chapter 2. The evidence from Table I.10 indicates that high-income economies drive the overall adverse effect documented in Table I.8. *Private Credit* is rather positively associated with growth in medium-low and low-income economies, albeit not systematically

in a significant way.

These findings suggest that financial depth has a different effect on growth depending on the income region the economies belong to. These estimates support a non-monotonous finance-growth nexus, not based on the level of financial depth, but rather on the development stage of an economy.

5.4 Growth, Financial Development and GDP level.

Finance is crucial at the early stages of development, by reducing the need to finance projects from own funds, it enables firms to engage in growth-enhancing activities (Gallor and Moav, 2004; Aghion, Howitt, and Mayer-Foulkes, 2005; Levine, 2005). The evidence from Table I.10 suggests a non-monotonous finance-growth nexus based on the development stage of an economy calls for additional investigation.

To further probe the relevance of a non-monotonous relationship between finance and growth, I turn to growth regressions with an interaction term between *private credit* and GDP per capita. Table I.11 presents various estimations based on two different time coverage and three estimations methods. First, the Difference-GMM estimates in the first two columns are somewhat inconclusive and fail to find a precise threshold. However, turning to the OLS fixed-effects estimates in columns (3) and (4) provide precisely estimated thresholds around 8,500\$ of GDP per capita. As emphasized by Figure I.5 displaying the marginal effect of *Private Credit*, below the threshold, private credit is positively associated with future growth whereas it exerts an adverse effect on subsequent growth above this threshold. This finding is consistent with the evidence from previous Table I.10 in which high-income economies face essentially adverse effects whereas medium-low income economies display an ambivalent yet possibly benefic association with financial depth.

To ascertain these findings, I provide additional estimates based on a fixed-effect panel threshold method (Hansen, 1999; Wang, 2015) to empirically investigate the non-monotonicity of the finance-growth nexus based on GDP per capita. This technique has the advantage of not imposing a priori restriction on the relationship, as would a quadratic form. It is suited to estimate a structural break between financial depth

Table I.11 – Institution interaction, Log(initial GDP/capita).

Estimator:	DIFF-GMM		OLS-FE		HPT-FE	
	(1)	(2)	(3)	(4)	(5)	(6)
NEW dataset						
Period:	1960-2015	1980-2015	1960-2015	1980-2015	1960-2015	1980-2015
Log(initial GDP/capita)	-3.169*** (0.806)	-2.310* (1.206)	-3.922*** (0.397)	-5.399*** (0.708)	-4.135*** (0.618)	-5.569*** (0.472)
Private Credit	4.358 (5.827)	13.810 (9.053)	10.719*** (2.645)	12.298*** (3.339)		
Log(initial GDP/capita) × Private Credit	-0.790 (0.582)	-1.784** (0.860)	-1.184*** (0.267)	-1.375*** (0.325)		
Private Credit IF Log(initial GDP/capita) < τ					8.201*** (2.492)	0.595 (0.626)
Private Credit IF Log(initial GDP/capita) $\geq \tau$					-1.401*** (0.497)	-1.866*** (0.448)
<i>Other parameter estimates omitted for clarity</i>						
Observations	915	658	1055	798	352	546
N. Countries	137	136	140	140	32	78
R^2	–	–	0.28	0.31	0.41	0.39
N. Instruments	128	68	–	–	–	–
AR(2) test (p -value)	0.653	0.069	–	–	–	–
Hansen test (p -value)	0.330	0.034	–	–	–	–
Threshold τ	5.51	7.74	9.05	8.95	7.31	9.95
Threshold 90% CI	[2.80-8.22]	[5.03-10.45]	[8.63-9.48]	[8.30-9.58]	[7.24-7.32]	[9.91-9.96]
Threshold-effect F -test (p -value)	–	–	–	–	0.06	0.01

Notes: This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The threshold τ is Log(initial GDP/capita). See complete Table A.25 in the appendix for omitted parameters. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (Parentes and Silva, 2012). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

and growth. Rousseau and Wachtel (2011) used this estimator to document that the finance-growth nexus depends on the level of inflation. We apply a similar setup, this time focusing on GDP per capita, as follow:

$$\Delta y_{i,t+k} = \lambda y_{i,t} + \beta_1 (y_{i,t} < \tau) PC_{i,t} + \beta_2 (y_{i,t} \geq \tau) PC_{i,t} + \gamma \mathbf{x}_{i,t} + \nu_{it+k}$$

$$\nu_{it+k} = \mu_i + \lambda_{t+k} + \varepsilon_{i,t+k}$$

where the threshold variable $y_{i,t}$ represents the GDP per capita (in logs), and τ is the threshold parameter that divides the equation into two regimes with β_1 and β_2 as respective coefficients (for more details, see Hansen, 1999; Wang, 2015)

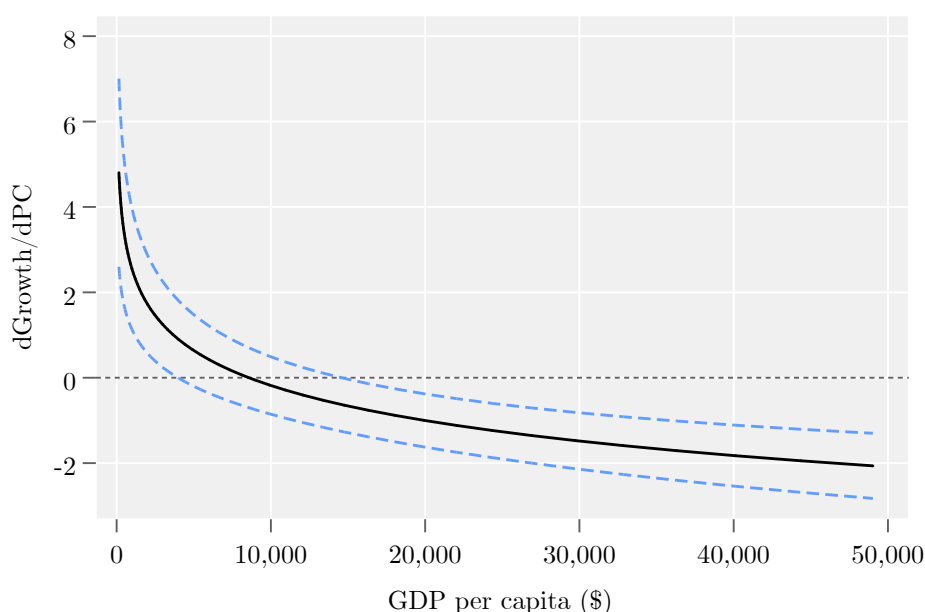


Figure I.5 – Estimated *Private Credit* parameter over the observed range of GDP per capita for 1960-2015 in the new sample. The solid black line plots the marginal effect of *Private Credit* from Table I.11, column (3). The solid light lines are 90% confidence intervals. The threshold estimated is estimated at a level of GDP per capita of 8,518\$.

The results are reported in columns (5) and (6). For the first column covering 1960-2015, the threshold is evaluated at 1,500\$. The estimates provide evidence of a highly positive association between finance and growth for low-income economies at an early stage of development. This evidence is in line with Huang and Lin (2009), documenting a more pronounced positive effect for the low-income countries. Above the reasonably low threshold, the estimates from column (5) support a significantly negative effect corroborating the previous results in this section.

Note that the LR statistic displayed in the top panel of Figure I.6 seems to identify several possible candidate thresholds for the regression displayed in Table I.11 column (5). For robustness, I checked a multiple thresholds regression based on the same setup. The single threshold model displayed in column (5) returns a F -stat of 25.07 with the associated p -value of 0.0075. In contrast, the double thresholds model returns a F -stat of 10.18 with a much higher p -value of 0.31. Therefore, I reported the single threshold estimates.

The shorter time-coverage in column (6) should be more reliable for several reasons.

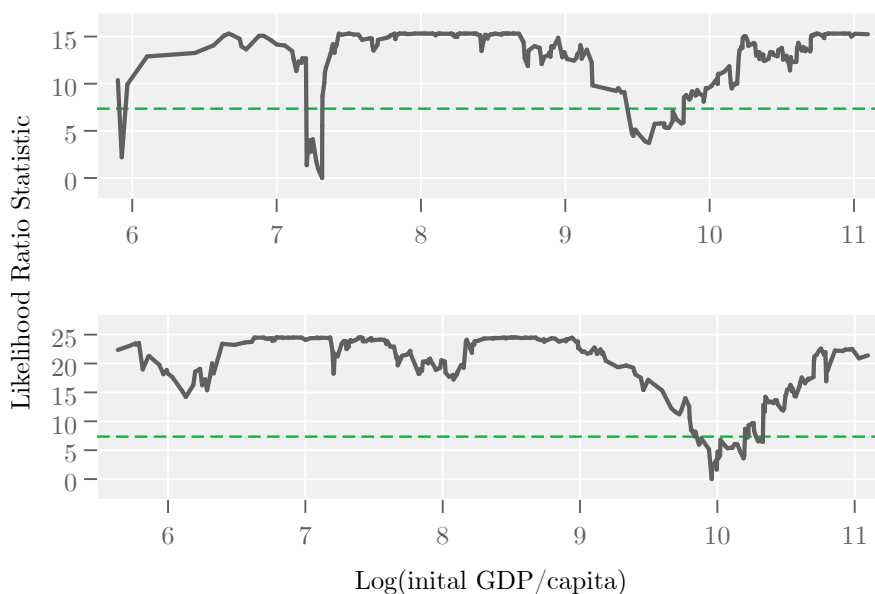


Figure I.6 – The LR statistic from Table I.11 columns (5) and (6), respectively top and bottom, is constructed as $LR = \frac{[SSR(\tau) - SSR(\hat{\tau})]}{\hat{\sigma}^2}$, where $\hat{\tau} = \arg \min SSR(\tau)$; SSR is the sum of squared residuals obtained by estimating the model for different values of the threshold variable. The green dashed line denotes the critical value (7.35) at the 95% confidence level for the LR statistic.

First, as the estimation procedure requires a balanced panel, going as far back as 1960 substantially reduces the sample size. Moreover, the LR statistic displayed in the bottom panel of Figure I.6 undoubtedly identifies a single threshold model. The threshold is precisely estimated at 21,000\$ (see the p -value lower than 0.01). This time, there is no significant association between finance and growth below this threshold value. Above the threshold, economies should experience the adverse effect of financial depth. The estimates from column (6) are, therefore, corroborating earlier evidence of the negative influence of financial depth being driven by economies at a higher stage of development.

For further robustness checks on the adverse effect of financial depth on growth, I also ran estimations controlling for 15 additional institutions proxies²⁵ extracted from the Worldwide Governance Indicators database²⁶ of Kraay, Kaufmann, and Mastruzzi (2010), the Economic Freedom of the World database²⁷ of the Fraiser Institute (Gwart-

25. See the data appendix page 245.

26. Worldwide governance index (6 dimensions), Voice and accountability, Political Stability and Absence of Violence/Terrorism, Government effectiveness, Regulatory quality, Rule of law, Control of corruption. See the appendix, Tables A.26–A.29 page 248–251.

27. Economic Freedom Index, Credit market regulation, Regulation. See the appendix, Tables A.30

ney, Lawson, and Norton, 2008), and the Quality of Governance database²⁸ (Teorell et al., 2020).

Overall, interactions with these various institution proxies fail to deliver a clear threshold. The main reason for the arguably low relevance of these estimates rests in a very scarce country and time coverage. For example, the Quality of Governance database covers post-2000 observations, leaving us with a shrunk time dimension of only one period for the Difference-GMM estimates and two periods for the fixed effects OLS estimates. Nonetheless, the overall adverse effect remains robust to these smaller samples and, more importantly, to the inclusion of a variety of institutional proxies. Thus, the adverse effect is not entirely due to a lack of suited institutions.

5.5 Further Financial Reform as a Possible Way Out?

Can financial reform mitigate the adverse effect of financial depth on growth? As emphasized by the early work of Jappelli and Pagano (1994), the liberalization of consumer credit, led to a reduction in saving and growth. Eichengreen and Leblang (2003) also documents a negative impact of financial liberalization on growth, along with Beck et al. (2012) who find that household's credit does not promote growth. Moreover, along the liberalization process, many countries experienced episodes of financial distress. An inadequate sequencing of reform might be behind such a detrimental outcome.

To understand to what extent financial reform might be a way out of financial depth adverse effect, I run the last set of regressions controlling for financial reform. This exercise resonates with the investigation of Rousseau and Wachtel (2011). They wonder if some Lucas critique could be the cause of the changing outcome of financial depth on growth. As emphasized by Figure I.7, in the 1970s, governments lunched a widespread reform process toward liberalization, which might have dissolved the initial advantages of financial expansion:

“[...] any change in policy will systematically alter the structure of economic models.” (Lucas, 1976, p.40)

and A.31 page 252.

28. State fragility index Functioning of government, Rule of law, Economic freedom index. See the appendix, Tables A.32 and A.33 page 254.

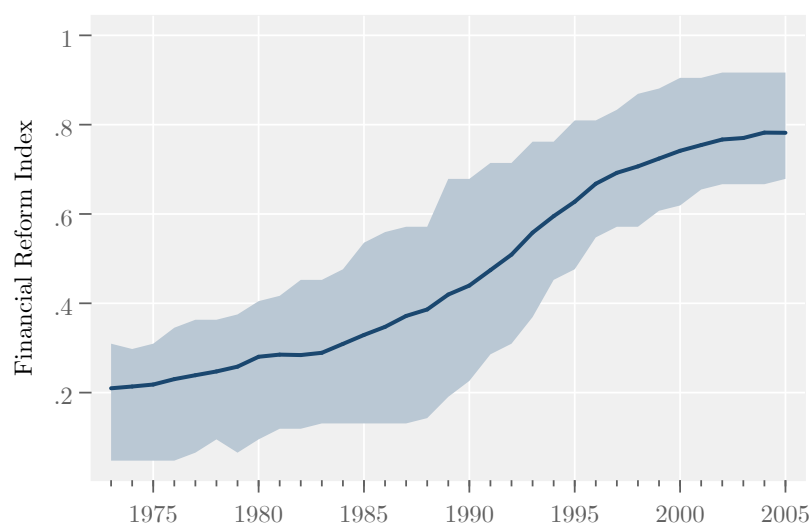


Figure I.7 – Financial reform toward liberalization of the financial system around the world. Financial reform index based on seven dimensions. Observations for 91 countries. The confidence band represents the 25th and 75th percentiles. Source: *Database of Financial Reforms*, (Abiad, Detragiache, and Tressel, 2010).

This subsection is based on the IMF financial reform index (Abiad, Detragiache, and Tressel, 2010). This index measures the degree of financial regulation, normalized to vary from 0 (stringent) to 1 (lax), yearly between 1973 and 2005. The index combines financial regulation policies along seven dimensions: i) credit controls and reserve requirements, ii) interest rate controls, iii) entry barriers, iv) state ownership, v) securities market policies, vi) banking regulations, and vii) restrictions on the financial account. Figure I.7 shows that overall economies relaxed financial constraints over the three decades to 2005.

Table I.12 reports the results of this exercise. First of all, the negative point estimates for private credit are robust to controlling for financial reform. The adverse effect of finance on growth is not exclusively a byproduct of liberalization. The Financial Reform Index's coefficient estimates are positive and statistically significant for the OLS fixed-effects estimates. While offering a possibility of adjusting the adverse effect of financial depth by growth-promoting financial reform, these results deliver an ambiguous message, as the index, by construction, mixes some financial deregulation proxies with banking supervision considerations. Nonetheless, the evidence tends to support that financial reform has been rather growth-promoting. One possible reading through this result

Table I.12 – Financial development growth effects controlling for financial reform (Abiad, De-tragiache, and Tressel, 2010).

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Old 1960-2010	Old 1980-2010	New 1960-2015	New 1980-2015	Old 1960-2010	Old 1980-2010	New 1980-2015	New 1990-2015
Private Credit	-2.941** (1.445)	-8.030*** (2.824)	-6.823*** (1.816)	-8.057*** (2.834)	-1.564** (0.598)	-2.285*** (0.641)	-1.877*** (0.530)	-2.780*** (0.661)
Financial Reform Index	4.182** (1.769)	3.923 (2.994)	1.682 (1.795)	0.609 (2.350)	2.869*** (0.956)	2.109* (1.078)	2.036** (0.872)	3.178*** (0.974)
<i>Other parameter estimates omitted for clarity</i>								
Observations	319	263	410	350	400	344	431	308
N. Countries	79	79	81	81	81	81	81	81
R^2	–	–	–	–	0.32	0.40	0.35	0.33
N. Instruments	72	39	87	54	–	–	–	–
AR(2) test p -value	0.230	0.114	0.549	0.200	–	–	–	–
Hansen test p -value	0.249	0.159	0.380	0.052	–	–	–	–

Notes: This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. See complete Table A.36 in the appendix for omitted parameters. While the first four columns present difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes and Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

would be that financial deregulation impelled financial depth while banking supervision enhanced the financial system's soundness.

Anyhow, the financial reform index remains an imperfect proxy, still at an early stage of construction. The estimates reported here provide some insight, but further research would be required to disentangle the financial reform's growth implication fully.

To conclude this section, in light of the evidence, the present paper further contributes to the analysis of the finance-growth nexus by providing alternative estimates supporting an overall damaging influence of financial development on economic growth. These estimates are robust to various time coverages, two estimators, two datasets, and several empirical exercises. Evidence suggests that the negative influence of financial depth is stronger in high-income countries, with possibly growth-enhancing effects at the early stages of development.

6 Conclusions

This paper investigates the relevance of a threshold beyond which financial depth tends to affect growth adversely. It seeks to understand why prior evidence relying on large panels led to such non-linear conclusions, where short panel or other estimations techniques failed to do so. Overall, this study contributes to the analysis of the impact of financial development on economic growth twofold.

First, from a methodological standpoint, this study provides a thorough reappraisal of recent advances in the finance-growth literature. It explores the soundness of various mainstream identification strategies. It presents a body of evidence reducing the confidence one can have in the thresholds estimates. Deriving new estimates, with additional data or slight changes in the methodology, casts new doubts about the existence and reliability of a financial tipping point, complementing the recent findings of Botev, Égert, and Jawadi (2019). The starting year of the five-year spells influences the results, as only one out of five possible anchoring years supports the non-linear evidence. This study demonstrates that the threshold conclusion requires a peculiar methodological setup relying on extensive use of either irrelevant or weak instruments. These problematic instruments, combined with the near-multicollinearity of the financial proxies, result in spurious threshold regressions overfitting a few outliers. This paper pledges for a systematic investigation of the instruments' validity and relevance. Beyond the standard specification tests, studies should systematically report choices regarding the instrument matrix, the instrument count, and test for weak instruments.

Second, by adequately accounting for country heterogeneity, along with a more contained use of instruments, evidence points to an overall damaging influence of financial development on economic growth. These empirical findings support the hypothesis that, in the long run, financial deepening has done more harm than good, with a stronger emphasis on more recent periods. This finding is in line with recent literature that finds similar evidence. The estimates also emphasize that some non-monotonicity might still drive the finance-growth nexus, not based on financial depth level, but rather on the development stage of an economy. Evidence suggests that the negative influence of

financial depth is stronger in high-income countries, with possibility growth-enhancing effects at the early stages of development.

From a policy perspective, the present paper's recommendations are comparable to those drawn from the thresholds evidence; however, for different reasons. Both advocate that there is indeed an excessive development of the financial sector. On the one hand, after a certain threshold level (met for most developed economies), on the other hand, for structural reasons. A dysfunctioning financial sector misallocates scarce resources and spurs speculation. Far from advocating that financial development is irrelevant to growth, this paper calls for a better alignment of the financial system to economic needs.

Hence, the negative impact of finance and growth could undoubtedly be dampened by proper policies, starting with reinforcing the quality of the institutions by pursuing financial regulation and supervision. Making the financial system more resilient would also certainly reduce the detrimental effect of financial deepening on growth. Both the Banking Union and Capital Markets Union within the European Union go along this road. Credit constraints should also be strengthened to limit the excessive expansion of financial credit. The massive expansion associated with credit easing as a monetary answer to the great financial crisis should be viewed with much caution. Peculiar attention should be drawn to the unprecedented rise of household credit.

This study also acknowledges a great deal of uncertainty in disentangling the finance-growth nexus. It provides grounds to explore further the impact of the financial sector development on economic growth. Specifically, research will need to provide stronger evidence of a causal impact as well as a better understanding of the various channels at work. The dark side of the financial sector remains mostly unaccounted.

The search for general laws applicable to all countries at all times seems doomed to fail. Further research based on country-level data should focus on more homogeneous sets of countries and more recent periods. It would undoubtedly be more reliable and informative for devising suited policies to ensure that we make the most of the financial sector. Finally, the profession's consensus to study financial depth solely from the perspective of the private sector should be challenged. The financial sector's development also rests on providing finances to the public sector, indisputably affecting growth.

The Interplay of Public & Private Debts

Abstract – Private credit and public debt levels are drifting to uncharted waters. Are such high levels concerning? This chapter suggests bridging private and public borrowing to study their interaction. This investigation relies on an array of econometric techniques and builds on disaggregated data on debt recipients covering 1970-2018. Overall, this paper demonstrates that the separate narratives that have emerged regarding private credit and public debt with output growth conceal meaningful and substantial interactions. In particular, evidence shows that private credit exerts a robust adverse effect on subsequent growth both in the short-run and long-run. The expansion of household credit and the triggering of financial crises account for most of this effect. This study also shows that it is not the level of public debt but rather its trajectory that affects economic growth. Public debt tends to be a drag on growth, not initially, but in the aftermath of a crisis if policymakers are unable to pursue a counter-cyclical fiscal policy. From a policy perspective, economies should closely monitor private credit expansions and maintain some fiscal space in normal times to buffer the fall-out from financial crises.

Keywords Public Debt · Private Credit · Growth · Business Cycles · Panel Data

JEL classification E44 · E32 · G01 · G2 · H63 · O4

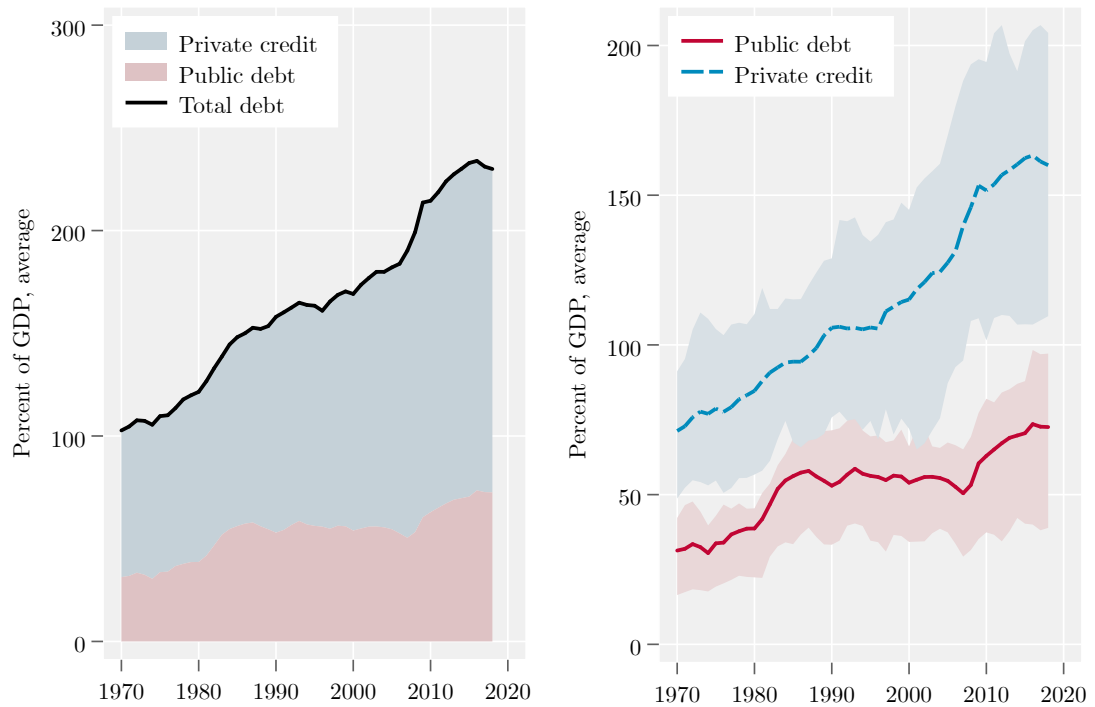


Figure II.1 – Forty years of average private sector liabilities and public debt relative to GDP for 34 countries in the sample. The confidence areas in the right panel represent the first and third quantiles.

1 Introduction

Since the wake of the Global Financial Crisis, the accumulation of both large private sector liabilities and public debts has been the subject of renewed interest. A decade later, financial depth persists in drifting to uncharted waters, as emphasized in Figure II.1. Are such levels concerning? Does the financial recipient matters in promoting output growth? Based on a panel of 34 countries ranging from 1970 up to 2018, the present study investigates if and how private credit and public debt interact in determining output growth.

The difference between the individual and joint impact lacks investigation. The interplay of private credit and public debt remains understudied. The separate literature and narratives surrounding public and private debts may conceal meaningful and understudied interactions. Consider the semantics. On the one hand, private credit, on the other hand, public debt. Debt and credit are two sides of the same coin. Intriguingly,

each branch of the literature has its semantics. Credit is associated with the financial depth literature, dealing with the allegedly productive aspects of the financial sector. Debt usually has a negative undertone and is commonly associated with the literature on public finances. Both private credit and public debt, however, perform very similar functions in theory, especially in recent times. Drawing on a historical perspective, Eichengreen et al. (2019) documents the change in the role of public debt, from war finance to financing public goods, with defaults being rather infrequent. Private sector liabilities and public debt both enable agents to shift resources over time, whether it is through a lifetime or generations.

The vast majority of the literature has either focused on private credit or public debts, keeping the two separated. The finance-growth literature, investigating the implication of financial development, has concentrated its efforts on credit to the private sector. The absence of a consensus is a key defining characteristic of this literature. However, the latter unites in the exclusion of public sector finance from its scope. Most often, this exclusion is implicit, considering it as too obvious to be argued otherwise (see Levine and Zervos, 1998). The seminal work of King and Levine (1993) is one of the rare studies to consider the issue explicitly:

“Financial systems that primarily fund private firms probably [sic.] provide more services than financial systems that simply funnel credit to the government or state enterprises.” (King and Levine, 1993, p. 718)

Presumably, financial systems funneling credit to the public sector might have different properties than financial systems providing funds to the private sector. However, the understanding of these properties should be rooted in hard evidence rather than mere assumptions. In modern economies, the public sector borrows on markets very much like a firm or a household. Both its solvability and fiscal choices are closely scrutinized to assess risks and interest rates. Rating agencies provide information.

Another strand of research has documented an important influence of public debt on the economy. Sparked by the controversial work of Reinhart and Rogoff (2010) and the European sovereign debt crisis, the empirical literature has shown a renewed interest into the growth effects of high public debt, specially focussing on nonlinearities and

thresholds.¹ Among others, Panizza and Presbitero (2013) invite policy makers to much caution in devising policies based on the latter findings. They emphasize the general dependency of the conclusion on change in data coverage or the specifics of the empirical technique resorted to. Thus, a strand of the literature now puts a strong emphasis on cross-country heterogeneity in the relationship (Gomez-Puig and Sosvilla-Rivero, 2014; Lof and Malinen, 2014; Eberhardt and Presbitero, 2015; Égert, 2015a; Puente-Ajovin and Sanso-Navarro, 2015; Ahlborn and Schweickert, 2018).

Research focusing on the importance of the overall financial structure is currently quite limited. Some early studies have focused on both private credit and public debt, yet only through the angle of external debt and almost exclusively for low-income economies (Pattillo, Poirson, and Ricci, 2002). Domestic credit has been used instead of private credit as a proxy for financial depth in the early finance-growth literature (King and Levine, 1993; Rajan and Zingales, 1998). This alternative variable comprises private credit as well as credit to the public sector. Unfortunately, it remains a narrow definition of public debt. Recent studies investigate the breakdown of private credit into household and non-financial corporations. They find that credit to corporations tends to be positively associated with economic growth. In contrast, household credit is not (Beck et al., 2012; Arcand, Berkes, and Panizza, 2015; Benczur, Karagiannis, and Kvedaras, 2019). To date, no study has conducted a thorough investigation of the overall fund recipients' structure of financial services, including the public sector.

Jordà, Schularick, and Taylor (2016) are among the first to study the interaction between private credit and public debt.² Based on an historical sample for 17 advanced countries from 1870 to 2012, they examine the co-evolution of public and private sector debt and find that high levels of public debt intensify the effects of financial crises mainly induced by private credit booms.

The present paper follows their steps and complements their work in several aspects.

1. See, among others, Grennes, Caner, and Koehler-Geib (2010), Cecchetti, Mohanty, and Zampolli (2011), Checherita-Westphal and Rother (2012), Baum, Checherita-Westphal, and Rother (2013), Kourtellos, Stengos, and Tan (2013), Afonso and Jalles (2014a), Panizza and Presbitero (2014), Teles and Cesar Mussolini (2014), and Woo and Kumar (2015).

2. See Obstfeld (2013), Battaglini and Coate (2016), and Romer and Romer (2017) for more on government's response to financial crises constrained by high debt load.

First, the present study focuses on shorter time coverage. Working with an excessively long sample period might be misleading. Indeed, the consistency of the relationship between economic variables over centuries is doubtful. As emphasized both by Eichengreen et al. (2019) and Jordà, Schularick, and Taylor (2016), the function exerted by public debt has evolved, with a major break in the aggregate debt dynamics in the 1970s. Therefore, the present paper starts from there.

Second, the study works with a recent dataset conferring several advantages over previous studies. Among them is a detailed split on debt recipients with a consistent definition across time and countries, adjusted for breaks, consolidated public debt, and a broad financial instrument coverage. Thus, the present study broadens the scope by including additional countries. Overall, the dataset allows assessing whether prior conclusions hold for a more contemporaneous sample.

Third, observations up until 2018 enables the study to introduce the Global Financial Crisis within its scope. This should confirm if the latter financial turmoil is consistent with historical evidence.

Fourth, beyond sample considerations, the range of the study is broader. The present paper resorts to an array of empirical techniques to assess the joint impact of private credit and public debt from several perspectives. The study focuses not only on the joint impact on medium-run output growth but also on the triggering and unfolding of business cycles, providing a thorough outlook of the underlying mechanisms.

Evidence stresses that not all types of finances matter; some are substantially more detrimental to growth than others. Private credit deepening exerts a robust and strong adverse effect on growth. Albeit public debt is mostly not relevant to future growth, it can exert positive influence at lower levels. Nevertheless, medium-run evidence suggests that higher levels of public debt enhance private credit's detrimental effect.

Decomposing private credit further unveil interesting patterns. If policymakers ought to be concerned about rising levels of private liabilities, household credit should be carefully held at much lower levels (Mian, Sufi, and Verner, 2017). Bringing household credit back to its early 2000s level would deliver an additional half a percentage point of output growth per year in the medium-run for European Union members.

Delving into the business cycle analysis allows for a sharper understanding of the interplay between private credit and public debt. The conjunction of the various horizons provides a clearer understanding of the underlying mechanisms at play. The investigations emphasize that private sector credit has a detrimental effect on growth as it is recession triggering. Indeed, the financial crises account for a substantial part of private credit's adverse effect. Overall, the various estimates confirm previous evidence in Jordà, Schularick, and Taylor (2016) in showing that financial crises recessions lead to a sharper slowdown and a more delayed recovery than with normal recessions. A much more credit-intensive expansion precedes business cycles associated with financial crises as compared to normal recessions.

In line with the recent work of Pescatori, Sandri, and Simon (2014) and Chudik et al. (2016), this paper's evidence shows that it is not the level of public debt but rather its trajectory that affects economic growth. Business-cycle evidence emphasizes that public debt tends to be a drag on growth, not initially but in the aftermath of a crisis if policymakers are unable to run a counter-cyclical fiscal policy. The interplay of private credit surges with high public debt levels significantly alters the recession-recovery path. Initially high level of public debt compound the aftereffect of private credit surge.

From a policy perspective, economies should closely monitor private credit expansions. They should also maintain some fiscal space in normal times to buffer the fall-out from financial crises and reduce the subsequent recession's economic cost. Low levels of public debt to GDP at the start of the financial crisis recession enable the authorities to pursue active Keynesian macroeconomic stabilization. Real public debt per capita can rise substantially to meet the requirements for a swift recovery.

The conclusion differs from Reinhart and Rogoff (2010) as they consider a general threshold not to exceed under any circumstance. This "safe debt level" (Reinhart, Rogoff, and Savastano, 2003) is far from homogenous across countries. In line with Bernardini and Forni (2017), the present paper emphasizes the importance of accumulating debt at a sustainable pace to avoid the simultaneous retrenchment of both private and public sector borrowing in times of crisis.

The separate narratives that have emerged regarding private credit and public debt

with output growth conceal meaningful and substantial interactions, calling for further research.

The paper is structured as follows. Section 2 presents the data used throughout the study and provides some preliminary comments on a few stylized facts. Then, section 3 investigates the medium-run impact of private and public borrowing on output growth. Section 4 focuses on the interplay of private credit surges and public debt overhang on the recovery path from normal and financial crisis recessions. As section 3 and 4 rely on different econometric approach, a specific subsection will present the statistical design. Section 5 concludes.

2 Preliminaries

2.1 The Data

This section provides details on data sources. The study is based on an unbalanced panel database consisting of 34 countries from 1970 to 2018 with an average of 30.2 years per country. This sample of countries reflects 27 high-income countries and 7 middle-income countries based on the World Bank classification (Table B.2 of the Appendix provides a list of the countries).

A strand of the recent finance and debt literature has set the focus on long time coverage. The controversial study of Reinhart and Rogoff (2010) spans about 200 years and the pioneering work of Jordà, Schularick, and Taylor (2016) looks back as far as 1870. Long time coverage imposes a small cross-sectional dimension. The early study of Rousseau and Wachtel (1998) work with only five countries. More recent studies manage up to 19 countries for these long time sample look as far back as the nineteenth century (see Rousseau and Sylla, 2003; Bordo and Rousseau, 2012; Afonso and Jalles, 2014a). Dealing with fewer countries could partially address country heterogeneity issues. However, working with an excessively long sample period might be misleading. Indeed, the consistency of the relationship between economic variables over centuries is doubtful. As emphasized both by Eichengreen et al. (2019) and Jordà, Schularick, and Taylor (2016), the function exerted by public debt has evolved over time.

In light of this structural change, the present study goes along a different road and works with a shorter and more contemporaneous time coverage. The study also includes more countries, mostly developed, thanks to a broader data availability. Both time and country coverages should mitigate heterogeneity concerns.

The present paper works with a dataset presenting several additional advantages over previous studies. The aim is to evaluate potential interactions and composition-driven effects of two types of debt holders on output growth. The task requires disaggregated data on the debt's recipients.

Since 2015, The BIS publishes a detailed split of the borrowing activity of the public sector and the private nonfinancial sector.³ The first advantage is the ability to break down the total credit series into credit to the public sector and the private nonfinancial sector. The latter can be further split between nonfinancial corporations and households, unfortunately, at the cost of a scarce coverage.

Second, the construction of the dataset ensure the absence of sectorial coverage overlap and guaranties consistency across countries and time, enabling safe cross-country comparisons.

Third, the public sector aggregates the central, state, and local governments, unlike Reinhart and Rogoff (2010) who only use central government's debt. The data is consolidated in order to net out intra-sector positions. The BIS data on public debt boils down to the Maastricht debt used in the European Union and ensure an accurate picture of public debt.

Fourth, the financial instrument coverage comprises both loans and debt securities. This definition remains large: all liabilities that require some form of payment of interest and/or principal by the debtor to the (domestic and international) creditor at some date in the future.⁴

Fifth, private credit data are also available from the World Bank's Global Financial Development Database (Cihak et al., 2012). These series, however, display numerous

3. See the original communications on the database in Dembiermont, Drehmann, and Muksakuratana (2013) and Dembiermont et al. (2015) for more details.

4. See Dippelsman, Dziobek, and Gutierrez Mangas (2012) for a detailed presentation of the various dimensions of public debt.

structural breaks, whereas the BIS credit data are adjusted for such breaks (Benczur, Karagiannis, and Kvedaras, 2019).

Finally, the time coverage of the public debt variable has been carefully extended for numerous countries based on the Historical Public Debt Database of the International Monetary Fund (see Figure B.1 in the Appendix).

Other control variables are common to the growth empirics. They include the logarithm of initial Gross Domestic Product per capita, a measure of the level of human capital, a measure of trade openness, and two measures of macroeconomic stabilization, the log of the inflation rate and the log of government consumption normalized by GDP. Following Cecchetti, Mohanty, and Zampolli (2011), the controls also include a measure of gross saving as a share of GDP and the total dependency ratio as a measure of population's aging structure. Table B.1 in the Appendix contains a detailed description of the sources of all the variables.

2.2 A Few Stylized Facts

The right panel of Figure II.1 shows that public debt remained stable on average from the mid-1980s up to the financial crisis of 2007/8. Private credit, however, has doubled from roughly 80% to 160% of GDP, with a pre-crisis steep rise, prefiguring the intuition that private credit surge might be a good crisis predictor. The tremendous rise in debts has begun far before the Global Financial Crisis. Total debt build-up is not driven by a few large economies. High debts appear to be a common defining hallmark spread across the sample. Half of the countries now display a level of total debt exceeding 250% of GDP, and one-sixth is now beyond 300%. Figure II.2 confirms that public debt remained relatively low and concentrated as opposed to the steady increase of private credit over the past four decades.

Post-Crisis empirical works have contributed to identifying thresholds above which both private credit and public debt becomes growth-damaging. Interestingly, their respective threshold are both estimated at roughly 90% of GDP (see Reinhart and Rogoff, 2010; Arcand, Berkes, and Panizza, 2015, for private and public debt thresholds estimates, respectively). Public debt levels remain mostly below the estimated threshold

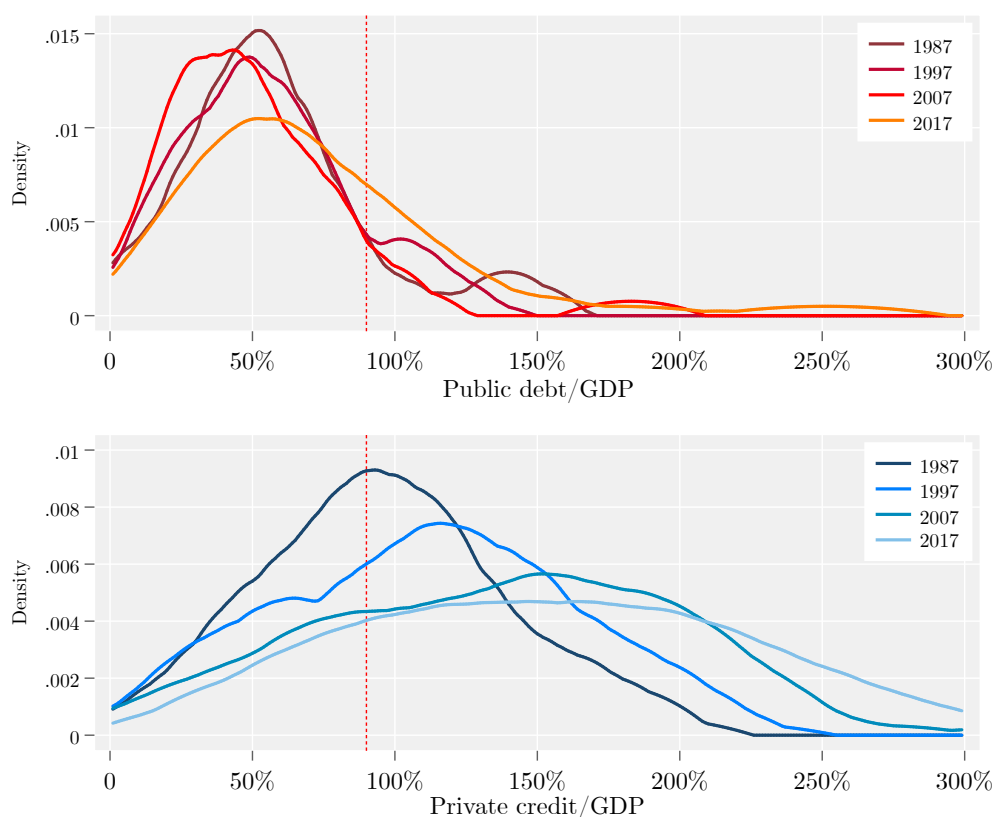


Figure II.2 – Overtime distribution of private sector liabilities and public debt relative to GDP. The vertical lines refer to the 90% of GDP threshold documented in the public debt literature (Reinhart and Rogoff, 2010) and finance-growth literature (Arcand, Berkes, and Panizza, 2015).

(marked by the vertical red dotted line) for the majority of countries in the sample. However, private credit surges during the past decades put most countries far above the 90% threshold. Therefore, in light of this evidence, the expected conclusions from the present paper would be a ambiguous effect for public debt, and a negative impact for private credit on growth.

These thresholds estimates should be viewed as heuristic.⁵ In what is now a very controversial paper, Reinhart and Rogoff connect median and average public debt to growth using bar charts to conclude to a public debt intolerance ratio at roughly 90%

5. Herndon, Ash, and Pollin (2014) unveiled that the Reinhart and Rogoff (2010) paper seems to contain a spreadsheet coding error that led to a miscalculation of the growth rates of some economies. As for the conclusions of Arcand, Berkes, and Panizza (2015), evidence shows that data cuts critically influence the outcome. Moreover, given the pervasiveness of weak instruments and the general dependence of the conclusion on a high instrument count, it is hard to rule out reverse or third variable causation. Finally, the quasi-colinearity of the financial proxies and few outliers further questions the existence of a threshold beyond which financial depth tends to adversely affect growth (see Chapter 1 for more details).

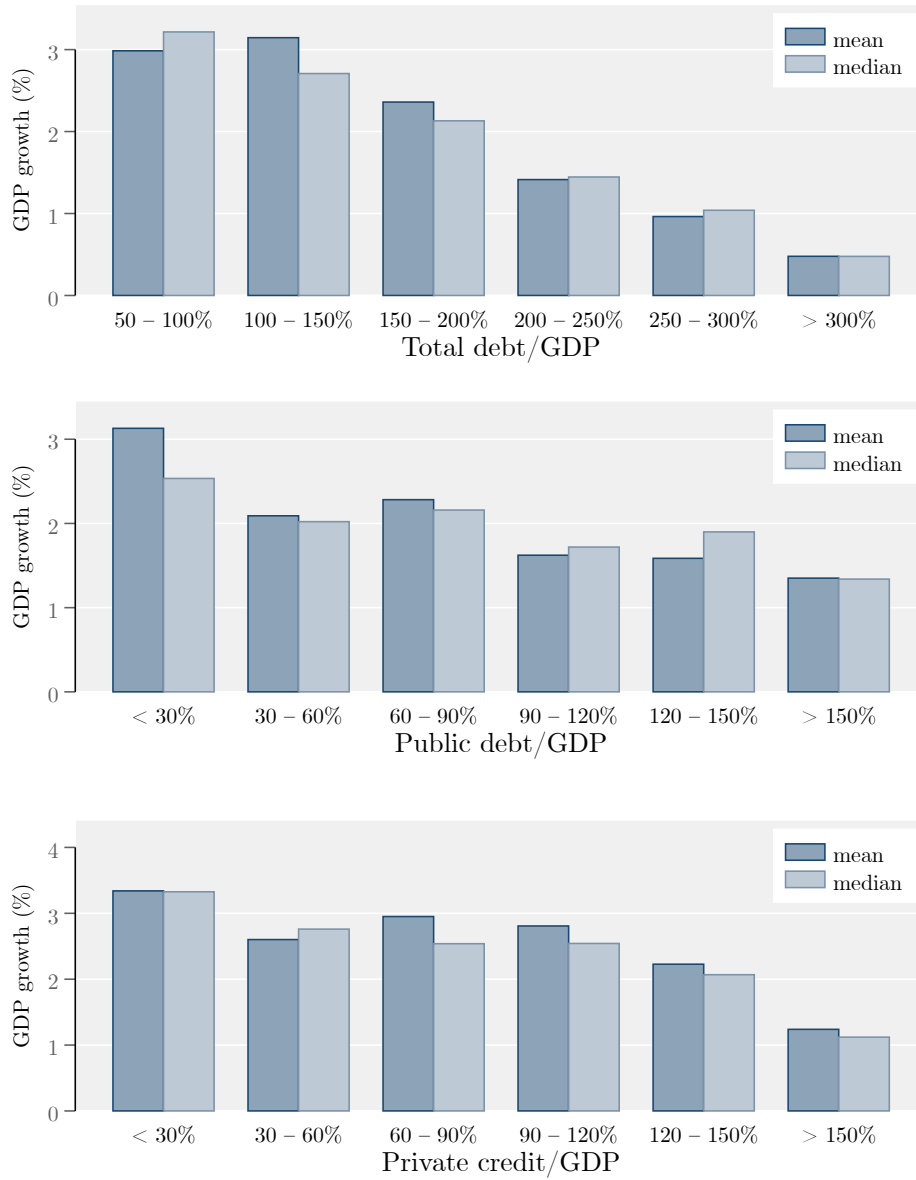


Figure II.3 – GDP growth and debt-to-GDP ratios. These figures follow the demonstration of Reinhart and Rogoff (2010) and extend the public and private debt categories up to 150%. Following the various remarks in section 3.1, these figures compare debt with five-year average future growth.

of GDP above which growth rates are distinctly much lower. Various policy recommendations of the OECD and the EU Commission have adopted this 90% threshold as a “natural target” for public spending. As a preliminary exercise, Figure II.3 derive similar bar charts not only for public debt, but also for total debt as well as private credit, expanding the various debt thresholds. The three charts displayed in Figure II.3 show similar downward trends suggesting a negative association between debt and future growth. Simple negative correlation coefficients between growth and the various debts corroborate this negative link (see correlation matrix for the various dimensions in Table B.4 of the Appendix). However, there is no clear cut drop advocating for the 90% threshold. Deriving policy recommendations solely from these charts would be tricky, as they certainly misrepresent the complex interplay between debts and growth.

3 Financial Interplay and Medium-Run Output Growth

Sections 3.1 and 3.2 overview the statistical design and cover parameter estimation. The econometric research strategy starts with aggregated financial variables and decomposes the overall structure of finance by fund recipients in section 3.3. Section 3.4 then examines non-linearities and interplay between private credit and public debt. This gradual approach intends to provide a precise picture of the medium-run debt-growth relationship.

3.1 Statistical Design

As is now common in the growth empirics, this study focuses on the impact of a set of variables on growth within a country. The statistical design follows Islam (1995) by estimating panel data regressions with time- and country-specific fixed effects. Let $i \in \{1, 2, \dots, N\}$ and $t \in \{1, 2, \dots, T\}$ stand respectively for country and time indices. For a fixed value of future horizon h , define the depend variable as average annual growth rate of per capita real GDP, for country i , between t and $t + h$ as:

$$GROWTH_{i,t,t+h} = \Delta y_{i,t,t+h} = \frac{1}{h} \sum_{j=1}^h (y_{i,t+j} - y_{i,t-1+j}) \quad (\text{II.1})$$

For a spell of five years, i.e. $h = 5$, this simplifies as:

$$\Delta y_{i,t,t+5} = \frac{1}{5} (y_{i,t+5} - y_{i,t})$$

The generic form for growth equation is usually obtained from a first-order approximation of the neoclassical growth model (Mankiw, 1995), and one can derive:

$$\Delta y_{i,t,t+h} = \lambda (y_{i,t} - y_i^*)$$

where $y_{i,t}$ is the initial level of log GDP per capita, and y_i^* the long-run (or steady-state) value. The parameter λ accounts for classical conditional convergence. For empirical purposes, the literature generally implicitly assumes that y_i^* can be modeled as a linear function of several variables that define the structure of the economy (Bekaert, Harvey, and Lundbald, 2005). In line with previous works of Cecchetti, Mohanty, and Zampolli (2011) and Benczur, Karagiannis, and Kvedaras (2019), the present paper estimates the following econometric model:

$$\Delta y_{i,t,t+h} = \lambda y_{i,t} + \beta DEBT_{i,t} + \theta \mathbf{X}_{i,t} + \mu_i + \gamma_t + \varepsilon_{i,t,t+h} \quad (\text{II.2})$$

with time- and country-specific fixed effects (μ_i and γ_t respectively). $DEBT_{i,t}$ consists of various breakdowns of the financial variable by fund users. $\mathbf{X}_{i,t}$ includes the various control variables presented in section 2.1. Overall, the use of two-way fixed effects regression models allows to systematically control for time and country heterogeneity.

3.2 Parameter Estimation and Caveats

Economic growth is volatile in the short-run. Thus, the dependent variable is specified as future average growth of GDP over the h years forward. Following the growth literature, the present paper works with $h = 5$. Such length purges out business-cycle fluctuations. The latter short-run fluctuations will be studied with more depth in section 4. The forward nature allows to avoid endogeneity that would arise from simultaneity.

Other variables are not averaged out over 5-year non-overlapping spells. First, evi-

dence suggests that the specific anchoring for each spell is not neutral, inducing some form of aggregation bias. Secondly, non-overlapping spells would substantially reduce the number of effective periods to a few. Therefore, this study retains annual observation in order to take full advantage of the time-series variation available in recent samples (Baltagi, Demetriades, and Law, 2009; Cecchetti, Mohanty, and Zampolli, 2011; Benczur, Karagiannis, and Kvedaras, 2019). The overlapping structure of the observation creates a moving average process to the error term. The use of robust clustered standard errors to compute the tests on the coefficient estimates mitigates this issue (White, 1980; Cecchetti, Mohanty, and Zampolli, 2011).

The regression equation is dynamically specified. The model can be consistently estimated by the fixed effect OLS estimator as long as the number of periods T grows to infinity. However, in finite sample, the estimates may be biased downward of order $O(1/T)$ (Nickell, 1981). A host of alternative estimators have been suggested to overcome this finite sample bias. As no suited external instruments have emerged in the finance-growth literature (for an overview, see Bazzi and Clemens, 2013), dynamically specified models are generally estimated with internal instruments estimators.

Whether it is the estimator of Anderson and Hsiao (1981) or the popular System-GMM estimator of Arellano and Bond (1991) and Arellano and Bover (1995), such techniques are subject to drawbacks. Several studies have carried out serious concerns with these internal instrument techniques.⁶ The instruments are usually very weak in the typical macroeconomic panel setups. To shed light on the internal instrument's quality, Blundell and Bond (2000) suggest a simple test, reported in Table B.5 in the Appendix. Overall, this preliminary test suggests that the internal instruments available in the present sample, like most macro-finance empirical setup, are at best very weak, if not totally irrelevant. The estimates based on these internal instruments would be severely biased

6. See, among others, Durlauf, Johnson, and Temple (2005): “many applications of instrumental variable procedures in the empirical growth literature [are] undermined by the failure to address properly the question of whether these instruments are valid”; Roodman (2009a, p.1): “Judging by current practice, many researchers do not fully appreciate that popular implementations of [System-GMM] estimators can by default generate results that simultaneously are invalid yet appear valid. The potential for type-I errors—false positives—is therefore substantial”; and Bazzi and Clemens (2013): “Concern has intensified in recent years that many instrumental variables used in widely-cited growth regressions may be invalid, weak, or both”.

(for a survey, see Stock, Wright, and Yogo, 2002; Murray, 2006; Mikusheva, 2013). Monte Carlo simulations show that for a moderate sample size of $T = 30$ and $N = 20$, the fixed effect OLS estimator performs fairly well. At least as well as or better than the instrumental variable estimators (Judson and Owen, 1999; Cecchetti, Mohanty, and Zampolli, 2011). Given that the sample covers 48 years, the present paper resorts to the fixed effects OLS estimator. One final advantage of this estimator over other internal instrument estimators is its transparency.

Finally, as is now common in this line of research, control variables enter the model with a lag treatment to avoid endogeneity stemming from simultaneous relationships. However, this does not fully rule out simultaneity bias as expectations about future growth conditions may affect the current financing decisions. Lag treatment is intended to dampen this bias as much as possible. As with previous empirical studies, the reading of the empirical results calls for caution. While the methodology may mitigate issues of reverse causality, a firm causality claim remains beyond the scope of this design.

3.3 Delving Into the Financial Structure

Are high levels of total debt concerning? Does the financial recipient matter in promoting output growth? Based on a panel of 34 countries ranging from 1970 up to 2018, Table II.1 reports OLS Fixed Effects (OLS-FE) estimates of several financial structure decomposition. All controls have the expected sign and are mostly statistically significant at conventional levels. Due to convergence, the initial real GDP has an expected negative sign. Specifically, the rate of convergence is 5% per year, consistent with previous findings (see Cecchetti, Mohanty, and Zampolli, 2011). Government consumption has an insignificant negative association with growth and captures the cyclical stabilization of the fiscal policy. Omitting this variable could lead to incorrectly conclude that public debt harms growth. The inflation rate is negatively associated with medium-run growth, consistent with the early findings of Rousseau and Wachtel (2002). Trade openness enhances future growth, so does human capital, albeit not in a significant fashion. Finally, the dependency ratio has a strongly negative and statistically significant impact on growth, measuring the aging structure of the economy. The aging population,

Table II.1 – Financial structure and medium-run future growth, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)
Log(Total debt/GDP)	-0.554 (0.776)			
Log(Public debt/GDP)		0.939** (0.380)		0.688* (0.365)
Log(Private credit/GDP)			-1.822*** (0.560)	-1.405** (0.541)
Log(initial GDP/capita)	-5.890*** (1.866)	-6.015*** (1.377)	-5.204*** (1.754)	-5.400*** (1.380)
Log(Government consumption)	-0.513 (0.979)	-0.698 (1.199)	-0.818 (1.181)	-0.913 (1.288)
Log(Inflation rate)	-0.435*** (0.122)	-0.381*** (0.105)	-0.485*** (0.121)	-0.435*** (0.102)
Log(Trade openness)	2.295*** (0.772)	1.539* (0.876)	2.616*** (0.721)	2.064** (0.822)
Log(School)	1.260 (1.202)	1.060 (1.354)	0.847 (1.177)	0.749 (1.278)
Log(Dependency ratio)	-5.102*** (1.864)	-5.795*** (1.690)	-4.863*** (1.746)	-5.383*** (1.658)
Log(Saving)	1.375* (0.770)	2.323*** (0.746)	1.092 (0.697)	1.732** (0.740)
Observations	1,026	1,026	1,026	1,026
R^2	0.441	0.477	0.481	0.499
Number of countries	34	34	34	34
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes: This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

along with rising dependency, is an essential factor to take into account as it has the potential to dampen growth dynamics. Omitting this variable would lead once again to incorrectly conclude that public debt and private credit have a negative impact on subsequent growth.

In what follows, none of the estimates controls for banking crises. This first section's focus rests on the overall effect of private credit and public debt on medium-run future growth. Section 4 is dedicated to the business cycles' impact and concentrates on disentangling normal cycles from the ones associated with banking crises. For now, the estimating strategy rests on previous evidence suggesting that private credit and public

debt are influencing factors for banking crises, and considers that banking crises are a byproduct of the financial sector (Jordà, Schularick, and Taylor, 2016). Therefore, at this stage, capturing banking crises independently from the overall effect would most certainly be misleading as it would lead to underestimating the overall impact.

The first column of Table II.1 provides estimates for total debt and returns a slightly negative but insignificant coefficient estimate. Total debt is not a good predictor of future growth. Thus, high total debt levels do not predict a significantly lower level of medium-run growth. Does this necessarily indicate that both private credit and public debt are irrelevant to growth? Certainly not. As shown in columns (2) to (4), the absence of a statistically significant effect is the consequence of effects operating in opposite directions. Indeed, splitting the overall amount of debt into public debt and private credit unveils that the structure of debt matters as there are some user-specific effects. The damaging or enhancing property of the financial sector on growth depends on the specificity of the fundholder.

Based on the coefficient estimates from column (2), increasing the public debt ratio by one standard deviation from its mean would imply an additional half a percentage point of growth per year for the subsequent five years.⁷ Considering the point estimate for private credit displayed in column (3), a similar increase by one standard deviation from the mean suggests a sizable decline in output growth of 0.7 percentage point per year for the following five years.⁸ In other words, by five years, per capita GDP is 3.5% lower.

Splitting total debt in column (4) indicates that joint estimation tends to mitigate each response. The coefficients estimates for private credit and public debt slightly loose in magnitude and precision when jointly estimated. This evidence is crucial as it underlines that studying private credit or public debt in isolation might lead to an oversized estimated effect.

To further understand the financial structure, Table II.2 extends the analysis by in-

7. A one standard deviation increase of the ratio of public debt to GDP from its mean (56% to 87%) is a log difference of 0.44, which is then multiplied by the coefficient estimate for public debt.

8. A one standard deviation increase of the ratio of private credit to GDP from its mean (113% to 166%) is a log difference of 0.38, which is then multiplied by the coefficient estimate for private credit.

Table II.2 – Financial structure changes and medium-run future growth, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)
Δ Total debt (per capita)	-3.875*** (1.305)			
Δ Public debt (per capita)		-0.352 (0.491)		-0.470 (0.462)
Δ Private credit (per capita)			-2.909*** (0.858)	-2.967*** (0.863)
<i>Other parameter estimates omitted for clarity</i>				
Observations	1,020	1,020	1,020	1,020
R^2	0.456	0.441	0.454	0.455
Number of countries	34	34	34	34
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes: This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

investigating the flows in the various per capita debt levels. Indeed, there is evidence that rapid change in credit to the private sector can trigger higher growth volatility. An increase in the total debt level is associated with a reduction in the future average annual growth rate. Columns (2) to (4) proceed just as in Table II.1, breaking down total debt by fundholders. Columns (2) and (3) show that an increase in public debt is not significantly associated with future growth, while the increase in credit allocated to the private sector has an adverse effect on future output growth. The joint estimation leaves the conclusion unchanged in column (4). Tables II.1 and II.2 suggest that whether it is a high level of credit to the private sector or an increasing level, both exert a growth-damaging effect on future growth.

Previous evidence suggests that all private recipients do not exert the same economic functions. Beck et al. (2012) concludes that household credit is considerably less productive than firm credit when it comes to promoting output growth. The dataset allows splitting the private sector's liabilities into credit to the corporate non-financial sector and the household sector. However, granularity comes at a cost. The lack of country and time coverage within the split does not allow for a sound estimation on the over-

Table II.3 – Financial structure and medium-run future growth, various sub-groups of countries, OLS Fixed Effects estimates.

Sub-group:	Non-Advanced		Advanced		EU		Eurozone	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS-FE								
Log(Public debt/GDP)	1.033** (0.432)	0.218 (0.527)	-0.155 (0.460)	-0.278 (0.507)	0.393 (0.352)	0.862** (0.366)	0.764 (0.694)	0.893 (0.501)
Log(Private credit/GDP)	-0.266 (0.957)		-1.246*** (0.412)		-1.394*** (0.381)		-1.655*** (0.483)	
Log(Firm credit/GDP)		-0.274 (1.159)		-0.189 (0.563)		0.054 (0.702)		-0.566 (0.787)
Log(Household credit/GDP)		-0.128 (0.264)		-1.233** (0.474)		-1.202** (0.473)		-1.178** (0.505)
<i>Other parameter estimates omitted for clarity</i>								
Observations	432	257	594	541	490	434	330	291
R^2	0.517	0.667	0.756	0.769	0.776	0.798	0.840	0.856
Number of countries	16	16	18	18	16	16	10	10
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. See the Data appendix page 262 for more details on subsample composition. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

all sample. Advanced economies and European Union members remain well preserved on this account. However, for Non-advanced economies, further splitting private credit imply a loss of almost half of the total observations.

Consequently, Table II.3 provides estimates for this additional split focusing on smaller sub-samples of countries. For each subsample, Table II.3 provides first the joint estimation of public debt and private credit as in Table II.1, then further split private credit into household and firm credit.

Column (1) shows that Non-advanced economies are driving the positive association between public debt and growth. Private credit does not seem to exert a significant impact on future growth. Such evidence emphasizes a great deal of heterogeneity in the finance-growth nexus at various stages of development.

Further splitting private credit in column (2) implies a dramatic loss of observations, which alter the conclusions of column (1). This justifies the focus of the subsequent columns into more stable subsamples. The remaining columns emphasize the robustness of the estimate of an adverse effect of private credit on future output growth. Increasing the ratio of private credit over GDP by one standard deviation from its mean implies

a sizable decline in output growth ranging from -0.35 to -0.55 percentage point per year for the following five years. This evidence is consistent with previous findings of Benczur, Karagiannis, and Kvedaras (2019).

Estimates from Table II.3 suggest the negative impact of private credit on growth is almost exclusively due to household credit. This result is in line with previous empirics (Beck et al., 2012). If the European Union members were to recover their early 2000s household debt levels (on average 40% in 2000, as compared to 62% of GDP in 2017), the estimation suggests that output growth could be higher by more than half a percentage point per year for the subsequent five years. Public debt no longer appears as a good predictor of future growth. The point estimates in Table II.3 indicate that inference based on this specification is not robust for public debt.

Not all types of finances matter: some are substantially more detrimental to growth than others. The crusade on public debt seems misguided in light of this evidence. If policymakers ought to be concerned about rising levels of private liabilities, it appears that household credit should be carefully held at lower levels.

3.4 Nonlinearities and the Interplay of Private Credit With Public Debt

Results in the previous sections supported that private sector credit tends to be a drag on future growth, mainly due to household credit. Measuring public debt's impact on growth has revealed more tricky and has lead to a usually positive yet neither strong nor statistically significant coefficients. One might be tempted to conclude to the irrelevance of public debt to growth. Before coming to such a conclusion, this section delves into potential nonlinearities and further analyzes the structure's complexity through quadratic specifications, Hansen panel threshold regressions, interaction terms and an Autoregressive Distributed Lag model.

3.4.1 Quadratic Estimates

Frist, consider modifying expression (II.2) into a quadratic model allowing for a non-monotonic relation between public debt and medium-run future growth:

$$\begin{aligned} GROWTH_{i,t,t+h} = & \beta_1 PUBLIC_{i,t} + \beta_2 (PUBLIC_{i,t})^2 + \beta_3 \ln(PRIVATE_{i,t}) \\ & + \lambda y_{i,t} + \theta \mathbf{X}_{i,t} + \mu_i + \gamma_t + \varepsilon_{i,t,t+h} \end{aligned} \quad (\text{II.3})$$

with time- and country-specific fixed effects (μ_i and γ_t respectively). The two variables $PUBLIC_{i,t}$ and $(PUBLIC_{i,t})^2$ are the ratio of public debt to GDP and its square term, respectively. $\mathbf{X}_{i,t}$ includes the various control variables presented in section 2.1.

The method proposed by Sasabuchi (1980) and developed by Lind and Mehlum (2011), henceforth SLM test, is suited to ascertain the location and relevance of the extremum point. It involves determining whether the marginal effect of public debt on growth is significantly different from zero and positive at a low level but negative at a high level, within-sample:

$$\begin{aligned} H_0 : & (\beta_1 + 2\beta_2 PUBLIC_{\min} \leq 0) \cup (\beta_1 + 2\beta_2 PUBLIC_{\max} \geq 0) \\ H_1 : & (\beta_1 + 2\beta_2 PUBLIC_{\min} > 0) \cup (\beta_1 + 2\beta_2 PUBLIC_{\max} < 0) \end{aligned}$$

The first column in Table II.4 provides results for this exercise. Based on the overall sample, the estimates in column (1) suggest that public debt has growth-enhancing properties below a threshold located at 115% of GDP, above which it becomes a drag on medium-run growth. Indeed, with a low p -value of 0.03, the SLM test indicates a well-identified threshold.

Figure II.4 presents the marginal impact for public debt displaying diminishing returns. Interestingly, estimates based on alternatives sample indicates that such non-linearity is not driving public debt for advanced economies (see Table B.9 in the appendix). On the other hand, non-advanced economies display a precisely estimated threshold at 107% of GDP. Focusing on the European Union members reveals a much lower threshold at 65% of GDP, also precisely estimated.

Table II.4 – Nonlinearities in the financial structure, medium-run future growth.

Nonlinearity:	QUADRATIC		HANSEN PANEL THRESHOLD		
	(1)	(2)	(3)	(4)	(5)
OLS-FE					
Private credit/GDP		-3.394** (1.304)			
(Private credit/GDP) ²		0.666 (0.403)			
Private credit/GDP < τ				0.976** (0.393)	
Private credit/GDP $\geq \tau$				-1.192*** (0.194)	
Private credit/GDP IF Public debt/GDP < τ					-1.321*** (0.195)
Private credit/GDP IF Public debt/GDP $\geq \tau$					-2.130*** (0.247)
Log(Private credit/GDP)	-1.655*** (0.557)		-1.523*** (0.228)		
Public debt/GDP	2.784** (1.315)				
(Public debt/GDP) ²	-1.216** (0.503)				
Public debt/GDP < τ			1.230*** (0.313)		
Public debt/GDP $\geq \tau$			0.195 (0.254)		
Log(Public debt/GDP)		0.626* (0.367)		0.367*** (0.142)	0.567*** (0.144)
<i>Other parameter estimates omitted for clarity</i>					
Threshold	115%**	255%	101%*	0.69%**	99%*
90% CI	[67%-136%]	–	[100%-103%]	[65%-72%]	[98%-100%]
SLM test (<i>p</i> -value)	0.03	0.35	–	–	–
Threshold-effect <i>F</i> -test (<i>p</i> -value)	–	–	0.06	0.03	0.08
Observations	1,026	1,026	825	825	825
<i>R</i> ²	0.498	0.509	0.558	0.569	0.563
Number of countries	34	34	25	25	25
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes: This table reports the results of a set of fixed effects regressions. The dependent variable is the average real GDP per capita growth rate. The SLM test provides *p*-value for the relevance of the estimated threshold for the quadratic estimates. The sample for the HPT regressions is smaller as the estimation procedure requires a balanced panel. See the appendix, Tables B.9, B.10, B.11, B.12 and B.13 for more details and sub-sample regressions. Robust standard errors in parentheses. *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

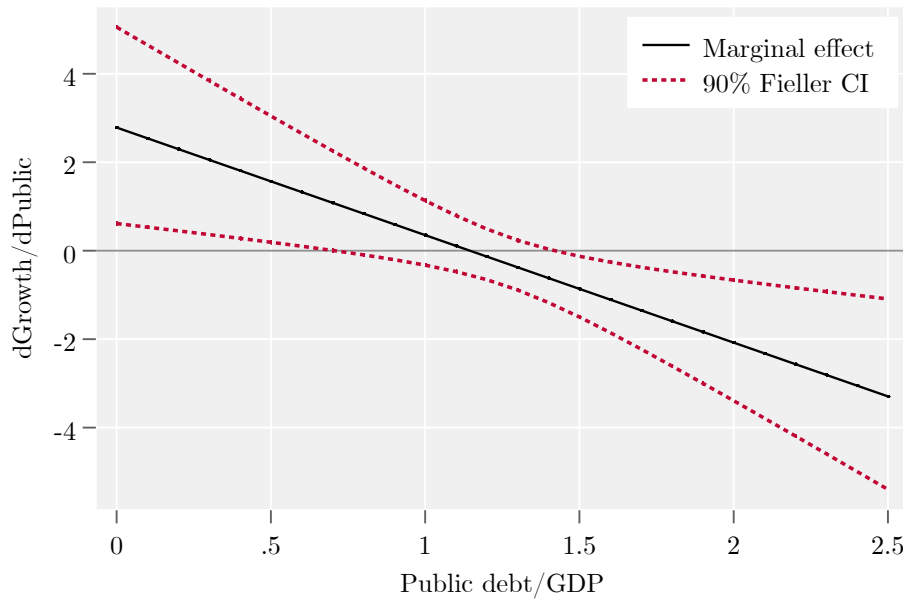


Figure II.4 – Marginal effect of public debt on growth. Estimation from column (1) in Table II.4. The dotted colored lines represent 90% Fieller confidence area.

Overall, quadratic estimates suggest that the widely spread thresholds regressions, prove to be sample-driven. Depending on the specification, the public debt thresholds range from 65% up to 115% of GDP (if not the absence of a threshold). Far from demonstrating that public debt is either enhancing or damaging, this exercise provides evidence that a simple non-linear specification does not drive public debt impact on medium-run output growth. This evidence is in line with the work of Panizza and Presbitero (2013), inviting policy makers to much caution in devising policies based on threshold evidence.

The first column of Table II.4 also provides an additional robustness check on the adverse effect of private credit. Coefficient estimates remain qualitatively similar to previous findings and remain consistent over various subsamples. To further probe the adverse effect of private credit on subsequent growth, column (2) run the same exercise as in column (1), this time for private credit. The estimates confirm the growth-damaging impact and the absence of an inverted U-shape. The evidence is robust to various subsample and suggests once again that the adverse effect is more pronounced for advanced economies (see Table B.10 in the appendix).

3.4.2 Hansen Panel Threshold Estimates

The last three columns of Table II.4 provides additional estimates based on a fixed-effect panel threshold method to empirically investigate non-monotonicity in the financial structure (Hansen, 1999). This technique has the advantage of not imposing a priori restriction on the relationship, as would a quadratic form. It is suited to estimate a structural break between financial depth and growth, as follow:

$$\begin{aligned}\Delta y_{i,t+k} &= \lambda y_{i,t} + \beta_1 (\psi_{i,t} < \tau) PC_{i,t} + \beta_2 (\psi_{i,t} \geq \tau) PC_{i,t} + \gamma \mathbf{x}_{i,t} + \nu_{it+k} \\ \nu_{it+k} &= \mu_i + \lambda_{t+k} + \varepsilon_{i,t+k}\end{aligned}$$

where the threshold variable $\psi_{i,t}$ represents successively the ratio of private credit to GDP, the ratio of public debt to GDP, and the GDP per capita (in logs). τ is the threshold parameter that divides the equation into two regimes with β_1 and β_2 as respective coefficients (for more details, see Hansen, 1999; Wang, 2015)

The point estimates from column (3) of Table II.4 emphasize that public debt has rather beneficial effects on growth performance, which fades at higher levels. The threshold is located at around 100% of GDP. The estimates on various subsamples confirm this intuition, with a threshold estimated around 100%. The Hansen panel threshold estimate supports the existence of non-linearities. However, the coefficient associated with public debt above the threshold usually remains positive, with a loss in magnitude. In some samples, it is no longer statistically significant. Yet, none of the estimates support a detrimental effect at higher levels.

These findings partially contradict the previous evidence derived from a quadratic form. However, the latter estimates based on a quadratic specification might be misleading as they impose a functional form on the relationship. As for private credit as a control variable, these estimates once again corroborate the overall adverse association with subsequent growth.

Now focusing on the estimates from column (4), the estimates suggest a relevant threshold for private credit located at 68% of GDP. The overall damaging impact seems to conceal some non-linearity. However, sub-sample estimates emphasize that this turn-

ing point is highly sensitive, as none of them corroborate this threshold. In the other estimations, both regimes display adverse effects.

Finally, column (5) of Table II.4 reports the ultimate exercise, which gives a taste of the investigations in the next part focusing on business-cycles. The Hansen panel threshold model is specified with the ratio of private credit to GDP as a regime dependent variable with the ratio of public debt to GDP as a threshold variable. This specification aims at assessing whether high public debt levels enhance the adverse effect of private credit on subsequent growth.

The estimate suggests that the negative association between private credit and growth is 60% higher when public debt is beyond 100% of GDP. Subsample estimates confirm that the adverse effect is more intense when public debt is high, with thresholds around 100%. This evidence confirms from a medium-run perspective the conclusions from Jordà, Schularick, and Taylor (2016) who find that private credit surges in time of high public debt exert a growth-damaging impact at the business cycle level.

3.4.3 Interplay Estimates

So far, this paper has investigated private credit and public debt either in isolation or jointly. Table II.5 further investigate potential sources of interplay. Looking into specifics of each economy unveils substantial gaps between debt levels. Each type of debt does not exactly have the same perimeter from one country to another. Moreover, there is no overall correlation between private credit and public debt levels (see Table B.4 in the appendix). Thus, high public debt levels can match either low or high private credit levels. Table II.5 investigates whether the effect of debt on future economic growth depends on either private or public debt levels or changes.

To investigate the interplay between private credit and public debt, expression (II.2) is rewritten in the following form:

$$\begin{aligned}
 GROWTH_{i,t,t+h} = & \beta_1 PUBLIC_{i,t} + \beta_2 PRIVATE_{i,t} + \beta_3 (PUBLIC_{i,t} \times PRIVATE_{i,t}) \\
 & + \lambda y_{i,t} + \theta \mathbf{X}_{i,t} + \mu_i + \gamma_t + \varepsilon_{i,t,t+h}
 \end{aligned}
 \tag{II.4}$$

Table II.5 – Interplay in the financial structure, medium-run future growth, OLS Fixed Effects estimates.

Nonlinearity: OLS-FE	INTERACTION				
	(1)	(2)	(3)	(4)	(5)
Log(Private credit/GDP)		-2.036*** (0.540)			
Log(Public debt/GDP)		0.617* (0.351)			
Private credit/GDP	-0.885* (0.522)			-1.319*** (0.407)	
Public debt/GDP	2.168** (0.993)		0.924 (0.607)		1.156* (0.619)
Δ Private credit/GDP			-6.234*** (2.167)	1.432 (2.620)	
Δ Public debt/GDP					0.612 (1.020)
Interaction term ^{α}	-1.543** (0.698)	-0.771** (0.327)	5.343 (3.317)	-5.068** (2.439)	-0.895 (2.092)
<i>Other parameter estimates omitted for clarity</i>					
Observations	1,026	1,026	1,014	1,014	1,026
R^2	0.497	0.509	0.471	0.501	0.453
Number of countries	34	34	34	34	34
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes: This table reports the results of a set of fixed effects regressions. The dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. See the appendix, Tables B.15, B.16, B.17, B.18, B.19 and B.20 for more details and sub-sample regressions. (α) The interaction term is the interplay of the two variables above. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

where $PUBLIC_{i,t}$ and $PRIVATE_{i,t}$ are ratios of public debt and private credit to GDP, respectively. Interacting public debt and private credit is expected to shed light on potential interplay. The marginal effect of public debt or private credit can be evaluated by examining the partial derivatives of economic growth for each variable:

$$\frac{\partial GROWTH_{i,t,t+h}}{\partial PUBLIC_{i,t}} = \beta_1 + \beta_3 PRIVATE_{i,t}$$

Column (1) in Table II.5 provides results for this first interaction exercise. The magnitude of the various debt/credit coefficients is different from the ones in Tables II.1 and II.3. Due to the added interaction term, the point estimates on debts now returns the

effect of private/public debts on future growth holding the other at zero.

The initial marginal effect of public debt is positive, whereas the initial marginal effect for private credit is negative. The coefficient associated with the interaction term is negative and statistically significant. Further accumulation of either public debt or private credit increases the adverse effect of private credit on growth. Whether public debt is growth-enhancing or not will depend on the current level of private credit. This evidence could explain why public debt is essentially growth-enhancing for non-advanced economies. It also conveys the intuition that private credit exerts its most negative impact when public debt has already reached a high level.

Similarly, column (2) in Table II.5 provides results for the first interaction exercise, this time in with the finance proxy in logs. Once again, it emphasizes the adverse effect of private credit on output-growth. This growth-damaging impact is even more substantial when the economy faces a high public debt ratio.

Jordà, Schularick, and Taylor (2016) find that private credit surges in time of high public debt exert a growth-damaging impact at the business cycle level. As a reappraisal, column (3) investigates whether this evidence reaches medium-run output growth. The point estimates from column (3) suggest it does not. The coefficient associated with the interaction term is not statistically significant. The lack of a medium-run association should not be surprising. The business cycle evidence suggests that the damaging impact occurs mostly in recessions associated with a financial crisis. Therefore, one should not expect private credit surges in time of high public debt to be systematically associated with lower medium-run output growth.

Column (4) focuses on the impact of an increase in private credit in a deepened private credit environment. The interaction term is both negative and statistically significant. Together with the negative point estimate for private credit, the evidence further suggests that private credit surges in a financially deepened environment amplify the adverse effect of private credit. This evidence is in line with Jordà, Schularick, and Taylor (2016), who find that private credit surges in a financially deepened environment are a good crisis predictor. The medium-run evidence, however, lacks robustness (see Table B.17 in the appendix for various sub-sample estimates).

Finally, column (5) investigates the impact of an increase in public debt in a high public debt environment. The point estimates fail to find a significant interaction. This evidence remains consistent with the previous finding from Table II.2, in which public debt variation was not statistically associated with future growth (unlike private credit surges).

3.4.4 Autoregressive Distributed Lag Estimates

This first part of the paper focuses on medium-run evidence, whereas the second part emphasizes the business cycle aspect of financial expansion. This penultimate subsection provides a transition. It implements a model that incorporates both short-run and long-run effects. Indeed, the averaging in the growth regression conceals the dynamic relationship between financial depth and growth at different time frequencies. This first glance at multiple horizons sets the basis for the subsequent business cycle analysis in section 4.

The method consists in a panel error-correcting model, where short-run and long-run effects are estimated jointly from a general Autoregressive Distributed Lag (ARDL) model. Several estimation techniques, both consistent and efficient can be conducted using standard tests (Pesaran and Smith, 1995; Pesaran, 1997; Pesaran, Shin, and Smith, 1999). Following Loayza and Ranci ere (2006), the dynamic heterogeneous panel regression can be incorporated into the error-correction model using the ARDL(p, q) technique, where p is the lag of the dependent variable, and q is the lag of the independent variables as follows:

$$\Delta y_{it} = \sum_{j=1}^{p-1} \gamma_j^i \Delta y_{it-j} + \sum_{j=0}^{q-1} \delta_j^i \Delta X_{it-j} + \psi^i \left[y_{it-1} - (\beta_0^i + \beta_1^i X_{it-1}) \right] + \varepsilon_{it} \quad (\text{II.5})$$

where y is the GDP per capita growth rate, X is a set of independent variables including financial development, γ and δ represent the short-run coefficients of lagged dependent and independent variables respectively, β are the long-run coefficients, and ψ is the coefficient of speed of adjustment to the long-run equilibrium. The subscripts i and t represent country and time indexes, respectively. The term in the square brackets

Table II.6 – Short-run and long-run growth effects of financial depth, Pooled Mean Group estimates of an ARDL model.

Sample:	FULL			ADVANCED	NON-ADV.
Estimator: PMG	(1)	(2)	(3)	(4)	(5)
LONG-RUN COEFFICIENTS					
Private credit/GDP	-1.900*** (0.466)		-2.792*** (0.469)	-3.334*** (0.553)	-2.589*** (0.973)
Public debt/GDP		-0.012 (0.181)	-0.142 (0.150)	-0.243 (0.195)	1.808*** (0.521)
<i>Other parameter estimates omitted for clarity</i>					
ERROR CORRECTION COEFFICIENTS					
ψ	-0.714*** (0.051)	-0.692*** (0.052)	-0.722*** (0.056)	-0.817*** (0.070)	-0.637*** (0.074)
SHORT-RUN COEFFICIENTS					
Δ Private credit/GDP	-4.520*** (1.251)		-4.647*** (1.313)	-4.995*** (1.938)	-4.329** (2.316)
Δ Public debt/GDP		-0.103 (1.105)	2.471 (2.067)	1.377 (1.567)	5.249 (3.676)
<i>Other parameter estimates omitted for clarity</i>					
Hausman test (p -value)	0.65	0.25	0.32	0.13	0.59
Number of countries	34	34	34	18	16
Observations	1341	1434	1341	810	531

Notes: This table report Pooled mean group estimates (PMG). For mean group and dynamic fixed effects, see Tables B.21-B.23 in the Appendix. All estimates are controlling for country and time effects. All control variables are specified in natural logs. Under the null hypothesis of the Hausman test, PMG is more efficient estimation than MG. In the present table, the Hausman test is indicating that PMG is consistent and efficient. The lag structure is ARDL(1, 1, 1, 1, 1). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

of equation (II.5) contains the long-run growth regression, which is derived from the following equation:

$$y_{it} = \beta_0^i + \beta_1^i X_{it} + \mu_{it} \quad \text{where } \mu_{it} \sim I(0)$$

Table II.6 presents the results of the estimation of both long-run and short-run parameters linking per capita GDP growth with private credit and public debt. The results are obtained using the Pooled mean group (PMG) estimator. As suggested by the specification tests, the PMG estimator is preferred given its gains in consistency and efficiency over other error-correction estimators.⁹

9. For comparison purposes, section 3.5 on page 289 in the appendix also reports estimates based on the Mean Group and Dynamic Fixed Effect estimators. Overall, the estimates are roughly in line with those presented here.

Columns (1) to (3) are based on the full sample and sequentially incorporate the two financial proxies. Whether it is on a short-run or long-run horizon, private credit exerts an adverse effect on subsequent growth. This effect is larger in the short-run. The last two columns provide estimates based on a split of the overall sample in two non-overlapping sub-samples. The results are qualitatively similar to those found on the full sample. The long-run and short-run coefficient signs and statistical significance remain unchanged, except for public debt, which displays a statistically positive coefficient for non-advanced economies. This result is in line with the evidence from Table II.3, showing that non-advanced economies are driving the positive association between public debt and growth.

These ARDL estimates for private credit are not in line with previous evidence from Loayza and Rancière (2006). Although the method is identical, the samples substantially differ. Most noticeably, their time coverage ends in 2000, whereas the sample mobilized here goes up to 2015. However, the present evidence is in line with the recent work of Samargandi, Fidrmuc, and Ghosh (2015) based on a more recent sample of middle-income economies.

3.4.5 Controlling for Financial Crises

As a final assessment, this last section focuses on the occurrence of financial crises. Indeed, the magnitude of the short-run coefficient on private credit compared to its long-run effect is usually attributed to banking crises triggering (Loayza and Rancière, 2006). Therefore, before delving into the business-cycle analysis, Table II.7 runs the estimates from Table II.1, this time controlling for banking crises. This exercise is intended to answer the following question: how much of the adverse effect of private credit is due to financial crises triggering?

The estimates reveal that financial distress is a substantial channel underlying the adverse effect of private credit. The coefficients for public debt and private credit both loose in magnitude and significance. As expected, banking crises are associated with much lower output growth. Each year spent in crisis dampens output growth by an average of 1 percentage point per year over the subsequent five years. This impact

Table II.7 – Financial structure and medium-run future growth, controlling for banking crisis, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)
Log(Total debt/GDP)	-0.204 (0.703)			
Log(Public debt/GDP)		0.758** (0.359)		0.610* (0.350)
Log(Private credit/GDP)			-1.295** (0.561)	-0.952* (0.536)
Banking crisis	-6.210*** (1.192)	-5.566*** (1.019)	-5.158*** (1.097)	-4.891*** (0.995)
<i>Other parameter estimates omitted for clarity</i>				
Observations	1,026	1,026	1,026	1,026
R^2	0.441	0.477	0.481	0.499
Number of countries	34	34	34	34
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes: This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Banking crises are accounted for using an indicator taking the value of $X/5$ corresponding to the number of banking crises X in the subsequent 5 years, as identified by Laeven and Valencia (2018). If one financial crisis occurs in the subsequent five years, the coefficient should be multiplied by 1/5 to assess the yearly impact in terms of percentage point over the following five years. See section 4.1 for more details on systemic financial crisis. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

is undoubtedly undersized due to the time fixed effects capturing part of the Global Financial Crisis of 2008.

The consequences of financial and economic crises should be further investigated to provide a clearer picture of the interplay between private credit and public debt over the business cycle.

*
* *

Summing up, this first section, focussing on medium-run evidence, suggests that the financial recipient does matter. Joint estimation of public and private liabilities emphasize that considering them in isolation might lead to oversized estimated effects.

Private credit exerts an adverse effect on medium-run output growth. This conclusion is robust, economically sizable, and in line with recent literature. The negative associa-

tion is higher in the short-run, but still carries-on in the long run. Some non-linearities might drive the nexus, but a simple quadratic specification fails to capture it. Indeed, the results suggest that private credit is harmful even for low levels of financial depth. Rapid accumulation in private credit is also detrimental to future growth.

Splitting private sector's liability suggests that the provision of credit to households explains most of the effect. Therefore, shifting the composition of finance to other credit recipients could promote growth.

Public debt produces an ambiguous effect. Estimates suggest a positive association with future growth at lower levels, mostly for non-advanced economies. However, estimating a common turning point is a tedious task. Estimates are highly sensitive, emphasizing a great deal of heterogeneity.

Interacting private and public liabilities unveil that high public debt levels enhance private credit's adverse effect on subsequent growth. From a medium-run perspective, this evidence confirms the conclusions from Jordà, Schularick, and Taylor (2016) who find that private credit surges in time of high public debt exert a growth-damaging impact at the business cycle level. Governments should contain both private credit and public debt at reasonably low levels in light of this evidence.

These simple specifications are indeed not unveiling the broad complexity and heterogeneity at work. Nonetheless, they provide grounds for further exploring this line of research. The paper now turns to lower horizon analysis for insight as to what happens through the cycles.

4 Private Credit and Public Debt Over The Business Cycles

So far, the present paper has provided a body of evidence that public debt and private credit interact and have a different impact on medium-run output growth. Hence, this study now turns to the analysis of business cycle interaction to seek a clearer picture of the interaction's underlying chain of effects. How do private credit and public debt borrowing evolve and interact over the cycle? Moreover, the previous section empha-

sizes that financial distress is a substantial channel. How does the behavior of private credit and public debt differs between normal cycles and those associated with financial distress? To answer these questions, the present paper closely follows the methodology of Jordà, Schularick, and Taylor (2016) and complement their seminal work on three fronts.

First, the present paper intends to focus on more recent observations, starting in 1970 and ranging up to 2018. This choice is motivated by the lack of certainty regarding the continuity and consistency of the financial sector when reaching as far back as two centuries. Indeed, “the 1970s mark a major break in the dynamics of aggregate debt” (Jordà, Schularick, and Taylor, 2016, p.58). Moreover, the response to crises two centuries ago most certainly far different from more contemporaneous responses. Such shorter time coverage should mitigate these concerns.

Second, recent observations allow for the inclusion of additional recessions, especially the Great Financial Crisis. Finally, focusing on contemporaneous time coverage also allows for incorporating additional countries. Therefore, this study investigates private credit and public debt on a larger sample of economies over more recent business cycles. Overall, the present paper investigates similar questions as Jordà, Schularick, and Taylor (2016) following their methodology, but with a different data scope.

Section 4.1 provides a first assessment of the association of private credit and public debt with the occurrence of financial crises. After an overview of the local projection statistical design in section 4.2, the subsequent sections investigates the role of private credit and public debt on the depth of the recession and the speed of the recovery. As a first step, private credit and public debt are considered distinctly. Section 4.3 investigates the aftermath of private credit booms. Then, section 4.4 examines the effect of low fiscal capacity when public debt is high. Finally, the study combines the two preceding approaches to focus on the interplay of private credit and public debt in section 4.5.

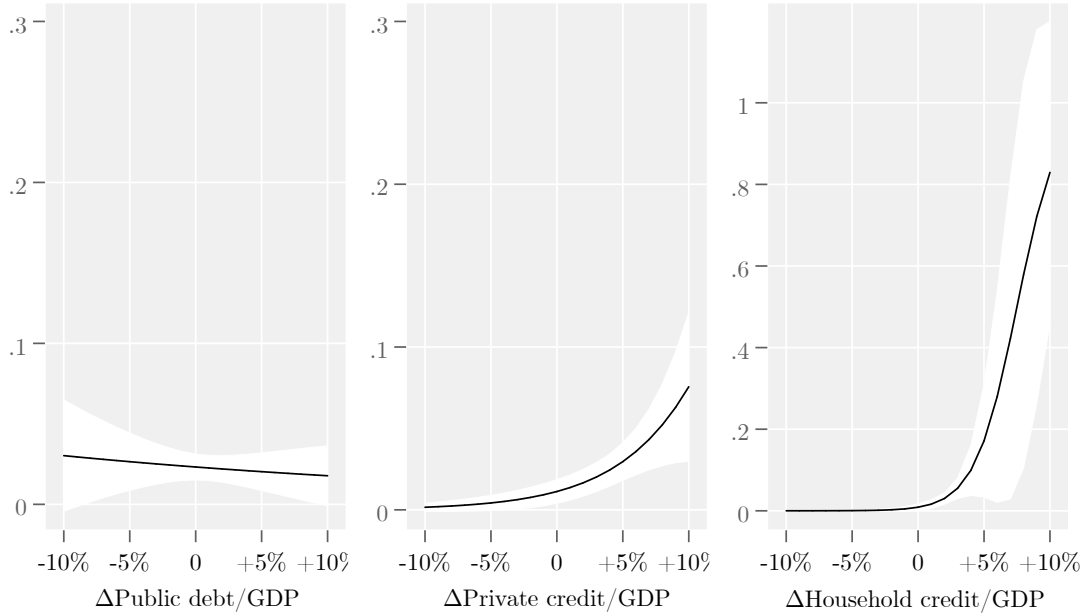


Figure II.5 – Probability of financial crisis, logit estimates. Predictive margins from the logit model classifiers presented in Table II.8 where the dependent variable is the financial crisis event dummy, and the regressors are specified as a five-year moving average.

4.1 Private Credit and Public Debt as Crisis Predictors

Jordà, Schularick, and Taylor (2016) provide evidence that private credit and public debt are not behaved equally when it comes to triggering financial crises. Hence, is private credit or public borrowing the greater risk to financial stability?

To provide an overview to this question, this section builds on the classification framework developed in Schularick and Taylor (2012), with a probabilistic model that specifies the log-odds ratio of a financial crisis event occurring in country i , in year t , as a linear function of lagged controls X_{it} , including changes or levels of the various financial sector proxies and allows for country fixed-effects:¹⁰

$$\log \left(\frac{\mathbb{P}[Crisis_{it} = 1 | X_{it}]}{\mathbb{P}[Crisis_{it} = 0 | X_{it}]} \right) = \beta_{0i} + \beta_1 X_{it-1} + \varepsilon_{it} \quad (\text{II.6})$$

10. As the intent of this first subsection is to introduce the business cycle analysis, the approach adopted here is a relatively simple static logit following Schularick and Taylor (2012) and Jordà, Schularick, and Taylor (2016). Naturally, an in-depth study of crisis predictors would undoubtedly benefit from a dynamic specification on a quarterly database as in Bismans and Majetti (2013).

Table II.8 – Financial crisis predictive ability, Logit models.

Classifier logit model	(1)	(2)	(3)	(4)	(5)
Δ Total credit/GDP (five-year m.a.)	6.56* (2.58)				
Δ Public debt/GDP (five-year m.a.)		-2.72 (5.44)			
Δ Private credit/GDP (five-year m.a.)			19.73*** (5.28)		
Δ Firm credit/GDP (five-year m.a.)				17.92* (8.04)	
Δ Household credit/GDP (five-year m.a.)					63.18*** (17.48)
AUROC	0.62***	0.61	0.72***	0.65**	0.76***
σ	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)
AUROC equality χ_2 test (p -value)					
$H_0 : (*) = \text{GOLD}$	0.03	0.17	0.00	0.01	0.00
$H_0 : (*) = (3)$	0.00	0.06	–	0.33	0.21
Observations	1,165	1,483	1,168	739	749
Country FE	YES	YES	YES	YES	YES

Notes: The table shows logit model classifiers where the dependent variable is the financial crisis event dummy, and the regressors are specified as a five-year moving average. The AUROC significance stars are based on the equality test with the GOLD standard model. The null GOLD standard model with only fixed effects has AUROC = 0.568 (0.04). AUROC equality χ_2 tests are based on sample specific GOLD standard models. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In order to determine the importance of these different criteria, the AUROC method is relevant, even more than simple statistical significance. It measures the model's ability to dissociate the true-positive signals from the false-positive signals, through a measurement between 0 and 1. The closer the AUROC is to 1, the more explanatory variables' informative content permits to predict the occurrence of a crisis accurately. The closer the AUROC is to 0.5, the less information the model provides to predict a crisis.

The dating of financial crises is compiled from Laeven and Valencia (2018). Indeed, Chaudron and Haan (2014) conduce a thorough assessment of the various banking crises databases and conclude that the one assembled by Laeven and Valencia is by far the most accurate.

Table II.8 provides estimates for the logit model (II.6) with the available decomposition of total debt over GDP into public debt, private credit, firm credit and household credit. Explanatory variables are specified as five-year averages changes preceding a systemic

Table II.9 – Financial crisis predictive ability, Logit models.

Classifier logit model	(1)	(2)	(3)	(4)
Δ Private credit/GDP (five-year m.a.)	19.73*** (5.28)	19.40*** (4.86)	41.33** (15.67)	
Δ Public debt/GDP (five-year m.a.)		-17.09* (7.04)		-22.50 (12.94)
Lagged level of private credit/GDP			1.66*** (0.49)	
Lagged level of public debt/GDP				0.09 (0.65)
Interaction term			-14.77* (7.17)	5.39 (13.36)
AUROC	0.73*** (0.05)	0.74*** (0.05)	0.77*** (0.04)	0.67 (0.05)
σ				
AUROC equality χ_2 test (p -value)				
$H_0 : (*) = \text{GOLD}$	0.01	0.01	0.00	0.15
$H_0 : (*) = (3)$	0.21	0.36	–	0.06
Observations	1,168	1,165	1,168	1,165
Country FE	YES	YES	YES	YES

Notes: The table shows logit model classifiers where the dependent variable is the financial crisis event dummy, and the regressors are specified as a five-year moving average. The null model with fixed effects only has AUROC=0.592 (0.05). Interaction term Δ Private credit/GDP \times Lagged level of private credit/GDP, and similarly for public debt. Robust standard errors in parentheses. Robust standard errors in parentheses. Country fixed effects in all models, not reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

financial crisis. Whether levels or changes of the financial variables are investigated, both reveal essentially similar conclusions.¹¹ Private credit stands out as a fairly well identified financial crisis predictor, confirming previous evidence in Schularick and Taylor (2012), and in the recent work of Mathonnat and Minea (2018). Further decomposing shows that household credit has an even stronger predictive ability. Figure 4.1 provides a visual outlook of the estimates emphasizing that public debt changes are not reliable crises predictor, as opposed to private credit changes. Therefore, all types of financial recipients are not equal with regard to triggering financial distress.

Several equality tests complements the AUROC statistics. First, the AUROC is compared to a gold standard corresponding to the AUROC of the model with only country fixed effects. Evidence shows that public debt is the only financial proxy that fails to enhance the predictive ability of the gold standard. Public debt is irrelevant with regard to

11. For an overview of levels as predictors, see Table B.25 in the appendix.

triggering financial crises. The second round of equality tests compares the predictive capacity of the variables respect to private credit in column (3). Private credit changes as a predictor are superior to changes in total credit changes public debt, but statistically equal to changes in firm credit or household credit.

Table II.9 further investigates the predictive ability of private credit. Albeit credit to the households seems to provide a larger predictive capacity, due to observation scarcity, Table II.9 focuses on private credit to ensure a broader coverage. Column (1) reproduces the simple model with the variation of private credit. Including public debt changes in column (2) does not provide additional prediction capacity with regard to the simple specification of column (1). Evidence from column (3), however, suggests that private credit surges occurring when private-sector liabilities are high tend to be an even better financial crisis predictor than the sole private credit surge. Column (4) confirms the public debt's lack of predictive ability.

Summing up, financial crises can be traced back to build-ups in the private financial sector. All the specifications that include private credit changes as a predictor display robust predictive capacity, as emphasized by the equality test of the AUROC with the gold standard. This preliminary investigation confirms Jordà, Schularick, and Taylor (2016) conclusion that high and/or rising public debt levels are not robustly associated with financial crises.

4.2 State-Dependent Local Projections Through the Business Cycles

Properly investigating private credit and public debt over the cycle calls for data on business cycle turning points. As no official data covers the sample, business cycle features are investigated based on the Bry and Boschan (1971) algorithm. The latter generates two auxiliary dummy variables for the timing of peaks and troughs in economic activity for each country in our sample separately. Based on real GDP per capita, a peak is a local maximum, whereas a trough corresponds to a local minimum. By definition, peaks and troughs alternate one another. Consequently, recessions refer to the period between a peak and the subsequent trough. Expansions refer to the period between the trough and the following peak.

Table II.10 – Summary statistics for recessions, private credit, and public debt variables.

	All recessions		Financial recessions		Normal recessions	
Financial recession indicator	0.25	(0.43)	1.00	(0.00)	0.00	(0.00)
Observations	162		40		122	
Normal recession indicator	0.75	(0.43)	0.00	(0.00)	1.00	(0.00)
Observations	162		40		122	
Change in private credit/GDP	1.74	(4.10)	3.91	(3.22)	1.03	(4.13)
Observations	113		28		85	
Change in public debt/GDP	0.31	(6.14)	0.70	(8.76)	0.16	(4.89)
Observations	122		33		89	
Public debt level/GDP	0.50	(0.35)	0.56	(0.43)	0.48	(0.31)
Observations	153		40		113	

Notes: Financial crisis and normal recession indicators are binary 0-1. Changes in private credit and public debt are in percentage points change per year in the prior expansion. Public debt level is the ratio relative to GDP at the business cycle peak. The table display average values. For nonbinary variables, the standard deviation is in parentheses.

Cycles substantially differ, notably with regard to how they end. Some expansion builds up to burst into severe financial crises recessions, while others end up with mild recessions. In order to investigate how the cyclical adjustment of public debt and private credit differs between normal cycles and those that end in severe financial crises, additional business features are introduced. Financial crisis recessions are defined as a recession that happens within a two-year window surrounding a major financial crisis, based on the updated systemic financial crisis database of Laeven and Valencia (2018). Recessions without financial perturbation are referred to as normal recessions.

Table II.10 summarizes the recessions universe since 1970. A quarter of all recessions were financial crises. This proportion is consistent with what Jordà, Schularick, and Taylor (2016) found over their historical coverage for 1870-1970. Private credit grows almost four times as rapidly before financial crisis recessions than before normal recessions. Such evidence is in line with previous findings that a rapid accumulation of private liabilities usually precedes financial distresses. By contrast, public debt does not display such a preceding build-up. Overall, business cycles associated with financial crises are characterized by a much more credit-intensive expansion as compared to normal recessions.

sions. The evidence suggests that the build-up of financial fragility typically occurs on private sector balance sheets, not through the government's debt.

The statistical design relies on the Local Projection (LP) approach introduced in Jordà (2005). The LP technique is suited to estimates impulse responses. It provides an analog to vector autoregression techniques, while being more flexible and less parameter intensive.¹² Indeed, LP is straightforward as it builds on simple regression techniques robust to misspecification, and accommodate experimentation with flexible specifications. Given the available sample, other techniques would be too parametrically intensive.

Although straightforward, this technique requires a bit of notation. Let M denote the cross-section dimension of countries and T the time dimension of the sample. The goal is to estimate how real GDP per capita evolve from the start of the recession to some distant horizon $h = 1, \dots, H$. The framework builds around a peak and measures the change from $t(p)$ to time $t(p) + h$ where $t(p)$ denotes the year t in which the p^{th} peak occurs, unfolding a recession.

Define $y_{it(p)}$ as the real GDP per capita for country $i = 1, \dots, M$ at time $t(p)$. The h period ahead change, defined as the percentage point change given by 100 times the h -step log-difference of the variable, is denoted $\Delta_h y_{it(p)+h}$. A set of macroeconomic aggregates are used as control variable and consolidated into the vector $X_{it} = [\Delta x_{it}^1 \dots \Delta x_{it}^J \ x_{it}^{J+1} \dots x_{it}^K]'$. The first J elements of this vector are expressed in first difference, the remaining $K - J$ variables are in levels. Let $z_{it(p)}$ represent the accumulated change of the variable z in the preceding expansion that ended at time $t(p)$ for country i . Part of the following experiments will consists in measuring the effect on the path of real GDP per capita of a perturbation on this variable from its long-run mean (for example, the acceleration of borrowing).

This study measures how the recession-recovery path are altered as $z_{it(p)}$ deviates at an experimental level $\bar{z}_i + \delta$ from a given baseline level taken as the long-run mean \bar{z}_i . The conditional path for the average cumulated response of real GDP per capita is

12. As a robustness check, the appendix provides PANEL VAR estimates (see the IRFs from Figures B.15 and B.15 in section 6 on page 316). Due to the sample size, the specification is a first-order PVAR with the smallest set of variables to limit the estimated parameters. Overall, these PVAR robustness exercises produce qualitatively similar results, corroborating the conclusions presented in this paper.

defined as in Jordà (2005):

$$CR(\Delta_h y_{it(p)+h}, \delta) = \mathbb{E}(\Delta_h y_{it(p)+h} \mid z_{it(p)} = \bar{z}_i + \delta; X_{it(p)}, X_{it(p)-1}, \dots) - \mathbb{E}(\Delta_h y_{it(p)+h} \mid z_{it(p)} = \bar{z}_i; X_{it(p)}, X_{it(p)-1}, \dots) \quad (\text{II.7})$$

In expression (II.7), $CR(\Delta_h y_{it(p)+h}, \delta)$ denotes the average cumulated response across countries and recessions, at horizon h in the future, in response to a δ change in the treatment variable, conditional on the lagged history of a rich set of control variables and their lags at the start of the recession. The cumulated response is estimated by assuming that the expectation can be approximated by a sequence of fixed-effects panel regressions:

$$\Delta_h y_{it(p)+h} = \theta_N d_{it(p)}^N + \theta_F d_{it(p)}^F + \beta_{h,N} d_{it(p)}^N (z_{it(p)} - \bar{z}_i) + \beta_{h,F} d_{it(p)}^F (z_{it(p)} - \bar{z}_i) + \sum_{l=0}^L \Gamma_{h,l} X_{it(p)-l} + \alpha_i + u_{h,it(p)} \quad (\text{II.8})$$

where θ_N is the constant associated with normal recessions $d_{it(p)}^N = 1$, θ_F is the constant associated with financial crisis recessions $d_{it(p)}^F = 1$. The specification includes L lags of the control variables¹³ X at time $t(p)$, with coefficient matrices $\Gamma_{h,l}$. Moreover, u is the error term and α_i are country fixed effects. When $z_{it(p)} = \bar{z}_i$, then θ_N and θ_F are the average cumulated response in normal version financial crisis recessions. When $z_{it(p)} = \bar{z}_i + \delta$, the marginal effect of the experiment δ is given by the coefficients $\beta_{h,N}$ and $\beta_{h,F}$ depending on the type of recession. Panel estimation with fixed effects is convenient as it identifies the common component of the response while taking into account differences across countries in the degree of financialization and other macroeconomic aspects.

Interpreting the estimation as the causal impact of an increase z on the recession-recovery path of real GDP per capita would requires δ to be exogenously determined. Such claim of a random assignment would be a stretch. However, since the amount of z

13. The set of control variable X includes the growth rate of real GDP per capita, the growth rate of private credit per capita, the inflation rate, the growth rate of real saving per capita, the growth rate of real public debt per capita and a measure of trade openness. The inclusion of data on lending and public debt should attenuate any effects measured through z , thereby limiting the odds of finding any effects of private credit or public debt on the recession and recovery path.

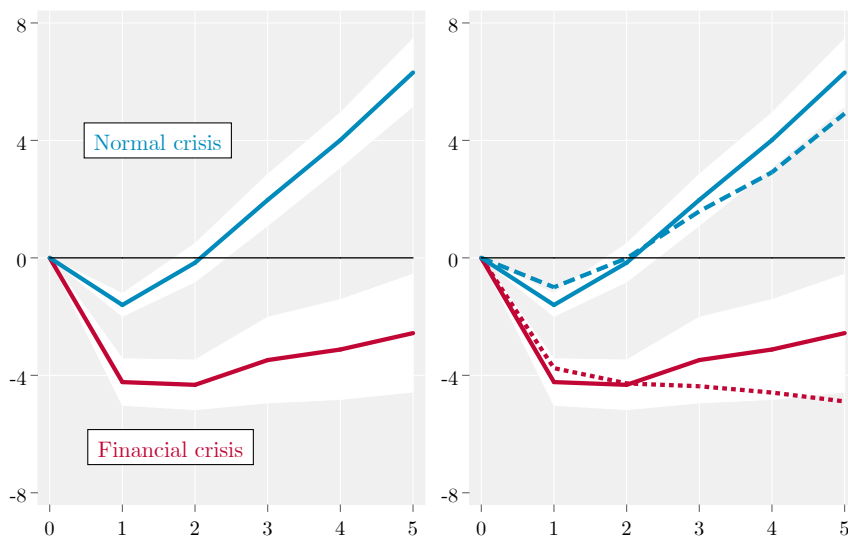


Figure II.6 – Local projection, private credit experiment. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the size of the preceding private credit surge and the type of recession. The dashed and dotted lines refer to a private credit experiment when private credit during the expansion grew at the mean plus one standard deviation. The solid line refers to the average path, and the shaded region is a 90% confidence interval. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

is predetermined, apart from potential anticipation during expansion on the severity of an imminent recession, there is no direct feedback mechanism.

4.3 Sluggish Recovery From Private Credit Surges

Financial crisis recessions appear to be much more credit intensive than normal recessions. This first step considers how private credit surges might alter the recession-recovery path. The measure of the credit intensity of the expansion phase consists in computing the per year change in private liabilities relative to GDP in percentage point per year for each business cycle. The experiment then evaluates the effect of a one standard deviation perturbation from the long-run average.

Figure II.6 displays the cumulated responses for real GDP per capita based on expression (II.8). Consider first the left panel, presenting the estimated path for both normal and financial crisis recession. In normal recessions, output declines in year one by 1.6%, swiftly recovers its original pre-recession level by year two, then goes on expanding in

Table II.11 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom. Sample: 1970-2018.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.60*** (0.24)	-0.16 (0.40)	1.97*** (0.53)	4.01*** (0.57)	6.31*** (0.70)
Financial crisis (average effect)	-4.23*** (0.48)	-4.32*** (0.51)	-3.48*** (0.89)	-3.12*** (1.04)	-2.56** (1.22)
Private credit, Normal	0.15*** (0.05)	0.04 (0.11)	-0.09 (0.12)	-0.27 (0.17)	-0.34* (0.18)
Private credit, Financial	0.16 (0.13)	0.02 (0.16)	-0.30 (0.26)	-0.49 (0.30)	-0.78** (0.33)
<i>F</i> -test equality of coefficients:					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (private credit) <i>p</i> -val.	0.92	0.91	0.44	0.43	0.14
Observations	87	87	87	87	87
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.76	0.57	0.44	0.52	0.59

Notes: Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Robust standard deviation is in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

the subsequent years. After five years, real GDP per capita is 6% higher than its pre-crisis level. The path is dramatically different for financial crisis recessions. They are significantly more painful, with a deeper recession reached in year two (-4%), and a slower recovery. After five years, real GDP per capita remains 2.5% below its pre-crisis level.

Detailed local projection coefficient estimates along with robust standard error clustered at the country level are reported in Table II.11. The first two rows contain the average response of real GDP per capita growth in each type of recession. Recall that three out of four recessions are of the normal type. The bottom of the table provides a standard *F*-test of the equality of the average responses in normal and financial crisis recessions. As expected, there is a significant difference in the two paths.

The two rows labeled *Private credit* report the marginal contribution of an excess of private credit during the previous expansion on the average path of output growth in

the current recession. As an example, consider that private credit expansion is running 3 percentage points (or one standard deviation) over its long-run country-mean. The dashed lines in the right panel of Figure II.6 display the predicted path of the recession fitting this experiment. For each year, the path is computed as the sum of the coefficient from the average response in Table II.11 plus 3 times the coefficient in the corresponding *Private credit* row. Therefore, after five years, real GDP per capita remains 5% below its pre-crisis level.

Overall, the evidence suggests that financial crises recessions are much more dramatic as it leads to a deeper recession and a much slower recovery than with normal recessions. Moreover, the accumulation of private sector liabilities during the preceding expansion significantly delays the recovery.

4.4 An Initially High Public Debt Constrains the Recovery

The second aspect worth considering is whether or not high public debt makes normal and financial recessions worse and recovery slower. In order to investigate this issue, equation (II.8) requires some slight modification to incorporate how the level of public debt to GDP ratio influences the average response in recession. The corresponding sequence of fixed-effects panel regressions is:

$$\begin{aligned} \Delta_h y_{it(p)+h} = & \theta_N d_{it(p)}^N + \theta_F d_{it(p)}^F + \varphi_{h,N} d_{it(p)}^N (g_{it(p)} - \bar{g}_i) + \varphi_{h,F} d_{it(p)}^F (g_{it(p)} - \bar{g}_i) \\ & + \sum_{l=0}^L \Gamma_{h,l} X_{it(p)-l} + \alpha_i + u_{h,it(p)} \end{aligned} \quad (\text{II.9})$$

where $g_{it(p)}$ represents the level of public debt to GDP ratio for country i at the start of the recession at time $t(p)$. Three experiments are considered based on model (II.9). They consist in determining the recession paths for public debt level at its average, one standard deviation below and above its long-run average (situations in which debt is around 25%, 55%, and 85%, respectively). Note, however, that in what follows, these perturbations in public debt are relative to the country mean.

Figure II.7 displays the trajectories of real GDP per capita for financial crisis and normal recessions, along with the aforementioned experiments. The green and red paths



Figure II.7 – Local projections, public debt experiment. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the public debt level at the start of the recession and the type of recession. The solid line refers to public debt at the historical mean, and hence replicates the average response reported in Figure II.6. The shaded region is a 90% confidence interval. The dotted lines correspond to public debt one standard deviation below mean, and the dashed lines to public debt one standard deviation above the mean. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

denote normal recessions and financial crisis recessions. The solid, the dotted, and the dashed lines denote trajectories where the public debt level at the start of the recession is at its long-run average (with 90% confidence interval), is one standard deviation below mean, is one standard deviation above mean, respectively. During normal recessions, the level of public debt at the start of the recession has no impact on the path of the recovery. The evidence is, however, different for financial crisis recessions. By year five, real GDP per capita significantly remains 6% below its pre-crisis level if the public debt was one standard deviation above its mean at the start of the recession. On the contrary, if the level of public debt was one standard deviation above its mean, then, by year five, real GDP per capita fully recovers its pre-crisis level.

Table II.12 provides detailed local projection coefficient estimates along with robust standard error clustered at the country level. The first two rows contain the average response of real GDP per capita growth in each type of recession. The bottom of the

Table II.12 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the public debt level at the start of the recession. Sample: 1970-2018.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.61*** (0.22)	-0.11 (0.47)	1.90*** (0.56)	3.96*** (0.63)	6.28*** (0.75)
Financial crisis (average effect)	-4.22*** (0.53)	-4.64*** (0.59)	-3.71*** (0.90)	-3.24*** (0.99)	-2.61** (1.12)
Public debt, Normal	0.13 (0.66)	-1.40 (1.44)	-1.38 (1.26)	-2.18 (1.68)	-1.64 (1.99)
Public debt, Financial	-1.04 (1.75)	1.55 (2.57)	-0.64 (4.79)	-6.42 (5.26)	-11.24* (6.20)
<i>F</i> -test equality of coefficients:					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (public debt) <i>p</i> -val.	0.53	0.32	0.88	0.45	0.17
Observations	91	91	91	91	91
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.73	0.55	0.39	0.48	0.58

Notes: Financial crisis and normal recession indicators are binary 0-1. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

table provides a standard *F*-test of the equality of the average responses in normal and financial crisis recessions. As in Table II.11, the null is rejected. An additional *F*-test provides the *p*-values of the test that the marginal effect of the initial level of public debt has the same effect in both types of recessions. As the coefficients remain imprecisely estimated in the given sample, the null cannot be rejected.

Overall, the evidence suggests that high levels of public debt alter the recovery capacity of the economy. The initially high level of public debt at the start of financial crisis recessions prevents a counter-cyclical use of public debt to stabilize the economy, due to constrained fiscal space.

4.5 Worst-Case Scenario: Private Credit Booms in Time of Low Fiscal Capacity

So far, evidence suggests that economies should maintain some fiscal space to buffer the fall-out from financial crises and reduce the economic cost of the subsequent recession.

Now the question this paper intends to address is if and how private credit and public debt interact through the business cycle. Would a crisis occurring in a time of private credit surge and low fiscal capacity be more critical? Would an initially high level of public debt compound the aftereffect of private credit surge?

A simple combination and extension of the previous models allows to jointly analyse the impact of private credit surge and high public debt levels. The corresponding sequence of fixed-effects panel regressions becomes:

$$\begin{aligned}
 \Delta_h y_{it(p)+h} = & \theta_N d_{it(p)}^N + \theta_F d_{it(p)}^F + \beta_{h,N} d_{it(p)}^N (z_{it(p)} - \bar{z}_i) + \beta_{h,F} d_{it(p)}^F (z_{it(p)} - \bar{z}_i) \\
 & + \varphi_{h,N} d_{it(p)}^N (g_{it(p)} - \bar{g}_i) + \varphi_{h,F} d_{it(p)}^F (g_{it(p)} - \bar{g}_i) \\
 & + \omega_{h,N} d_{it(p)}^N (z_{it(p)} - \bar{z}_i) (g_{it(p)} - \bar{g}_i) \\
 & + \omega_{h,F} d_{it(p)}^F (z_{it(p)} - \bar{z}_i) (g_{it(p)} - \bar{g}_i) \\
 & + \sum_{l=0}^L \Gamma_{h,l} X_{it(p)-l} + \alpha_i + u_{h,it(p)}
 \end{aligned} \tag{II.10}$$

Similar to the analysis of equation (II.8), the coefficients $\beta_{h,N}$ and $\beta_{h,F}$ measure the effect of the private sector liabilities accumulation during the preceding expansion. As specified for expression (II.9), coefficients $\varphi_{h,N}$ and $\varphi_{h,F}$ capture the impact of the initial public debt level at the start of the recession (in deviation from the country mean). The new coefficients $\omega_{h,N}$ and $\omega_{h,F}$ provide insights on the interplay of the initial public debt level with private credit surges.

Figure II.8 provides three panels displaying the results of the following experiment. Consider that private-sector liabilities expanded at one standard deviation above the long-run average during the preceding expansion. Additionally, the experiment allows for the effect of this private credit overhang to be modulated based on the initial level of public debt to GDP ratio at the start of the recession. Besides the average path displayed as a solid line along with a 90% confidence interval, Figure II.8 investigates three outlines. They indicate how the recession-recovery path is altered when the private credit overhang faces an initial level of debt to GDP ratio one standard deviation below its mean at the start of the recession (dotted line, about 25% of GDP on average), at the

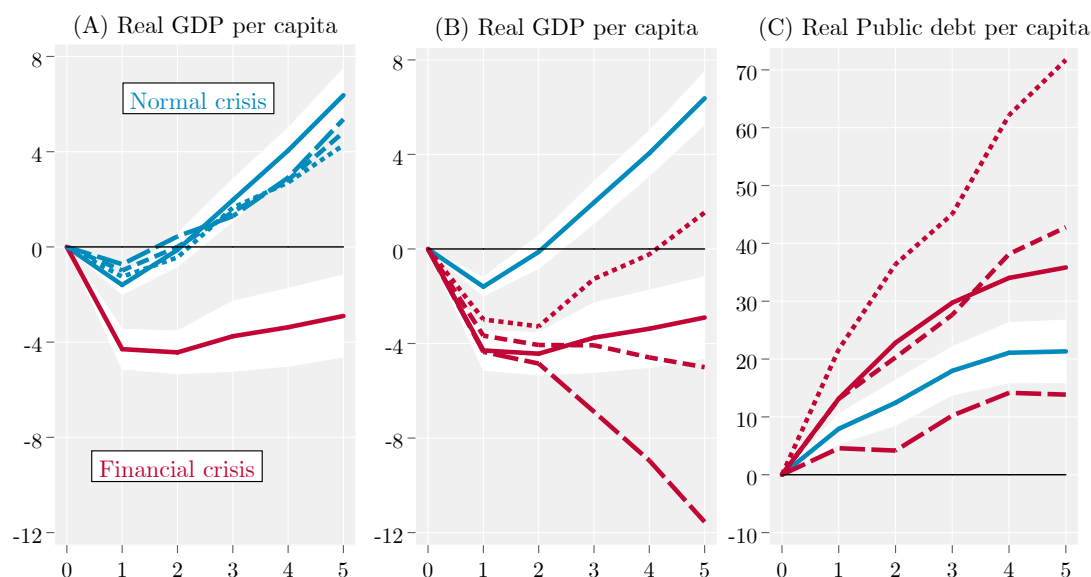


Figure II.8 – Local projections, private credit booms in time of low fiscal capacity. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the interaction of the size of the preceding private credit surge and the initial public debt level at the start of the recession and the type of recession. The solid lines depict the average response in each type of recession with all variables at their mean. The shaded region is a 90% confidence interval. The various nonsolid lines indicate how the path of the economy differs with two simultaneous perturbations: when private credit grows at the average level plus one standard deviation in the previous expansion; and, in addition, when public debt is set one standard deviation below mean (dotted line), at the mean (dashed line), or at one standard deviation above the mean (long-dashed line).

long-run country mean (dashed line, about 55% of GDP on average), or one standard deviation above the country mean (long-dashed line, about 85% of GDP on average).

Panel (A) in Figure II.8 conveys similar conclusions as previous Figures II.6 and II.7. Neither private credit surges nor public debt levels have a notable impact on the recovery trajectory through normal recessions, which are usually swiftly recovered from. The evidence is, however, far more pronounced in panel (B). The three experiments emphasize that a high initial level of public debt to GDP constitutes a substantial drag on both the depth and speed of the post-financial crisis recovery. Financial crises, preceded by a private credit surge, are far more costly when the level of public debt to GDP at the start of the recession is high than when it is low. After five years, economies burdened by a high initial level of public debt are still sinking into the recession, with

real GDP per capita 12 percentage points below its pre-crisis level. On the contrary, economies displaying a low initial level of public debt face the depths of the recession by year two, with a much benign real GDP per capita 3.5 percentage points below its pre-crisis level. By year five, these economies have fully recovered their pre-crisis real GDP per capita level.

As a robustness check, the appendix provides estimates dropping Greece and Portugal from the sample as their trajectory through the Great Financial Crisis of 2008 could bias the results. The local projections estimates presented in Table B.32 and Figure B.11 highlight that the estimates are broadly similar to those presented here with the full sample. Removing all observations related to the 2008 global economic crisis reveals an even sharper conclusion (see Table B.29).

Panel (C) provides insights confirming these findings. On the one hand, low levels of public debt to GDP at the start of the financial crisis recession enable the authorities to pursue active macroeconomic stabilization by running a budget deficit, generating additional public debt. Real public debt per capita can rise substantially to meet the requirements for a swift recovery. On the other hand, high public debt to GDP at the start of the financial crisis recession further constrains the stabilizing capacity. The lack of fiscal space limits the ability of debt to increase and contribute to the sound unfolding of the recovery.

Table II.13 provides detailed local projection coefficient estimates along with several tests. The first two rows contain the average response of real GDP per capita growth in each type of recession. They are comparable to the results of Tables II.11 and II.12, both in magnitude and precision. Once again, the F -test of the equality of the average responses in normal and financial crisis recessions undoubtedly rejects the null that they are identical. The next two rows present the marginal contribution of public debt in the same fashion as in Table II.12. The negative contribution in financial crisis recessions is now larger and more precisely estimated. It remains significantly different from the lack of impact in normal recessions. The novelty in Table II.13 consists in two additional rows presenting the interplay of private credit and public debt through interaction terms. Starting in year three, the coefficients are significantly negative for

Table II.13 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom and the public debt level at the start of the recession. Sample: 1970-2018.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
NORMAL RECESSION					
Average path	-1.60*** (0.24)	-0.12 (0.42)	1.97*** (0.54)	4.05*** (0.58)	6.37*** (0.68)
Private credit	0.15*** (0.05)	0.03 (0.10)	-0.12 (0.11)	-0.30* (0.15)	-0.38** (0.17)
Public debt	0.64 (0.83)	0.25 (1.85)	-0.40 (1.89)	-0.77 (2.11)	-0.01 (2.35)
Public debt \times private credit	0.04 (0.12)	0.27 (0.27)	-0.04 (0.28)	0.26 (0.30)	0.42 (0.34)
FINANCIAL CRISIS					
Average path	-4.30*** (0.51)	-4.43*** (0.54)	-3.75*** (0.90)	-3.38*** (0.99)	-2.90*** (1.05)
Private credit	0.21 (0.17)	0.12 (0.23)	-0.11 (0.36)	-0.41 (0.44)	-0.70 (0.48)
Public debt	-0.83 (1.73)	-0.10 (2.57)	-3.27 (4.76)	-9.50* (5.17)	-15.41*** (5.47)
Public debt \times private credit	-0.57 (0.61)	-0.94 (0.65)	-2.39** (1.10)	-2.23* (1.28)	-2.97* (1.47)
<i>F</i> -test equality of coefficients (<i>p</i> -value):					
Normal = Financial (average effect)	0.00	0.00	0.00	0.00	0.00
Normal = Financial (private credit)	0.70	0.69	0.97	0.80	0.47
Normal = Financial (public debt)	0.45	0.89	0.52	0.09	0.01
Normal = Financial (interactions)	0.32	0.08	0.03	0.06	0.03
Observations	87	87	87	87	87
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.76	0.59	0.48	0.57	0.67

Notes: Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

financial crisis recessions. This mechanism captures a significant friction in the recovery path. A deviation of both private credit and public debt of one standard deviation above their long-run country mean would result in -2.7% less cumulative growth in real GDP per capita by year five (about -0.5 percentage point per annum). The interplay effect is significantly different between normal and financial crisis recessions as the null of equality is rejected in years two, three, four and five.

The present study gives support to the conclusions drawn by Jordà, Schularick, and Taylor (2016). Changing the scope of the data, both in time and country coverage leads to identical outcomes.

5 Conclusions

The present study investigates the joint impact of private credit and public debt over various horizons. It conducts a reappraisal of the business cycles evidence in Jordà, Schularick, and Taylor (2016) and extends the analysis to the medium-run horizon, abstracting from business cycle fluctuations. By looking into 50 years of private credit and public debt interplay, this paper contributes to analyzing the impact of the financial structure on economic growth. The transparent methodology and the dataset consistently decomposing the overall debt structure enable a thorough investigation of the impact of a country's financial structure. Overall, this paper lends support to previous evidence suggesting maintaining both private and public borrowing at lower levels, although for different reasons. Several salient facts emerge.

Evidence stresses that not all types of finances matter; some are substantially more detrimental to growth than others. Private credit deepening exerts a robust and strong adverse effect on growth. Albeit public debt is mostly not relevant to future growth, it can exert positive influence at lower levels. Nevertheless, medium-run evidence suggests that higher levels of public debt enhance private credit's detrimental effect.

Decomposing private credit further unveils interesting patterns. If policymakers ought to be concerned about rising levels of private liabilities, household credit should be carefully held at much lower levels, confirming previous evidence in (Mian, Sufi, and

Verner, 2017). Bringing household credit back to its early 2000s level would deliver an additional half a percentage point of output growth per year in the medium-run for European Union members.

Delving into the business cycle analysis allows for a sharper understanding of the interplay between private credit and public debt. The conjunction of the various horizons provides a clearer understanding of the underlying mechanisms at play.

The investigations emphasize that private sector credit has a detrimental effect on growth as it is recession triggering. Indeed, the financial crises account for a substantial part of private credit's adverse effect. Its effect is more pronounced in the short-run but carries on in the long-run.

Overall, the various estimates confirm previous evidence in Jordà, Schularick, and Taylor (2016) in showing that financial crises recessions are much more dramatic as they lead to a sharper slowdown and a more delayed recovery than with normal recessions. When facing a normal recession, neither private credit surges nor public debt has a notable influence on the recovery trajectory. However, a much more credit-intensive expansion precedes business cycles associated with financial crises than normal recessions. Private credit grows almost four times as rapidly before the financial crisis recessions than before normal recessions.

In line with the recent work of Pescatori, Sandri, and Simon (2014) and Chudik et al. (2016), this paper's evidence shows that it is not the level of public debt but rather its trajectory that affects economic growth. Business-cycle evidence emphasizes that public debt tends to be a drag on growth, not initially, but in the aftermath of a crisis if policymakers cannot run a counter-cyclical fiscal policy. Financial crises are worse when the government's fiscal space is constrained. The interplay of private credit surges with high public debt levels significantly alters the recession-recovery path. Initially high level of public debt compound the aftereffect of private credit surge. Indeed, after five years, economies burdened by an initially high level of public debt are still sinking into the recession, with real GDP per capita 12 percentage points below its pre-crisis level. On the contrary, economies displaying an initially low level of public debt face the financial recession's depths by year two, with a much benign real GDP per capita 3.5

percentage points below its pre-crisis level. By year five, these economies fully recover their pre-crisis level of real GDP per capita.

From a policy perspective, economies should maintain some fiscal space in normal times to buffer the fall-out from financial crises and reduce the subsequent recession's economic cost. Low levels of public debt to GDP at the start of the financial crisis recession enable the authorities to pursue active Keynesian macroeconomic stabilization by running a budget deficit, generating additional public debt. Real public debt per capita can rise substantially to meet the requirements for a swift recovery. Hence, the lack of fiscal space limits the capacity of debt to increase and thereby contribute to the recovery's sound unfolding.

The conclusion differs from Reinhart and Rogoff (2010) as they consider a general threshold not to exceed under any circumstance. This "safe debt level" (Reinhart, Rogoff, and Savastano, 2003) is far from homogenous across countries. The various estimates in this paper underscore the lack of a common threshold and a great deal of heterogeneity. In line with Bernardini and Forni (2017), the present paper emphasizes the importance of accumulating debt at a sustainable pace to avoid the simultaneous retrenchment of private and public sector borrowing in times of crisis.

The separate narratives that have emerged regarding private credit and public debt with output growth conceal meaningful and substantial interactions, calling for further research.

« When the capital development of a country becomes a by-product of the activities of a casino, the job is likely to be ill-done »


John M. Keynes (1936)

Le fonds de sortie des emprunts toxiques : une alternative au contentieux ?

Résumé – Face à la crise des emprunts toxiques des communes, l’État français a créé un fonds de soutien accessible sous condition d’abandon des poursuites judiciaires contre ces emprunts. Cet article étudie économétriquement la stratégie suivie par l’État, au travers des facteurs explicatifs du bénéfice d’une aide du fonds. Le ratio de surcoût du crédit ressort comme le principal facteur de l’octroi d’une aide, devant la situation financière des communes. Cet indicateur a servi de fondement aux instances judiciaires pour donner gain de cause aux communes. Le fait qu’il prédise le bénéfice du fonds rend compte d’une stratégie de l’État pour limiter l’opportunité du contentieux. Cette contribution discute ainsi les choix stratégiques de sortie de crise.

Classification JEL D74 · G2 · H12 · H7 · H81 · K41

Mots clés Dexia · fonds de soutien · municipalités · finances publiques · évaluation des politiques publiques · économie des conflits · économie de la régulation

 *Ce chapitre se base sur un travail réalisé avec Alexandre Mayol (Université de Lorraine) et Samuel Ligonnière (Université de Strasbourg).*

1 Introduction

Les liaisons dangereuses des communes avec les emprunts structurés ne sont pas récentes. En 1991, la Chambre des Lords britannique¹ a été contrainte de neutraliser environ 10 milliards de dollars d'emprunts structurés contractés par les communes de Hammersmith et Fulham, alors au bord de la faillite. Cette décision a déclenché une vague de recours contentieux des communes britanniques pour contester la légalité de leurs emprunts structurés toxiques afin d'éviter la faillite.

En France, à partir des années 2000, les besoins croissants de financement des collectivités et la contrainte de voter un budget local à l'équilibre ont conduit à l'émergence d'innovations financières en matière de dette publique locale. Parmi elles, les produits structurés, proposés principalement par la banque Dexia, ont connu un succès important auprès des collectivités. Ils permettaient en effet de bénéficier d'une conjoncture favorable au calcul des taux variables de court terme (en se basant sur des parités monétaires avantageuses au moment de la signature). Dans la littérature, Pérignon et Vallée (2017) ont analysé les déterminants électoralistes du recours aux emprunts Dexia, où les élus ont privilégié un financement de court terme au détriment du long terme. Entre 2000 et 2011, environ 3000 contrats d'emprunts toxiques seront signés par plus de 1500 collectivités françaises.

Pourtant, la survenance de la crise de 2008 va remettre en cause les équilibres contractuels des emprunts et conduire à la crise des dettes publiques locales. L'évolution des parités monétaires (notamment Euro-Franc suisse) a conduit au déclenchement de clauses contractuelles mettant en œuvre un taux variable prohibitif pour les communes (jusqu'à 80% dans certains cas).

Face à cette situation, la réponse de l'État français sera séquentielle. Dès 2009, une première tentative de médiation entre les communes et les établissements de crédits sera menée, sans succès (moins de 50 accords de médiation signés). Dans le même temps, des communes vont opter pour la voie du recours judiciaire et obtiendront gain de cause devant le Tribunal de grande instance (TGI) de Nanterre en 2013. Le tribunal décidera

1. House of Lords, *Hazell vs. Hammersmith and Fulham London Borough Council* [1991] 2 AC 1.

d'invalider le mécanisme de taux variable des contrats signés avec Dexia pour défaut d'information.

L'application de cette jurisprudence très favorable aux communes (et aux finances publiques) aurait pourtant posé un problème majeur en raison de la recapitalisation de la banque Dexia par la France, la Belgique et le Luxembourg. La perte de capital aurait représenté un surcoût de 17 milliards d'euros pour l'État français². C'est la raison pour laquelle, dans un second temps, le gouvernement fait adopter une loi en 2014 pour invalider rétroactivement cette jurisprudence. En contrepartie, l'État décide de créer un fonds de sortie des emprunts structurés permettant de venir en aide aux communes contraintes de respecter un contrat devenu insoutenable pour les finances publiques locales.

Le fonds d'aide aux collectivités est donc un mécanisme d'État créé en 2014. Les communes devaient déposer un dossier de demande d'aide et recevaient (ou non) ensuite une proposition de refinancement. Enfin, l'acceptation de l'aide était conditionnée par l'abandon de toute poursuite devant les juridictions judiciaires. L'application de la jurisprudence du TGI de Nanterre aurait permis d'épargner aux communes la totalité du poids des intérêts devenus toxiques, alors que dans l'hypothèse d'une aide maximale par le fonds de soutien, celui-ci laisse au mieux 55% des indemnités de remboursement anticipé – très élevées – à la charge des communes.

Dans ce contexte, la finalité réelle du fonds peut être questionnée, d'une part au regard de la chronologie de la gestion de cette crise (la volonté de l'État de ne pas laisser la justice venir en aide aux communes touchées par la crise de la dette toxique), et d'autre part sur la difficile lisibilité des critères d'attribution de l'aide du fonds. De plus, le fait que des communes aient finalement opté pour le contentieux et aient obtenu gain de cause interroge sur la stratégie du fonds : était-il destiné à venir en aide aux communes, ou bien simplement à limiter l'impact des contentieux sur la banque Dexia ?

La littérature économique s'est saisie de l'étude des fonds de soutien en période de crise. En macroéconomie, des travaux ont ainsi analysé la conditionnalité des interventions du FMI (Bi, Chamon et Zettelmeyer, 2016) ; tandis que d'autres ont questionné le caractère

2. Selon les estimations de la Direction Générale du Trésor, présentées dans le rapport annuel de la Cour des comptes (2018).

préventif ou curatif de ces interventions (Asonuma et Trebesch, 2016). D'autres travaux en évaluation des politiques publiques analysent les sauvetages d'agents économiques locaux par le gouvernement central (Sorribas-Navarro, 2011 ; Dietrichson et Ellegård, 2015). Ces études autour du *bail-out* traitent des effets d'un relâchement de la contrainte budgétaire des gouvernements locaux et soulignent que l'intervention du gouvernement central se fonde sur des critères de santé financière de l'organisme public à secourir.

L'ensemble de ces travaux sur les fonds de soutien a pour point commun d'étudier les conséquences du sauvetage sur les agents économiques. Notre travail contribue à cette littérature au travers d'un éclairage nouveau consistant à analyser la stratégie suivie en amont par le fonds pour résoudre une crise de dette publique locale. Au regard du contexte ayant présidé à la constitution du fonds, avec un éclairage de la littérature, nous pouvons identifier deux stratégies possibles pour le fonds de soutien qui font l'objet de propositions testables.

Premièrement, nous considérons une stratégie de sauvetage financier tout à fait classique au regard de la littérature (e.g. Hagen et Dahlberg, 2004 ; Poole, 2009 ; Blau, Brough et Thomas, 2013 ; Eichler et Hofmann, 2013 ; Grossman et Woll, 2014 ; Cabrera, Dwyer et Samartín-Saénz, 2016 ; Cooper et Nikolov, 2018). Le sauvetage financier est la fonction annoncée par les critères réglementaires encadrant le fonds dans le cas étudié³.

Cette stratégie peut être nuancée par une deuxième stratégie identifiée au regard du contexte ayant conduit à la création du fonds. Cette approche consiste à considérer que le fonds serait en réalité une alternative au contentieux et s'inscrirait dans une stratégie d'évitement du conflit judiciaire étudiée en économie des conflits (Priest et Klein, 1984 ; Cooter et Rubinfeld, 1989 ; Cooter, 1991 ; Kennan et Wilson, 1993 ; Shavell, 1997 ; Baumol, 2000 ; Lee et Klerman, 2016).

Dans le cas d'étude proposé, il s'agit de la confrontation de deux agents étudiés ici pour la première fois : l'État (au travers des garanties offertes à Dexia) et les communes. Ces dernières ont deux issues possibles : le recours contentieux – incertain – ou le bénéfice certain d'une aide du fonds de soutien, correspondant ici à une solution amiable

3. Les quatre critères énoncés par le décret n° 2014-444 du 29 avril 2014 sont : (i) la dette par habitant, (ii) la part des produits structurés dans l'endettement total, (iii) la capacité de désendettement de la commune et (iv) le coefficient de mobilisation du potentiel fiscal.

(Alexander, 1991 ; Lederman, 1999 ; Kaplow et Shavell, 2002).

Notre étude s'insère donc dans l'économie des conflits en étudiant la résolution des litiges entre le gouvernement central et les gouvernements locaux. Elle apporte un regard nouveau à cette littérature qui s'est construite sur des litiges entre agents privés, consacrés principalement aux brevets et aux actions de groupe (Powell, 1996 ; Crampes et Langinier, 2002 ; Lemley et Shapiro, 2005 ; Dickey, Orszag et Tyson, 2010 ; Lerner, 2010). Ce chapitre offre ainsi l'opportunité d'étudier non seulement un cas de contentieux entre des acteurs publics, mais aussi d'éclairer les critères déterminant la résolution d'un conflit par une solution amiable. Ces critères sont généralement inaccessibles car tenus secrets entre les agents privés (Boyd et Hoffman, 2013).

Afin de répondre à notre question de recherche et de tester ces deux propositions, nous mobilisons des données communales pour les 1542 communes ayant contracté des emprunts structurés Dexia au travers d'un logit. Notre travail se distingue par le recours à une méthodologie économétrique spécifique, dite de l'AUROC (Schularick et Taylor, 2012), encore jamais mobilisée pour l'étude des sauvetages financiers d'agents économiques. Cette méthode permet de dissocier les vrais signaux positifs des faux signaux positifs, afin d'établir précisément les critères de choix du fonds, et notamment d'établir l'importance relative des stratégies considérées.

Nos résultats principaux indiquent que le ratio de surcoût constitue le meilleur prédictif du recours au fonds et confirme que ce dernier a été une alternative au contentieux pour les communes. Cet indicateur extraréglementaire a été utilisé économétriquement comme un proxy révélant une stratégie d'évitement du contentieux en raison de son utilisation par le TGI de Nanterre pour faire droit aux demandes des communes ayant opté pour la voie judiciaire après 2014. Ainsi, le surcoût du crédit apparaît comme une variable centrale pour identifier les gains et les coûts d'opportunité entre la stratégie du traitement amiable ou judiciaire du conflit. A l'inverse, la stratégie du sauvetage financier est moins significative, démontrant par-là que les communes aidées n'étaient pas nécessairement les plus en difficulté.

Ces résultats importants permettent de questionner le choix de la répartition de la charge des emprunts toxiques entre les collectivités locales et l'État. De plus, notre

étude illustre la mesure empirique de l'arbitrage entre deux modes de règlement des différends en fonction des bénéfices/coûts espérés.

La structure de cet article sera la suivante : après un descriptif du cas Dexia et du contexte institutionnel en Section 2, nous présentons les données et la stratégie empirique en Section 3. Les résultats et les discussions associées suivent en Section 4.

2 Dexia et les collectivités locales

2.1 Les besoins de financement des collectivités et l'émergence des produits structurés

Au début des années 1980, le gouvernement français a souhaité engager un vaste mouvement de décentralisation qui consiste à renforcer l'autonomie des collectivités locales. Ces dernières ont ensuite vu leurs compétences et leurs dépenses s'accroître avec le temps, tout en étant contraintes de voter un budget à l'équilibre. Plusieurs auteurs, comme Binet et al. (2016) ou Taugourdeau et Juliette (2019) ont mis en avant la problématique récurrente du financement des collectivités.

Pourtant, la dotation globale de l'État n'a cessé de diminuer, incitant les collectivités à trouver d'autres modes de financement. Le recours à la fiscalité locale a alors été un levier important – mais toutefois limité – de financement de l'action publique locale. C'est dans ce contexte de ressources contraintes que le recours à l'emprunt s'est imposé comme un mode de financement alternatif et indispensable pour les communes. Ainsi l'endettement des communes représentait 108 milliards d'euros en 2014. Ce phénomène d'accroissement de l'endettement a été particulièrement important après l'acte II de la décentralisation engagé en 2003 qui a considérablement augmenté les compétences des collectivités, sans pour autant accroître leurs ressources autonomes.

Les avantages bien connus du recours à l'endettement (lissage inter-temporel de la dépense, taux d'intérêt bas attractifs...) et le mouvement de libéralisation financière débuté dans les années 1980 ont favorisé l'émergence d'innovations financières comme les produits structurés⁴. Ces produits structurés ont été principalement émis par la banque

4. Rapport de la Cour des comptes de Février 2018.

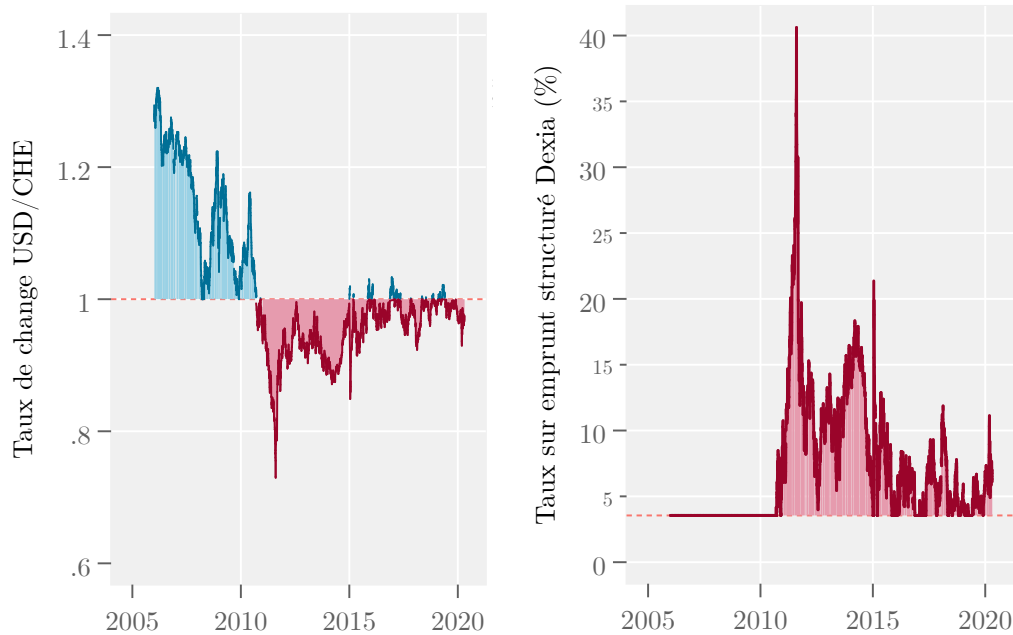


Figure III.1 – Variation du taux sur emprunt structuré Dexia USD/CHE.

Dexia qui détenait un tiers de la dette des collectivités locales et représentait 80% du marché des emprunts structurés (Bartolone et Gorges, 2011). Ces derniers comportent un taux d'intérêt fixe à court terme (2 à 5 ans selon les contrats), bonifié par rapport aux prix du marché, en contrepartie d'un taux variable au-delà reposant sur un écart de taux ou une parité de change, assorti de multiplicateurs et d'effets d'échelle qui accroissent le risque supporté par la commune à moyen terme⁵.

L'attrait de ces emprunts, bien que risqués, réside dans la réduction du coût de l'endettement à court terme. Ainsi, Pérignon et Vallée (2017), en étudiant les déterminants économiques et politiques qui ont motivé les collectivités à recourir à ce type d'emprunt, rendent compte d'une forte préférence pour le présent de ces décideurs publics.

2.2 La crise Dexia et les finances publiques locales

La survenance de la crise de 2008 a bouleversé l'équilibre contractuel des produits structurés puisque l'évolution des taux d'intérêt, d'une part, et surtout l'évolution des

5. Voir les figures C.1 et C.2 page 322 en annexe pour un extrait de contrat d'emprunt structuré ainsi qu'un schéma synthétique.

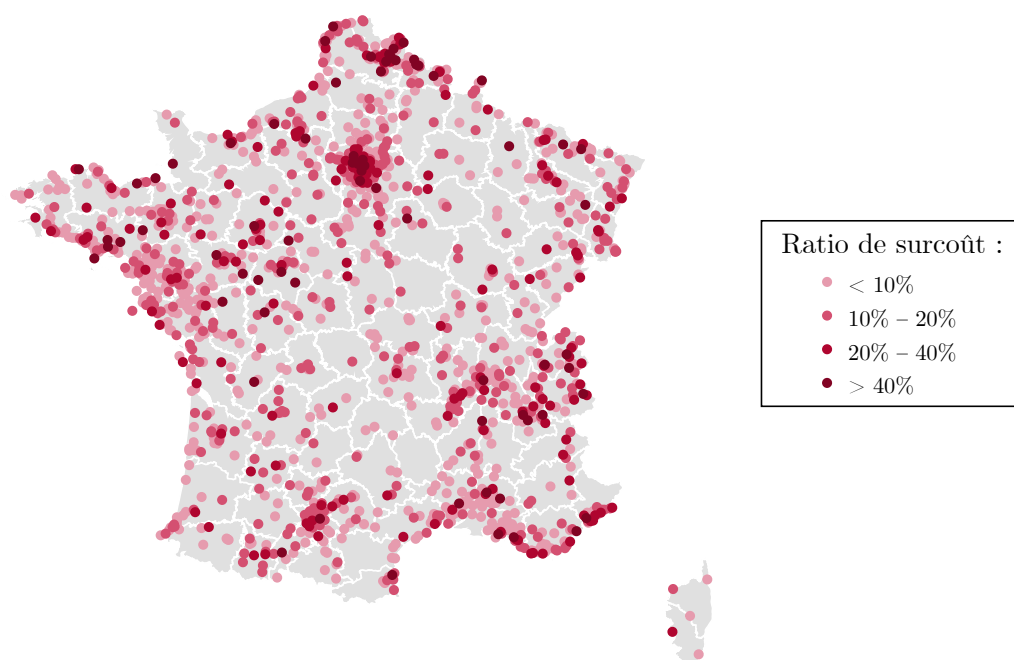


Figure III.2 – Les communes touchées par les emprunts toxiques Dexia.

parités monétaires, d'autre part, ont mis en péril l'équilibre financier des communes qui se sont subitement retrouvées surendettées par le jeu de la variabilité des taux.

Le graphique III.1 illustre ainsi la très forte volatilité des taux supportés par une commune ayant souscrit un contrat de prêt structuré Dexia fondé sur la parité entre le Dollar et le Franc suisse. La commune bénéficie d'un taux bonifié à 3,45% pour les premières années, puis d'un taux variable qui se déclenche si la parité USD/CHE passe sous le seuil de 1. Lorsque la parité USD/CHE plonge en dessous de 1 à partir de 2011, cela déclenche la part variable du contrat avec un taux supporté par la commune qui passe de 3,45% à plus de 40%.

L'exemple du graphique III.1 n'est pas un cas isolé. La crise Dexia représente ainsi plus de 3000 contrats structurés ayant touché un peu plus de 1500 communes en France. La carte présentée dans le graphique III.2 témoigne de l'ampleur de la crise Dexia. Les communes faisant face à des surcoûts élevés sont nombreuses. Ainsi, le ratio de surcoût sur la carte représente le rapport entre le coût de l'emprunt avec la réalisation de la

partie variable du contrat, rapporté au coût initial de l'emprunt si celui-ci était resté au taux fixe initial. Ce ratio de surcoût atteint ainsi 80% pour certaines communes.

Dès le début de la crise, plusieurs rapports ont chiffré l'encours de ces emprunts à risque à hauteur d'une dizaine de milliards d'euros en 2010. Cette situation a conduit de nombreuses collectivités à se retrouver en péril budgétaire et financier, notamment face à des taux d'intérêt à deux chiffres et des clauses pénales impossibles à payer pour ces collectivités en cas d'incident de paiement. Face à cette situation, l'État n'est pas resté en retrait et son intervention a été graduelle.

2.3 Chronologie de la gestion de la crise par les pouvoirs publics

L'État partie prenante À la suite de l'aggravation de la crise de l'endettement souverain, des tensions qu'elle a provoquées sur le marché interbancaire et de graves erreurs de gestion, la banque Dexia, au bord de la faillite, a été recapitalisée par les États français, belge et luxembourgeois. Elle est depuis sous garantie de ces derniers. L'État français est donc une partie prenante de cette situation qui oppose les communes à Dexia.

1^{ère} réponse : la tentative de médiation Dans un premier temps, l'État français a mis en place une médiation avec les établissements bancaires (sous l'impulsion de la Mission Gissler) dès décembre 2009. Il s'agissait d'obtenir une négociation des Indemnités de Remboursement Anticipé (IRA) entre les établissements de crédit et les collectivités, tout en prévoyant une aide financière à la sortie des emprunts de la part de l'État par la création d'un premier fonds de soutien. Cependant, non seulement cette médiation n'a pas permis de résoudre toutes les difficultés au regard du très grand nombre de contrats passés, mais la dotation programmée initialement pour payer les IRA s'est rapidement révélée insuffisante pour faire face aux remboursements. Au total, entre 2010 et 2013, seuls 50 accords de médiation seront signés, soulignant l'échec relatif de cette première réponse.

2^{ème} réponse : le succès des recours contentieux des communes contre Dexia Parallèlement à ce mécanisme de médiation très limité, d'autres communes vont recourir

aux juridictions judiciaires pour contester la validité des contrats d'emprunts structurés. Le Tribunal de grande instance (TGI) de Nanterre fera droit à cette demande dans trois jugements⁶ importants du 8 février 2013. En effet, ces décisions invalident le mécanisme des produits structurés assis sur la volatilité des taux d'intérêt, au motif d'un défaut d'information sur la variabilité des taux appliqués (absence de Taux Effectif Global). L'application de ce jugement conduit à appliquer le taux légal (proche de zéro) en lieu et place du taux contractuel, et ce rétroactivement :

« [La décision du TGI] annule la stipulation conventionnelle d'intérêts, [et] dit que le taux légal doit être substitué au taux conventionnel depuis la conclusion du contrat de prêt [...] ».

Il est donc important de relever qu'à ce stade les collectivités n'étaient juridiquement plus mises en difficultés face à la "crise Dexia" puisque la jurisprudence du TGI de Nanterre venait effacer le surendettement lié aux emprunts structurés. Cette décision entraînera un accroissement très important du contentieux puisque 446 contrats seront attaqués en justice jusqu'en 2014 (Pérignon et Vallée, 2017). Pourtant, face à cette situation, l'État français a réagi puisque cette décision lui faisait courir un risque important de pertes financières, à hauteur de 17 milliards d'euros (selon les estimations de la Direction Générale du Trésor) au travers des garanties dont bénéficie la banque Dexia.

3^{ème} réponse : la création d'un fonds de soutien Face à cette situation de contentieux massif, l'État fait le choix de bloquer cette voie de sauvetage financier par le biais de la loi du 29 juillet 2014 *relative à la sécurisation des contrats de prêts structurés souscrits par des personnes morales de droit public*. Cette loi a ainsi validé rétroactivement l'intégralité des contrats de prêts, pourtant jugés non conformes.

Le recours à une loi rétroactive est très exceptionnel. Le principe de droit est celui de la non-rétroactivité de la loi, énoncé à l'article 2 du Code civil français : « La loi ne dispose que pour l'avenir ; elle n'a point d'effet rétroactif ». Une loi rétroactive est donc, sauf exceptions, contraire à la sécurité juridique, ce que n'a pas manqué de souligner

6. TGI Nanterre 6^e ch., 8 févr. 2013, n^o 11/03778, Département de la Seine-Saint Denis c/ Société Dexia Crédit Local « FIXIL ». TGI Nanterre 6^e ch., 8 févr. 2013, n^o 11/03779, Département de la Seine-Saint Denis c/ Société Dexia Crédit local « DUALYS OPTIMISE ». TGI Nanterre 6^e ch., 8 févr. 2013, n^o 11/03780, Département de la Seine-Saint Denis c/ Société Dexia Crédit local « DUAL ».

le Conseil constitutionnel en censurant la première version de cette loi au motif que sa portée était trop large⁷.

La mouture finalement retenue dans la loi du 29 juillet 2014 ne portait plus que sur l'absence de TEG préalablement invoquée par le TGI de Nanterre, laissant ouverte la possibilité de recours pour d'autres motifs. Les communes dénoncent alors une « amnistie bancaire », jugée « désastreuse pour les finances locales [...] au moment même où plusieurs décisions de justice donnaient raison aux collectivités, en ramenant le taux d'intérêt de leurs prêts [...] au taux d'intérêt légal proche de 0% »⁸.

Par conséquent, un fonds de soutien a été mis en place pour résoudre le problème de solvabilité des collectivités locales. L'enveloppe de ce fonds de soutien, initialement prévue à 1,5 milliard d'euros, va doubler pour s'adapter aux évolutions de la conjoncture économique (notamment la fin de la parité forcée entre le Franc suisse et l'Euro). Le fonds a vocation à financer les IRA liées à la renégociation du contrat. En ce sens, le fonds de soutien repose sur une voie amiable entre les communes et Dexia.

Le fonctionnement du fonds de soutien a été encadré par le décret n° 2014-444 du 29 avril 2014 *relatif au fonds de soutien aux collectivités territoriales et à certains établissements publics ayant souscrit des contrats de prêt ou des contrats financiers structurés à risque*. Ce décret précise ainsi des critères d'éligibilité larges qui englobant toutes les variantes possibles d'emprunts structurés. Ce même décret établit également les quatre critères d'octroi d'une aide : (i) la dette par habitant, (ii) la part des produits structurés dans l'endettement total, (iii) la capacité de désendettement de la commune et (iv) le coefficient de mobilisation du potentiel fiscal. L'existence de ces critères, à la fois objectifs et transparents, souligne ici que le fonds de soutien tient compte principalement de la situation financière de la collectivité pour proposer une aide (proportionnelle aux IRA). Les demandes d'aide devaient être déposées avant le 30 avril 2015 auprès des préfetures.

Le bénéfice de l'aide est toutefois assorti d'une contrepartie : l'abandon de tout recours contentieux contre le contrat souscrit par la collectivité. Le fonds de soutien se construit donc autour de la sécurisation des prêts structurés en invitant à une solution amiable.

7. Décision n°2013-685 DC du 29 décembre 2013.

8. M. Damgé, « Emprunts toxiques : les départements pris au piège de taux d'intérêt démesurés », *Le Monde*, 21 mars 2015.

2.4 Quelle stratégie pour le fonds de soutien ?

La chronologie de la réponse à la crise Dexia soulève de multiples interrogations quant à la stratégie de l'État. En effet, l'action contentieuse des communes aurait suffi à éteindre financièrement cette crise par l'invalidation des contrats signés. Pourtant, l'intervention de l'État a visé à priver de fondement les recours engagés et à proposer, en guise de compensation, un fonds de soutien pour aider les communes à faire face aux taux variables des contrats. Deux questions se posent à ce stade.

La stratégie adoptée pour octroyer l'aide du fonds peut être questionnée. S'agissait-il d'un mécanisme d'aide aux collectivités – lesquelles n'avaient plus véritablement besoin d'une aide si la jurisprudence du TGI de Nanterre s'était appliquée – comme le suggèrent les critères réglementaires d'octroi d'une aide du fonds, ou bien était-il destiné à éviter le coût lié à l'application de cette jurisprudence pour l'État ? Le conditionnement de l'aide du fonds au renoncement au recours contentieux peut questionner de ce point de vue.

La loi du 29 juillet 2014 *relative à la sécurisation des contrats de prêts structurés* n'a d'ailleurs pas totalement éteint le mouvement de recours contentieux. De nouvelles actions contentieuses se révéleront victorieuses, comme l'illustre la décision du TGI de Nanterre du 26 juin 2015 :

« Sera en revanche pris en compte le surcoût du prêt, à savoir le surplus d'intérêts auquel la commune s'est vu exposée en comparaison du coût qu'elle aurait dû assumer si le taux d'intérêt ne s'était pas dégradé. [...] Au total, le tribunal retiendra donc que le préjudice de perte de chance de la commune s'établit à la date du présent jugement à 50% de ce surcoût [...] ».

Cette nouvelle jurisprudence change donc son fondement pour contourner la loi rétroactive et sanctionner les contrats Dexia en invoquant désormais la perte d'une chance liée au surcoût excessif au regard de l'emprunt contracté par la commune. L'apparition de ce nouveau critère interpelle car il permet aux communes de réaliser un bilan "coût-avantage" entre recourir au fonds et aller devant le tribunal. Notre étude visera donc à mieux comprendre la stratégie d'octroi du fonds et le comportement des communes face à l'opportunité de recourir au juge plutôt qu'une solution amiable supportée financièrement par le fonds.

3 Propositions sur les différentes stratégies adoptées par le fonds

Nous proposons donc d'étudier le comportement du fonds de soutien et le profil des communes ayant bénéficié d'une aide financière pour contribuer à répondre à la question de recherche suivante : que nous dit le comportement du fonds de soutien de la stratégie de l'État face à la crise Dexia ? Pour cela, nous considérons principalement deux propositions qui correspondent chacune à un comportement stratégique possible pour l'État au regard de la littérature et du contexte présenté précédemment.

3.1 Le sauvetage financier des communes

Une littérature abondante documente ainsi les sauvetages de gouvernements locaux par le gouvernement central (e.g. Khemani, 2003 ; Sorribas-Navarro, 2011 ; Dietrichson et Ellegård, 2015). Dans cette approche, l'intervention du gouvernement central se fonde sur des critères de santé financière de l'organisme public à secourir.

Le décret n° 2014-444 du 29 avril 2014 *relatif au fonds de soutien aux collectivités territoriales ayant souscrit des contrats de prêt ou des contrats financiers structurés à risque* établit dans son article 5 les critères réglementaires d'évaluation pour la décision d'octroi d'une aide du fonds à une commune. Ces critères visent précisément à évaluer la santé financière de la commune.

Le fonds exerce donc en principe les fonctions typiques d'un fonds de garantie, en proposant une aide pour des communes faisant face à des difficultés financières. Nous considérons ainsi dans un premier temps une stratégie de sauvetage financier tout à fait classique au regard de la littérature (e.g. Hagen et Dahlberg, 2004 ; Poole, 2009 ; Blau, Brough et Thomas, 2013 ; Eichler et Hofmann, 2013 ; Grossman et Woll, 2014 ; Cabrera, Dwyer et Samartín-Saénz, 2016 ; Cooper et Nikolov, 2018).

Proposition 1 : sauvetage financier

La dégradation de la situation financière de la commune explique le bénéfice d'une aide du fonds de garantie.

3.2 L'évitement du conflit

Nous avons vu que le fonds relève en apparence d'une démarche de sauvetage financier typique. Néanmoins, la décision du TGI de Nanterre aurait en principe suffi à assurer le sauvetage des communes en difficulté. Comme précédemment indiqué, le coût potentiel de 17 milliards d'euros amène l'État à mettre en place un certain nombre de mesures. L'État accompagne ainsi la mise en place du fonds de soutien, non seulement d'une loi à caractère rétroactif visant à endiguer l'essor du contentieux, mais aussi d'une contrepartie pour bénéficier du fonds reposant sur l'abandon du recours contentieux sur les motifs qui n'auraient pas été visés par la loi rétroactive.

À la lumière de la chronologie des événements et du contexte, nous pouvons nous demander si nous ne sommes pas face à une stratégie d'évitement du recours contentieux. Cette proposition se fonde sur le travail pionnier en économie des conflits de Priest et Klein (1984) et Cooter et Rubinfeld (1989) qui pose un cadre pour identifier le profil des affaires qui vont au procès, et celles qui font l'objet d'un arrangement amiable à partir de plusieurs dimensions dont notamment l'asymétrie des enjeux entre les parties (Lee et Klerman, 2016).

Il s'agit donc dans notre cas de la confrontation assez classique entre deux agents, avec deux solutions possibles : le recours contentieux ou le bénéfice d'une aide du fonds, ce dernier correspondant ici à une solution amiable (e.g. Alexander, 1991 ; Lederman, 1999 ; Kaplow et Shavell, 2002 ; Boyd et Hoffman, 2013). La spécificité par rapport aux études précédentes est que nous considérons deux agents n'ayant jamais fait l'objet de travaux en économie des conflits : l'État, au travers des garanties offertes à la banque Dexia, et les communes. Cette approche apporte un regard nouveau à cette littérature qui s'est construite sur des litiges entre agents privés, consacrés principalement aux brevets et aux actions de groupe (Powell, 1996 ; Crampes et Langinier, 2002 ; Lemley et Shapiro, 2005 ; Dickey, Orszag et Tyson, 2010 ; Lerner, 2010).

Nous cherchons ici à étudier si le montant du dommage potentiel pour les communes, susceptible de motiver un recours contentieux, est le critère déterminant du fonds. Si cela est vérifié, le fonds de soutien joue alors le rôle d'un arbitre pré-contentieux pour limiter les recours devant les instances judiciaires.

Proposition 2 : évitement du contentieux

Le montant du dommage potentiel susceptible de motiver le recours contentieux explique le fait de bénéficier du fonds de garantie.

4 Méthodologie empirique et données

4.1 Sources des données

Données sur les communes. Tout d'abord, nous mobilisons les données détaillées des bilans des communes, fournies par la Direction Générale des Finances Publiques. Ce jeu de données nous permet d'appréhender avec beaucoup de précision l'état des finances publiques locales. Afin de présenter l'octroi d'une aide à une commune, la structure du bilan des communes a été modifiée à partir de 2017, année de première contribution du fonds, pour faire apparaître l'aide du fonds de soutien. Soulignons que nos demandes pour des données plus détaillées auprès du Service à compétence nationale de pilotage du dispositif de sortie des emprunts à risque ont fait l'objet de refus.

Nous complétons ces données financières, par des données socio-économiques prélevées dans la base *Compareur des territoires* de l'INSEE. Pour contrôler que la gestion du fonds n'a pas conduit à une capture politique de l'aide, nous mobilisons le *Registre national des élus*. Ce jeu de données nous a été transmis par le Ministère de l'Intérieur. Pour chaque commune, nous disposons d'informations sur la couleur politique du maire. Nous pouvons ainsi étudier si l'affiliation politique de la commune a été un élément déterminant dans la décision d'octroi d'une aide.

Données sur les emprunts structurés Dexia. Aucune statistique publique ne permet de déterminer quels types d'emprunts constituent l'encours de dette des communes. En effet, la législation n'impose pas aux communes de présenter leurs emprunts structurés séparément dans leur bilan. Cependant, en septembre 2011, pendant les négociations portant sur le sauvetage de la banque Dexia par l'État français, le journal *Libération* a obtenu des données confidentielles de Dexia. Ce fichier détaille l'intégralité des prêts

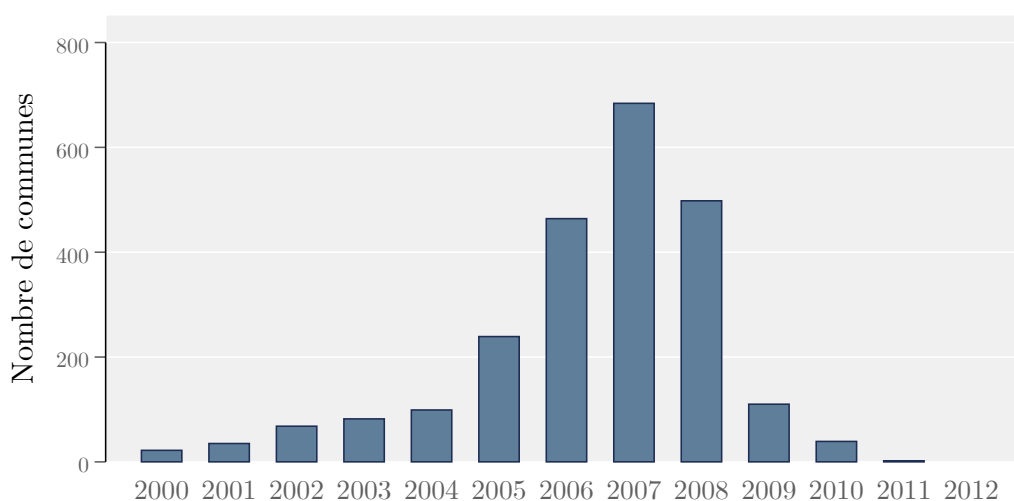


Figure III.3 – Nombre de communes ayant souscrit au moins un nouveau contrat structuré Dexia au cours de l’année.

structurés fournis par Dexia aux communes. Il regroupe non moins de 3016 prêts octroyés à 1586 communes. Pour des raisons de disponibilité et de concordance avec les autres bases, nous ne retenons que 1542 communes ayant souscrit des emprunts structurés Dexia, uniquement en métropole, dont l’échéance des emprunts est postérieure à 2015, année d’étude des dossiers par le fonds.

Nous avons donc un panorama des emprunts structurés, dits «toxiques», distribués par Dexia de 2000 à 2011, et encore en exercice en 2011. Nous disposons du montant de ces emprunts, du surcoût attendu pour les collectivités, de leur date d’émission et de fin. Ce dernier aspect nous renseigne sur la temporalité de ces prêts structurés toxiques dans le bilan des communes. Ainsi, la figure III.3 montre que l’émission de ces prêts toxiques a connu un essor jusqu’en 2007 avant de se contracter. En 2010, moins de 40 communes souscrivaient à de nouveaux prêts toxiques contre près de 700 au cours de l’année 2007.

Il est important de noter à ce stade que ce jeu de données n’est pas parfait puisque Dexia n’est pas la seule source d’emprunts structurés accessible aux communes. Nous faisons l’hypothèse que cette base permet d’avoir une image relativement fiable de l’exposition des communes aux emprunts structurés. En effet, Dexia bénéficie d’une position dominante sur le marché des prêts structurés à destination des communes, elle représente plus de 80% des prêts octroyés (Cour des comptes, 2018).

Enfin, après son sauvetage en 2011, Dexia s'est vue interdire le droit de prêter aux collectivités locales par le biais de produits structurés. De plus, les communes se sont également vu interdire le financement par ces emprunts. Les emprunts présents dans notre base constituent vraisemblablement la majeure partie de ces prêts structurés toxiques.

4.2 Spécification estimée

Nous cherchons à évaluer les déterminants du choix d'octroi d'une aide du fonds de soutien à une commune touchée par les emprunts toxiques. Pour cela, nous commençons par un modèle de probabilité de type logit qui associe à la décision d'octroi d'une aide du fonds un ensemble de variables explicatives comme suit :

$$\log \left\{ \frac{\mathbb{P}(\text{Fonds}_i = 1)}{\mathbb{P}(\text{Fonds}_i = 0)} \right\} = \beta_0 + \beta_1 \mathbf{X}_i + \varepsilon_i$$

où la variable Fonds_i est une indicatrice prenant la valeur 1 si la commune a bénéficié d'une aide du fonds de soutien en 2017 ou 2018 (années des premiers versements), et 0 dans le cas contraire⁹. La matrice \mathbf{X}_i correspond à des groupes alternatifs de variables explicatives¹⁰. Nous regroupons ainsi les nombreuses variables potentielles par dimension. Chaque dimension sera analysée successivement. Ces variables sont mesurées pour l'année 2014, lors de laquelle les communes ont pu demander une aide auprès du fonds, et ainsi déposer un dossier présentant l'état de leurs finances¹¹. La table III.1 synthétise ces dimensions et présente les différences de moyenne entre les communes ayant bénéficié d'une aide et les autres. Ces données préliminaires sont complétées par la méthode du Bayesian Model Averaging pour identifier les caractéristiques des communes qui bénéficient d'une aide du fonds, dont les résultats sont présentés dans la section 5.4 de

9. La section 5.2 page 338 en annexe présente des méthodes d'estimations alternatives, avec un modèle de Poisson puis le Penalized Maximum Likelihood Logit de Firth (1993), qui confirment bien les estimations obtenues par un modèle logit conventionnel.

10. En complément des dimensions présentées ici, la section 3 page 327 en annexe fournit des analyses complémentaires corroborant d'une part l'absence de capture politique du fonds de soutien et d'autre part l'absence d'intention de redistribution dans la gestion allocative du fonds.

11. Nous avons estimé ce modèle sur les données de 2013 et 2012, constituant ainsi un test de robustesse complémentaire. Les résultats sont qualitativement similaires, soulignant la relative inertie dans les finances publiques locales. Ces résultats sont présentés dans les Tables C.9 et C.10, dans la section 5.1 page 334 en annexe.

Table III.1 – Différences parmi les communes éligibles, entre celles ayant touché une aide du fonds de soutien et celles n’ayant rien perçu.

<i>Aide du fonds de soutien :</i>	Moyenne		Différence
	NON	OUI	
Dimension réglementaire → (p₁) : sauvetage financier			
· Dette par habitant	1321	1873	0.552**
· Part Dexia dans l’endettement total	0.448	0.570	0.121
· Capacité de désendettement	9.698	12.12	2.423
· Coefficient de mobilisation du potentiel fiscal	1.124	1.110	-0.014
Dimension financière → (p₁) : sauvetage financier			
· Taux d’équipement	0.261	0.198	-0.063***
· Taux d’endettement	0.931	1.106	0.18***
· Taux d’autofinancement des nouveaux investissements	0.277	0.160	-0.12*
· Taux de capacité d’autofinancement	0.148	0.108	-0.034***
· Taux de rigidité des charges de structure	0.562	0.599	0.037***
Dimension recours contentieux → (p₂) : évitement du conflit			
· Ratio de surcoût total	0.106	0.231	0.125***
Autres variables de contrôle			
· Population	15392	23845	8452.7*
· Niveau vie médian	20510	20429	0.009
· Taux de chômage	0.139	0.148	0.009
· Ratio de 15-64 ans	0.620	0.622	0.003
· Extrême gauche	0.043	0.050	0.007
· Gauche	0.321	0.283	-0.038
· Droite	0.579	0.642	0.063
· Extrême droite	0.006	0.000	-0.006
· Sans étiquette et autres	0.051	0.025	-0.026
Observations	1422	120	

l’annexe, et permettent d’identifier trois variables pertinentes : le ratio de surcoût, le taux d’endettement et la taille de la commune.

Afin de déterminer l’importance des différents critères, la méthode dite de l’AUROC (Area Under the Receiving Operator Curve) à la Schularick et Taylor (2012) est pertinente, plus encore que la simple significativité statistique. Elle permet de dissocier les vrais signaux positifs des faux signaux positifs, à travers une mesure comprise entre 0 et 1. Plus l’AUROC est proche de 1, plus le contenu informatif des variables explicatives permet de véritablement sélectionner les “bonnes” communes (i.e. celles qui ont effectivement reçu de l’aide du fonds) et de ne pas inclure les “mauvaises” communes. Plus

l'AUC est proche de 0,5 et moins le modèle fournit d'informations permettant de déterminer les raisons poussant à dissocier une commune ayant reçu cette aide par rapport à une autre (le modèle performe alors aussi bien qu'un lancer de pièce).

Dimension réglementaire. Cette dimension reprend les critères présentés dans le décret n° 2014-444 du 29 avril 2014 *relatif au fonds de soutien aux collectivités territoriales et à certains établissements publics ayant souscrit des contrats de prêt ou des contrats financiers structurés à risque*. Ce dernier établit dans son article 5 les critères réglementaires d'évaluation pour la décision d'octroi d'une aide du fonds à une commune :

1. *le montant de la dette rapporté à la population* ;
2. *la capacité de désendettement* mesurée par le rapport entre l'encours de la dette et l'épargne brute. Elle représente le nombre d'années nécessaires pour rembourser la totalité du stock de dette dans l'hypothèse où la collectivité y affecterait l'ensemble de son épargne ;
3. *la mobilisation du potentiel fiscal de la commune*, mesure la pression fiscale exercée par la collectivité sur son territoire par rapport à la moyenne nationale. Cet indicateur permet donc d'évaluer la marge de manœuvre du levier fiscal d'une collectivité au regard des autres collectivités de taille comparable. Plus la commune exerce une pression fiscale forte, moins elle sera à même de mobiliser ce levier pour répondre à des difficultés financières ;
4. *la part des contrats structurés dans l'encours total de la dette*, qui sera ici évaluée par la part des contrats structurés Dexia dans l'encours total de la dette.

Ces critères sont présentés dans le décret précédemment cité comme ceux qui fondent la décision d'octroi d'une aide du fonds. Cette dimension permet donc dans un premier temps de tester la proposition (P₁) de stratégie de sauvetage financier par le fonds, la stratégie annoncée. Nous nous attendons donc logiquement à trouver une forte capacité prédictive de ces quatre critères, et donc de cette dimension réglementaire, dans la décision d'aide.

Dimension financière. Afin de compléter la dimension réglementaire, nous proposons un ensemble de critères financiers plus poussés qui évaluent, par une série de taux, la santé financière de la commune ainsi que ses marges de manœuvre. Cette dimension permet également, dans un second temps de tester la proposition (P₁) de sauvetage financier sur des critères toujours financiers, mais non réglementaires :

1. *le taux d'équipement*, défini comme le rapport des dépenses d'équipement brut et des recettes réelles de fonctionnement, il mesure l'effort d'équipement de la commune au regard de sa richesse ;
2. *le taux d'endettement*, mesuré par le rapport entre l'encours de dette et les recettes de fonctionnement. Un ratio élevé signifie que la collectivité est fortement endettée par rapport à ses revenus ;
3. *le taux d'autofinancement des nouveaux investissements*, défini comme le rapport entre l'épargne nette de la commune et ses dépenses réelles d'équipement. Ces dernières se définissent comme les dépenses d'investissement, desquelles on déduit les remboursements de capital des emprunts. Ce taux mesure donc la part autofinancée des nouveaux investissements. Le ratio permet de mesurer la capacité de la collectivité à financer par son épargne une partie de ses investissements. Plus ce ratio est élevé, plus la part autofinancée des investissements est importante, et plus le recours à l'emprunt est limité. À l'inverse, un faible taux signifie que la collectivité est davantage tributaire de l'emprunt pour financer ses équipements ;
4. *le taux de capacité d'autofinancement*, défini comme le rapport entre l'épargne brute et les recettes réelles de fonctionnement. Il permet d'appréhender la capacité de la commune à générer de l'épargne pour autofinancer ses investissements ;
5. *le taux de rigidité des charges de structure* mesure le poids des dépenses de personnel dans les dépenses de fonctionnement. Les dépenses de personnel sont en effet considérées comme difficilement modulables : elles ont tendance à augmenter du fait du "glissement vieillesse-technicité".

Dimension évitement du conflit. Cette dimension vise à tester la proposition (P₂) d'évitement du recours contentieux. Pour cela, nous cherchons à déterminer si l'aide de

fonds a été dirigée en priorité vers les communes les plus susceptibles de gagner lors d'un recours contentieux. Au regard des différentes décisions rendues par le TGI de Nanterre, il apparaît que le ratio de surcoût est déterminant non seulement pour obtenir gain de cause, mais également pour déterminer le montant des dommages et intérêts obtenus par la commune en compensation (là où la jurisprudence antérieure appliquait le taux légal proche de zéro en lieu et place du taux contractuel).

De plus, un ratio de surcoût élevé n'est pas nécessairement le signe d'un péril financier pour la commune. En effet, une commune peut faire face à un ratio de surcoût élevé et être tout à fait à même de rembourser ses emprunts à taux variables. Ainsi, la corrélation entre le ratio de surcoût et le taux d'endettement de la commune est faible ($\rho = 0.13$), et proche de zéro pour les autres critères réglementaires qui, rappelons-le, évaluent la santé financière de la commune et sa capacité de remboursement. En effet, le ratio de surcoût supporté par la commune n'est corrélé ni à sa capacité de désendettement ($\rho = 0.01$) ni aux marges de manœuvre fiscales dont dispose la commune ($\rho = 0.02$).

Cette dimension se construit donc autour du ratio de surcoût sur les emprunts structurés Dexia. Cette variable mesure la valeur de marché du produit structuré rapportée à la valeur initiale du produit au moment de la signature. Plus le ratio de surcoût est élevé, plus la commune a de chance de gagner au contentieux et plus les indemnités perçues seront élevées.

Caractéristiques socio-économiques et politiques. Nous considérons enfin des critères non-financiers. Un premier groupe de variables apporte un éclairage sur la population qui constitue la commune. Cet ensemble de variables constitue avant tout un moyen de contrôler le caractère redistributif de l'aide :

1. *la population*, nous prenons le logarithme de cette variable afin de quantifier un éventuel effet taille ;
2. *le niveau de vie médian* permet de déterminer si la richesse de la commune a eu une incidence déterminante, nous prenons également le logarithme de cette variable ;
3. *le taux de chômage* permet de mesurer le dynamisme économique de la commune ;
4. *la part de 15-64 ans dans la population* mesure le dynamisme de la commune.

Enfin, nous étudions l'incidence de la couleur politique de la commune dans l'octroi d'une aide par le fonds de soutien. Cet ensemble de variables permet de contrôler l'éventuelle capture politique du fonds de soutien. Pour cela, nous mobilisons le *Registre national des élus*, permettant d'associer chaque commune à une des grandes familles politiques suivantes : extrême gauche, gauche, sans étiquette et autres, droite et extrême droite. Nous utiliserons également une variable de nuance définie comme un continuum allant de -2 pour l'extrême gauche à $+2$ pour l'extrême droite, avec -1 pour la gauche, $+1$ pour la droite et 0 pour les sans étiquette et autres.

5 Résultats empiriques

Nous allons présenter et commenter les résultats issus de nos différents modèles. Pour des raisons de lisibilité et d'interprétabilité, nous avons choisi de proposer plusieurs régressions avec des ensembles alternatifs de variables. Chaque dimension de la section 4 fera l'objet d'une estimation pour tester nos hypothèses. Par souci de transparence et de clarté, la dernière colonne de chacune des tables de résultats présentera les estimations de la dimension étudiée assortie des autres dimensions comme variables de contrôle.

5.1 Un sauvetage financier affranchi des critères réglementaires

Considérons tout d'abord les critères réglementaires. En toute logique, ces critères devaient être hautement déterminants dans la décision d'octroi d'une aide. Cependant, comme nous avons pu le remarquer en considérant quelques statistiques descriptives, les communes qui ont bénéficié d'une aide ne semblent pas se distinguer des autres sur la base de ces critères.

La table III.2 illustre comment les critères réglementaires peuvent expliquer la probabilité de bénéficier d'une aide du fonds de soutien. Les colonnes (1) à (4) intègrent progressivement les variables de la dimension réglementaire. Cet ajout progressif permet de s'assurer de la stabilité des coefficients. À ce stade, nous pouvons faire plusieurs constats. D'abord, les variables réglementaires n'ont qu'une très faible capacité prédictive de l'octroi d'une aide du fonds. Alors que ces critères sont légalement annoncés

Table III.2 – Critères réglementaires.

Régressions Logit	(1)	(2)	(3)	(4)	(5)	(6)	
Autres dimensions comme variables de contrôle :	NON	NON	NON	NON	NON	OUI	Effets margin.
(a) Dette par habitant	0.063* (0.028)	0.064* (0.028)	0.063* (0.028)	0.074* (0.033)	0.044 (0.025)	0.023 (0.035)	0.001 (0.002)
(b) Part Dexia dans l'endettement total		0.050 (0.026)	0.051 (0.026)	0.050 (0.026)	0.024 (0.028)	0.110** (0.039)	0.006** (0.002)
(c) Capacité de désendettement			0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	-0.002 (0.002)	-0.000 (0.000)
(d) Coefficient de mobilisation du potentiel fiscal				-0.342 (0.285)	-0.370 (0.340)	-0.198 (0.395)	-0.011 (0.022)
Ratio de surcoût total					10.308*** (1.162)	9.614*** (1.010)	0.540*** (0.061)
Observations	1542	1542	1542	1542	1542	1542	–
AUROC	0.687 (0.023)	0.749 (0.019)	0.741 (0.019)	0.629 (0.025)	0.858 (0.018)	0.872 (0.017)	– –

Notes : La dernière colonne présente les estimations en intégrant l'ensemble des dimensions considérées. La table C.8 en annexe présente l'ensemble des coefficients pour cet exercice. Écart-types robustes entre parenthèses. La section 5.3 page 340 en annexe présente un exercice d'interaction entre la variable d'endettement et le ratio de surcoût. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

comme déterminants, seul l'endettement par habitant est significativement positif.

Pour s'assurer de la robustesse de ces résultats, nous contrôlons pour l'ensemble des dimensions. Cet exercice est reporté dans la colonne (6). Nous constatons que la dimension réglementaire n'a pas une valeur prédictive importante concernant la décision d'octroi d'une aide par le fonds. L'endettement par habitant n'est plus statistiquement significatif alors que la part des emprunts structurés Dexia dans l'ensemble de la dette le devient. Le ratio de surcoût total demeure en revanche une variable essentielle dans la décision du fonds.

Les effets marginaux permettent de confirmer cette lecture. Par exemple, la *part Dexia dans l'endettement total*, bien que significative, présente un faible effet marginal. Une hausse de 10 points de pourcentage (ci-après *pp*) de la part des emprunts structurés Dexia dans l'endettement total n'accroît que de 0,06*pp* la probabilité d'obtenir une aide du fonds de soutien. En revanche, l'effet marginal moyen d'une hausse de 10*pp* du taux

de surcoût total supporté par la commune augmente de 5,30pp la probabilité de percevoir une aide du fonds. Cet effet est donc sensiblement plus élevé.

Nous pouvons interpréter le fait que les critères d'éligibilité ne départagent pas les communes bénéficiaires et non-bénéficiaires du fonds à la lumière de la doctrine élaborée par le comité de suivi. En effet, comme l'indique la Cour des comptes (Rapport de février 2018), le comité a souhaité une analyse des dossiers à l'aune des critères réglementaires, mais sans se dispenser d'une analyse subjective des demandes à partir de leur situation financière. Ainsi, nous pouvons nous attendre à une certaine distance face à cette dimension réglementaire. Toutefois, nous observons très nettement que les critères réglementaires n'ont eu que peu ou pas d'incidence dans la décision d'octroi d'une aide par le fonds de soutien.

Au regard de ces résultats, l'analyse de la dimension réglementaire ne permet pas de valider la proposition (P_1) de stratégie de sauvetage financier par le fonds de soutien. Avant d'écarter définitivement la proposition (P_1), nous considérons la dimension financière complémentaire. Cette dimension repose sur un ensemble d'indicateurs permettant une analyse plus fine de l'état de santé financière de la commune. Il est tout à fait envisageable que le fonds se soit basé sur un ensemble de critères proches de ceux définis dans le décret, mais possédant une plus grande fiabilité d'analyse.

La table III.3 illustre comment ces critères financiers complémentaires peuvent expliquer la probabilité de bénéficier d'une aide du fonds de soutien. Comme pour les critères réglementaires, les colonnes (1) à (5) intègrent progressivement les différentes variables de cette dimension financière. Tout d'abord, nous constatons que cette dimension semble expliquer plus finement la décision d'octroi d'une aide. Ainsi, les communes qui étaient le moins tournées vers l'investissement (capturées par le taux d'équipement) avaient plus de chance de bénéficier d'une aide du fonds. De plus, leur taux d'endettement semble être un facteur important qui influence positivement la probabilité d'obtenir une aide. Les communes qui avaient le plus de difficultés à générer de l'épargne, sur la base de leurs recettes réelles de fonctionnement, ont une plus grande probabilité de recevoir une aide. Enfin, la rigidité des charges de structure est positivement et significativement associée à l'octroi d'une aide. Ces résultats sont de prime abord cohérents. Bénéficie ainsi

Table III.3 – Autres dimensions financières.

Régressions Logit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Autres dimensions comme variables de contrôle :	NON	NON	NON	NON	NON	NON	OUI	Effets margin.
(a) Taux d'équipement	-3.228*** (0.906)	-3.289*** (0.868)	-3.350*** (0.887)	-2.227** (0.838)	-2.008* (0.856)	-2.386* (1.045)	-2.704* (1.279)	-0.152* (0.072)
(b) Taux d'endettement		0.711*** (0.157)	0.673*** (0.165)	0.831*** (0.180)	0.752*** (0.193)	0.585** (0.190)	0.819*** (0.237)	0.046*** (0.014)
(c) Taux d'autofinancement des nouveaux investissements			-0.180 (0.101)	0.204 (0.114)	0.184 (0.125)	0.051 (0.142)	0.124 (0.151)	0.007 (0.008)
(d) Taux de capacité d'autofinancement				-6.775*** (1.474)	-6.061*** (1.461)	-5.055** (1.642)	-4.209* (1.894)	-0.236* (0.106)
(e) Taux de rigidité des charges de structure					3.404* (1.662)	2.772 (1.699)	1.563 (1.923)	0.088 (0.107)
Ratio de surcoût total						9.840*** (1.032)	9.614*** (1.010)	0.540*** (0.061)
Observations	1542	1542	1542	1542	1542	1542	1542	–
AUROC	0.621 (0.024)	0.678 (0.024)	0.687 (0.024)	0.715 (0.023)	0.716 (0.024)	0.861 (0.017)	0.872 (0.017)	–

Notes : La dernière colonne présente les estimations en intégrant l'ensemble des dimensions considérées. La table C.8 en annexe présente l'ensemble des coefficients pour cet exercice. Écart-types robustes entre parenthèses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

d'une aide la commune endettée qui avait des difficultés à investir, à dégager des marges d'autofinancement, et dont la structure de dépenses de fonctionnement est rigide.

Nous contrôlons enfin dans la colonne (7) pour l'ensemble des dimensions et variables de contrôle. Les effets marginaux confirment l'ampleur de l'incidence de ces variables sur la probabilité de bénéficier du fonds. Ainsi, la hausse de 10pp du taux d'équipement diminue de 1,52pp cette probabilité, tandis que 10pp de taux de capacité d'autofinancement supplémentaires diminuent de 2,36pp la probabilité d'accès au fonds. Un taux d'endettement plus élevé de 10pp n'accroît en revanche que de 0,5pp la probabilité d'obtenir une aide du fonds.

Quoiqu'il en soit, ces résultats sont importants car ils permettent de quantifier et de retrouver la stratégie d'allocation du fonds qui se fait donc bien sur une approche financière. Ces résultats permettent de valider la proposition (P₁) de stratégie de sauvetage financier par le fonds de soutien, non pas sur les critères réglementaires mais sur d'autres critères financiers.

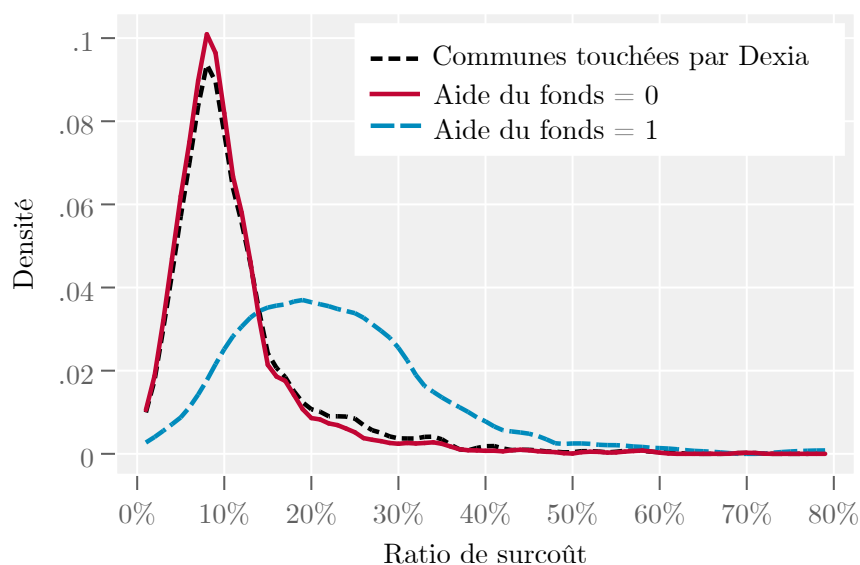


Figure III.4 – Distribution des communes selon leur ratio de surcoût.

5.2 La prénance de la stratégie d'évitement du conflit

Les communes ayant bénéficié d'une aide présentent en moyenne un taux de surcoût sur leurs emprunts structurés Dexia plus de deux fois supérieur à celles n'ayant pas reçu d'aide. Il semblerait donc que cet aspect ait été un facteur déterminant. La figure III.4 présente la distribution des communes en terme de ratio de surcoût. Il existe une différence notable entre les communes ayant bénéficié d'une aide du fonds de soutien et celles n'ayant pas bénéficié d'une telle aide.

Sur la base de nos estimations, la figure III.5 mesure la prédiction moyenne pour les niveaux observés de taux de surcoût supportés par les communes, toutes choses égales par ailleurs. Ce graphique souligne ainsi que, dans notre échantillon, les communes ayant un faible taux de surcoût ont également une faible probabilité d'obtenir une aide du fonds de soutien. Cette probabilité augmente ainsi avec le taux de surcoût supporté par la commune. Elle est d'environ 80% pour les communes présentant un taux de surcoût de 60%.

L'étude des trois dimensions précédentes a mis en évidence que le ratio de surcoût total est systématiquement le facteur déterminant et robuste pour prédire l'octroi d'une aide par le fonds de soutien. Dans la table III.2, le ratio de surcoût est positif et sta-

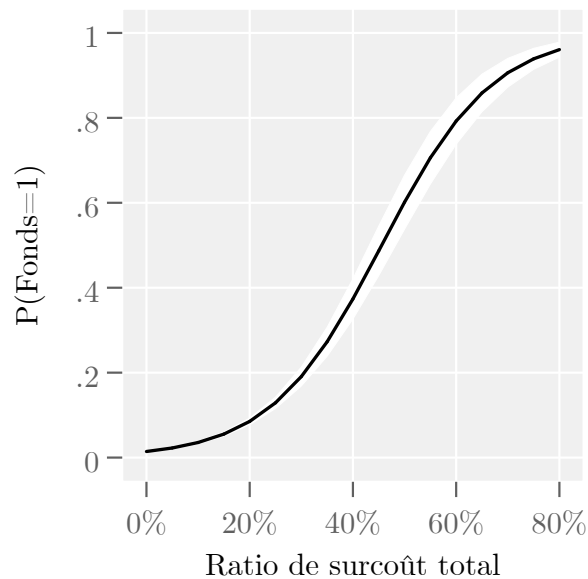


Figure III.5 – Prédiction moyenne d'obtention d'une aide du fonds de soutien pour différents niveaux de surcoûts observés.

tistiquement significatif à 1%. De plus, l'AUC passe de 0,63 pour l'estimation avec l'ensemble de la dimension réglementaire de la colonne (4) à 0,86 pour l'estimation de la colonne (5) intégrant le ratio de surcoût total supporté par la commune.

De manière similaire, dans la table III.3, l'AUC passe de 0,72 pour l'estimation avec l'ensemble de la dimension financière de la colonne (5), à 0,86 pour l'estimation de la colonne (6) intégrant le ratio de surcoût total supporté par la commune. Cet écart souligne une fois encore que le ratio de surcoût est une variable déterminante pour l'octroi d'une aide par le fonds de soutien.

Ainsi ces résultats valident la proposition (P_2) selon laquelle le fonds de soutien a adopté une stratégie d'évitement du contentieux. Le montant du dommage potentiel susceptible de motiver le recours contentieux explique le fait de bénéficier du fonds de garantie.

Nous avons donc montré que le fonds a adopté plusieurs stratégies. Le fonds a bien fonctionné sur la base d'un sauvetage financier, mais avec une certaine liberté vis-à-vis des critères réglementaires. En effet, le fonds de soutien a également opté pour une stratégie d'évitement du contentieux. Au regard de ces stratégies cumulatives, nous pouvons nous demander quelle est l'importance relative de ces différentes stratégies. Si la

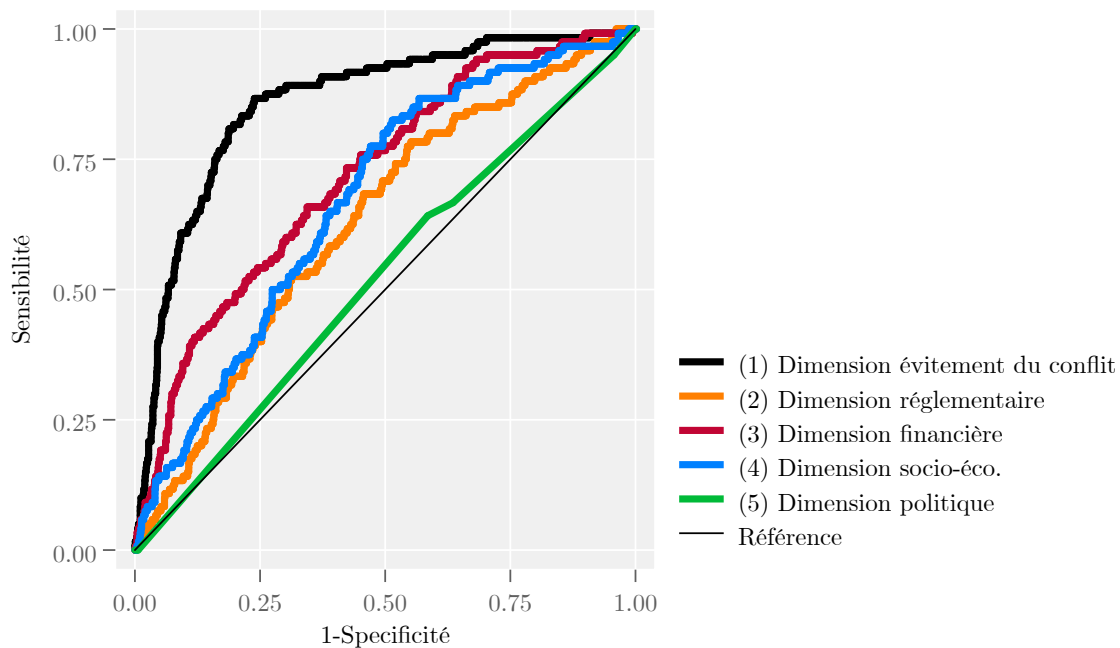
significativité statistique du coefficient est certes importante, nous proposons d'analyser graphiquement la statistique AUROC pour une analyse plus fine de la capacité prédictive des différentes dimensions, et de leur importance relative.

La figure III.6 présente les courbes ROC (Receiver Operating Characteristic) de plusieurs estimations des sections précédentes. L'AUROC mesure l'aire sous la courbe ROC qui présente le taux de vrais positifs (sensibilité) en fonction du taux de faux positifs (antispécificité). C'est ainsi une mesure fiable de la performance d'un classificateur binaire (de type logit dans notre étude).

La courbe ROC noire établit la qualité prédictive du seul ratio de surcoût. Les autres courbes ROC (2) à (4) représentent la performance des estimations fondées sur chacune des trois dimensions étudiées. La supériorité de l'estimation basée sur le seul ratio de surcoût apparaît immédiatement. Cette performance est également soulignée par une statistique AUROC de 0,86 qui est bien supérieure à celle obtenue par les modèles basés sur chacune des autres dimensions.

Bien que le fonds semble avoir adopté des stratégies cumulatives, ce résultat permet d'établir que la stratégie d'évitement du conflit (P_2) est la stratégie dominante du fonds. Ainsi, s'agissant de déterminer quelle fut la stratégie d'allocation du fonds, il ressort de notre étude que la stratégie dominante adoptée par le fonds, plus que le sauvetage financier, a été l'évitement du conflit.

Figure III.6 – Receiving Operator Curves : l'évitement du conflit (P_2) est la stratégie dominante.



Notes : Le modèle (1) correspond à une régression logit pour dimension évitement du conflit. Le modèle (2) est une régression logit avec pour variables explicatives l'ensemble des quatre variables réglementaires. Ce modèle reprend la régression présentée à la table III.2, colonne (4). Le modèle (3), présenté dans la colonne 5 de la table III.3, se concentre sur les autres dimensions financières pertinentes. Le modèle (4) évalue l'importance de la dimension socio-économique. Il est présenté dans la colonne (4) de la table C.6. Enfin, le modèle (5) évalue la dimension politique présentée dans la colonne (3) de la table C.5. Les courbes ROC de ces différents modèles sont représentées dans la figure ci-dessus.

Nous pouvons prolonger en nous demandant si les autres dimensions (réglementaire, financière et socio-économique) ne peuvent pas toutefois affiner la qualité prédictive du modèle fondé sur la seule dimension évitement du conflit. Pour cela, nous avons comparé l'estimation avec uniquement le ratio de surcoût aux estimations de ce même ratio augmenté successivement de chacune des dimensions. La figure C.4 et la table C.7 en annexe synthétisent cette démarche. Il ressort que les autres dimensions ne permettent pas d'améliorer la capacité prédictive du modèle fondé sur le seul ratio de surcoût.

6 Conclusion

Table III.4 – Synthèse des résultats par proposition.

Hypothèse	Résultat
<p>Proposition (p₁) : sauvetage financier La dégradation de la situation financière de la commune explique le bénéfice d'une aide du fonds de garantie.</p>	<p>→ VALIDÉE Mais pas sur la base des critères réglementaires</p>
<p>Proposition (p₂) : évitement du contentieux Le montant du dommage potentiel susceptible de motiver le recours contentieux explique le fait de bénéficier du fonds de garantie.</p>	<p>→ VALIDÉE Stratégie dominante</p>

Ce travail économétrique a permis d'analyser la stratégie du fonds de soutien aux collectivités dans le cadre de la crise Dexia. Nous avons pu identifier que la stratégie d'évitement du contentieux avait, semble-t-il, été privilégiée par le fonds. La prégnance du ratio de surcoût comme prédicteur du bénéfice du fonds dans nos estimations (alors même qu'il ne figure pas en principe dans les critères d'octroi du fonds) coïncide avec sa mise en exergue par les juridictions judiciaires pour sanctionner les contrats Dexia. Ceci valide notre proposition (P₂) qui s'inscrit dans la littérature traditionnelle en économie des conflits.

La prévalence de ce critère permet de comprendre davantage comment le fonds de soutien a choisi de verser une aide aux communes les plus susceptibles de se pourvoir en justice pour peser dans l'arbitrage des communes. Celles-ci ont alors le choix entre opter pour une aide du fonds ou bien parier sur une victoire contentieuse qui, si elle peut être plus avantageuse, n'en reste pas moins incertaine.

À l'inverse, la plus faible prégnance des critères financiers pour prédire le recours au fonds suggère que les communes qui ont été aidées n'étaient pas nécessairement celles qui étaient le plus en difficulté financièrement. Le fonds, sur ce point, semble s'être écarté de l'application des critères légaux. En ce sens, l'aide a été distribuée en partie sur une approche discrétionnaire en s'éloignant du cadre légal. Enfin, le fait que la dimension politique (Table C.5) n'explique pas l'octroi d'une aide du fonds laisse penser que les

critères retenus n'ont pas été supplantés par des considérations politiques.

Dans ces conditions, notre travail contribue à la littérature sur les sauvetages financiers en apportant un éclairage sur les choix d'attribution d'une aide en amont du processus de sauvetage. De plus, le comportement d'arbitrage entre l'accord amiable et le recours contentieux ancre cette étude dans la littérature de l'économie des conflits et de l'économie du droit. La prégnance du ratio de surcoût rend compte d'une volonté, d'une part, d'éteindre au plus vite la crise Dexia, afin d'éviter des contentieux judiciaires longs et coûteux ; d'autre part, d'opérer une modification de la charge de ces emprunts toxiques en proposant une répartition entre les collectivités locales et l'État que ne permettait pas l'application de la jurisprudence du TGI de Nanterre qui faisait peser intégralement le coût de la crise sur l'État.

Ces résultats sont donc particulièrement utiles pour comprendre à la fois les différents comportements stratégiques lors des sauvetages financiers d'agents économiques, mais aussi pour éclairer les jeux qui peuvent exister entre les acteurs publics eux-mêmes, ici les gouvernements locaux face au gouvernement central. L'étude des fonds de soutien pose donc des questions essentielles, non seulement sur leurs conséquences, mais également sur leur stratégie de sauvetage au travers du choix des agents à sauver. Au regard de nos résultats, nous pouvons affirmer que les fonds de soutien ne visent pas uniquement le sauvetage des agents touchés par une crise. En effet, nos analyses empiriques montrent qu'un fonds de soutien peut jouer un rôle central de répartition de la charge de la crise entre les acteurs : il sert de catalyseur.

Enfin, un travail économétrique complémentaire pourrait être mené pour analyser les coûts cachés qu'ont impliqué le recours ou le non-recours au fonds : quelles contreparties en terme de contrôle budgétaire et financier ? Quel coût, *in fine*, pour les communes ayant choisi l'option contentieuse ? À mesure que nous prenons du recul sur la crise Dexia, les données qui remontent progressivement permettront de lever petit à petit le voile sur les gains ou les pertes des différents acteurs. En définitive, les communes ayant refusé l'aide du fonds ont-elles eu raison ? Un travail économétrique supplémentaire permettrait de confirmer ou d'infirmer le célèbre adage d'Honoré de Balzac « *un mauvais arrangement vaut mieux qu'un bon procès* ».

Conclusion et perspectives

« Any change in policy will systematically alter the structure of econometric models. »

Robert Lucas (1976)

Rappel des objectifs

Dans cette thèse, structurée autour de trois chapitres, nous avons étudié les conséquences de l'expansion financière. L'analyse qui y a été conduite s'inscrit majoritairement dans le champ de la macroéconomie financière, mais propose également une incursion microéconomique. Ainsi les trois chapitres proposent une contribution en adoptant trois angles d'approches différents mais complémentaires.

Ce travail s'articule notamment autour d'un double objectif. Tout d'abord, un questionnement économique qui interroge notre compréhension des conséquences de l'expansion de la sphère financière, initiée à la fin des Trente Glorieuses comme nouveau ressort d'une croissance alors en perte de vitesse. De manière plus incidente, ce travail se penche aussi sur la problématique de la mesure des phénomènes étudiés, portant une réévaluation de la capacité de la science économique à mesurer des régularités par le biais des outils économétriques.

Nous présenterons ici dans un premier temps les principaux résultats et contributions à tirer de ces trois chapitres, pour évoquer ensuite les perspectives de recherche qu'ils offrent.

Principaux résultats et contributions

Nous avons exposé en introduction que l’essor de la recherche empirique s’était construit sur l’émergence de méthodes à variables instrumentales “internes”. Nous constatons que ces méthodes obèrent toute considération quant à la qualité de l’instrumentation sous-jacente. Ce constat offre ainsi un point de départ pour cette thèse, dont la première contribution, par ordre et non par importance, est méthodologique.

Le premier chapitre prolonge donc ainsi sur le volet empirique un débat initié par Romer (2016), poursuivi par Chatelain et Ralf (2018b) et Blanchard (2018) sur les dérives de la macroéconomie comme discipline scientifique :

“The treatment of identification now is no more credible than in the early 1970s but escapes challenge because it is so much more opaque.”
(Romer, 2016, p. 1)

Il est reproché à la macroéconomie théorique son manque de transparence et sa complexité, brouillant l’interprétation et la compréhension des conclusions. Cette “pathologie de l’identification”, pour reprendre les mots de Paul Romer, n’est donc pas circonscrite au seul champ théorique. Le problème soulevé par l’instrumentation, notamment les instruments faibles, n’est pas nouveau. Pourtant, de nombreux travaux de recherche semblent oublier ce qui a été découvert concernant les problèmes – bien réels – d’identification. La démarche d’évaluation étant moins aisée, l’hypothèse qu’elle n’est pas nécessaire est souvent retenue. En effet, les travaux basés sur le System-GMM proposant une évaluation complète de la qualité de l’instrumentation, sont rares, voire inexistants. Et ce, malgré les mises en garde répétées depuis plusieurs années.

Le premier chapitre, en conduisant une analyse approfondie de la méthodologie qui guide une grande partie de la littérature empirique sur la relation finance-croissance, met en lumière un certain nombre de problèmes qui remettent en question la fiabilité des résultats antérieurs, dont notamment la conclusion selon laquelle la relation entre expansion financière et croissance économique serait non-linéaire. De plus, cet approfondissement méthodologique offre également un éclairage pour une variété de sujets reposant sur ces méthodes d’identification, au-delà de la seule littérature macro-financière.

Ainsi, notre première conclusion est que les panels macroéconomiques sont particu-

lièrement exposés aux instruments faibles. L'utilisation d'estimateurs à variables instrumentales internes ne peut donc s'abstraire d'une évaluation de la qualité de l'instrumentation. Nous proposons également dans ce premier chapitre, un certain nombre de tests sur la qualité des instruments, facilement accessibles. Ces tests conduisent systématiquement à révéler la faiblesse des instruments dans les panels macroéconomiques. Ainsi, ne pas traiter ce problème sur le fondement qu'aucun test standard n'existe pas conduit à retenir des résultats pouvant être substantiellement erronés.

A partir de cette première étude qui analyse notre capacité à mesurer les conséquences macroéconomiques de l'expansion financière, nous avons voulu approfondir la question, d'un point de vue économique cette fois-ci, en tenant compte des limites méthodologiques précédemment soulevées. Cette démarche débute ainsi dans le chapitre 1 et se poursuit dans les chapitres 2 et 3, chacun offrant un éclairage différent sur les conséquences de l'expansion financière.

C'est ainsi que le premier chapitre, après avoir démontré que les effets de seuils dans la relation croissance-finance sont trompeurs, propose une estimation alternative tenant compte au mieux des différentes limites. Ces estimations concluent à un effet globalement négatif de l'expansion de crédit au secteur privé sur la croissance, soulignant par cette occasion que l'effet négatif a eu tendance à s'intensifier dans le temps, et apparaît comme plus prononcé dans les économies avancées.

Le chapitre 2 de cette thèse s'inscrit dans le prolongement du chapitre 1 en analysant sous un angle différent les conséquences de l'expansion financière. Il propose une analyse jointe de l'endettement privé et public pour appréhender plus pleinement l'expansion financière, jusqu'alors étudiée soit sous l'angle de crédit au secteur privé, soit de l'endettement public. Dans l'ensemble, les conclusions invitent à contenir le niveau des dettes privées et publiques.

Nos estimations soulignent que l'endettement des ménages, des entreprises ou du secteur public ne produit pas les mêmes conséquences. Certains types d'endettement étant nettement plus préjudiciables à la croissance que d'autres. Ainsi, l'approfondissement du crédit au secteur privé exerce un effet négatif, robuste et important sur la croissance future, rejoignant ainsi une littérature maintenant abondante documentant un tel ef-

fet. Nous montrons de plus que la relation entre finance et croissance s'est dégradée au cours du temps, avec un effet négatif qui s'est intensifié. Deux éléments semblent pouvoir éclairer ce phénomène.

Tout d'abord, nous montrons que l'occurrence de crises financières, qui trouvent leurs racines dans l'expansion de la sphère financière, explique une part importante de cet effet négatif du crédit au secteur privé. En effet, une expansion beaucoup plus importante du crédit au secteur privé précède et caractérise les crises financières, par rapport aux récessions normales. La survenue de crises financières de très grande ampleur, sans précédent dans l'histoire contemporaine, ne peut que renforcer l'effet négatif observé.

Ensuite, l'étude de la structure financière montre que le crédit alloué aux ménages est de loin le plus dommageable, rejoignant les conclusions de Beck et al. (2012) sur ce point. En effet, le crédit alloué aux ménages limiterait la consommation future et favoriserait l'émergence de bulles immobilières (Mian et Sufi, 2011 ; Mian, Sufi et Verner, 2017 ; Mian, Straub et Sufi, 2020). L'effet négatif du crédit au secteur privé a ainsi pu se renforcer dans le temps à mesure que la structure financière a connu une modification profonde avec l'essor du crédit destiné aux ménages.

Par contraste, la dette publique n'est généralement pas associée significativement à la croissance future. Elle peut néanmoins exercer une influence positive à des niveaux faibles. En revanche, nous montrons que des niveaux plus élevés de dette publique renforcent l'effet néfaste du crédit alloué au secteur privé. La dette publique tend à freiner la croissance, non pas initialement, mais à la suite d'une crise financière, si le gouvernement est incapable de mener une politique budgétaire contra-cyclique. Un niveau élevé d'endettement public au début de la crise accentue donc les conséquences de l'expansion du crédit privé en renforçant l'incidence de la crise. Le cloisonnement – sans porosité – dans la littérature financière entre le crédit au secteur privé et la dette publique masque ainsi des interactions significatives et substantielles, appelant à des recherches supplémentaires sur ce sujet.

En préconisant de maintenir les niveau d'endettement bas en période d'expansion, nous ne rejoignons que très partiellement les conclusions de Reinhart, Rogoff et Savastano (2003) et Reinhart et Rogoff (2010). En effet, là où leurs travaux considèrent qu'il

existe un “seuil d’intolérance” pour la dette publique posant une limite structurelle à l’endettement, nous ne faisons pas tant état d’un seuil que de la nécessité de préserver un espace fiscal. La conclusion est ici plutôt que les gouvernements doivent veiller à préserver leur capacité à conduire une politique budgétaire contra-cyclique lors d’éventuels chocs économiques, dans une logique d’essence keynésienne. Cela nécessite généralement de reconstituer des capacités d’endettement lors des phases ascendantes du cycle économique, afin d’éviter l’émergence d’une dynamique insoutenable de la dette publique.

Ainsi, nos conclusions rejoignent les travaux récents de Pescatori, Sandri et Simon (2014), Chudik et al. (2016) et Bernardini et Forni (2017), en soulignant que la trajectoire de l’endettement public est ici l’élément déterminant, plus encore que le niveau d’endettement. Aussi, au regard du contexte actuel, nos conclusions viennent en soutien des préconisations de Blanchard (2019) et plus récemment Blanchard et Summers (2020) qui invitent les décideurs publics à s’engager dans des politiques de relance contra-cyclique dans l’environnement actuel de taux bas. Au regard de la crise sanitaire et économique actuelle, d’une ampleur considérable, nos conclusions invitent les décideurs publics à s’engager dans des plans de relance ambitieux. Une telle démarche a d’ailleurs été mise en place dans de nombreuses économies dont les niveaux d’endettement pouvaient jusqu’ici faire craindre une réticence face à de telles politiques contra-cycliques. Dans la zone euro, les taux souverains, maintenus bas par une politique monétaire expansionniste de la Banque centrale européenne, ont certainement contribué à cette issue.

Le dernier chapitre offre un changement d’échelle d’analyse, permettant de dépasser les difficultés de la mesure d’un effet macroéconomique en adoptant un prisme différent. En basculant sur une approche microéconomique visant à évaluer une politique publique, ce travail offre une illustration d’une dérive de la libéralisation financière engagée en France dans les années 1980 pour stimuler le financement des communes.

En étudiant le sauvetage financier des communes exposées aux emprunts toxiques de la banque Dexia, nous montrons que l’octroi d’une aide a été le théâtre d’une stratégie d’évitement d’un contentieux qui aurait été coûteux pour l’État. La plus faible prégnance des critères financiers pour prédire l’octroi d’une aide du fonds suggère que les communes qui ont été aidées n’étaient pas nécessairement celles qui étaient le plus en

difficulté financièrement. En ce sens, l'aide a été distribuée en partie sur une approche discrétionnaire en s'éloignant du cadre légal qui visait en principe des critères de santé financière.

Dans ces conditions, notre travail contribue à la compréhension des sauvetages financiers en apportant un éclairage sur les choix d'attribution d'une aide en amont du processus de sauvetage. La stratégie adoptée ici rend compte d'une volonté, d'une part, d'éteindre au plus vite la crise Dexia, afin d'éviter des contentieux judiciaires longs et coûteux ; d'autre part, d'opérer une modification de la charge de ces emprunts toxiques en proposant une répartition entre les collectivités locales et l'État.

Ces résultats sont donc particulièrement utiles pour comprendre à la fois les différents comportements stratégiques lors des sauvetages financiers d'agents économiques, mais aussi pour éclairer les jeux qui peuvent exister entre les acteurs publics eux-mêmes, ici les gouvernements locaux face au gouvernement central. L'étude des fonds de soutien pose donc des questions essentielles, non seulement sur leurs conséquences, mais également sur leur stratégie de sauvetage au travers du choix des agents à sauver. Au regard de nos résultats, nous pouvons affirmer que les fonds de soutien ne visent pas uniquement le sauvetage des agents touchés par une crise financière. En effet, nos analyses empiriques montrent qu'un fonds de soutien peut jouer un rôle central de répartition de la charge de la crise entre les acteurs : il sert de catalyseur.

*
* *

Enfin, au regard des analyses conduites tout au long de la présente thèse, nous pouvons conclure que l'expansion financière, initiée à la fin des Trente glorieuses par un processus de libéralisation pour mettre fin à la "répression financière", a produit des résultats bien différents de ceux qui étaient escomptés. En effet, le système financier devait ainsi permettre de mutualiser des ressources, assurer le transfert de celles-ci dans le temps et dans l'espace, assurer la liquidité des investissements, fournir des mécanismes de gestion des risques, révéler l'information et créer des outils permettant de résoudre les problèmes d'incitation (Merton, 1990). En somme, le système financier devait agir

comme un nouveau ressort de croissance. La réalité a semble-t-il été bien différente. Il est probable que la politique de libéralisation ait dissout les avantages initiaux associés à l'expansion financière, dans une logique rejoignant la critique de Lucas de l'intervention économique (Lucas, 1976, p.40). Ainsi, comme le soulignait plus récemment Mervyn King, alors gouverneur de la Banque d'Angleterre :

« En économie, il existe probablement très peu de paramètres ou de relations authentiquement “profonds” (et donc stables), par opposition aux sciences physiques, où les lois de la gravité sont une approximation de la réalité aussi fiable hier que demain ». (King, 2010, p. 4)

Perspectives de recherches

Au terme de cette thèse, plusieurs pistes de recherches semblent intéressantes à explorer pour prolonger ce travail et en combler certaines limites.

D'une manière générale, la contribution méthodologique conduite dans le premier chapitre invite à reconsidérer un champ très varié de conclusions empiriques reposant sur des estimations à variables instrumentales internes. Plus spécifiquement, si ce chapitre infirme la non-monotonie dans la relation croissance-finance qui serait fondée sur le niveau de développement financier, les travaux qui y sont conduits n'épuisent pas notre compréhension des mécanismes, soulignant au contraire leur grande complexité.

Par ailleurs, au regard de la critique de Lucas (1976) évoquée précédemment, un prolongement intéressant consisterait à évaluer plus précisément comment la libéralisation financière a transformé la relation entre finance et croissance. Le travail de Rousseau et Wachtel (2011) constitue sur ce sujet un point d'entrée intéressant. Leurs conclusions, bien que mitigées, reposent sur une mesure de la libéralisation financière qui mériterait d'être actualisée.

Ensuite, nous avons étudié deux types de chocs économiques dans le deuxième chapitre : les récessions et les crises financières. Un prolongement intéressant consisterait à intégrer les récentes données de Romer et Romer (2017) mesurant l'intensité du choc économique. Cela permettrait de mieux tenir compte de l'hétérogénéité des crises financières notamment.

Enfin, le dernier chapitre ne constitue que la première pièce d'un travail plus large sur

la crise Dexia et son incidence sur les finances publiques locales. Dans le prolongement de ce chapitre sur le sauvetage financier, un projet en cours nous amène à étudier les choix d'investissement et de financement des communes à la suite de leur exposition aux emprunts toxiques. L'objectif est ici de déterminer, d'une part, si les communes qui ont détenu des emprunts toxiques réduisent leur dépendance aux modes de financement externes – l'emprunt notamment – pour privilégier l'autofinancement ; d'autre part, si ces communes ont réduit leurs dépenses d'investissement, mesurant ainsi les effets réels des emprunts toxiques.

Annexes

Annexes du chapitre 1

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1 Data Appendix

The data appendix consists of four tables (Tables A.1 to A.4) listing the countries within the different samples used throughout this chapter. For clarity, they are grouped by income group following the World Bank classification.

The number of countries in the cross-country regressions in section 3 is substantially smaller than the subsequent panel estimates of sections 4 and 5. This sample difference rests in the cross-country regressions structure, consisting of regressing the average annual growth observed over the sample with a combination of average observation and initial values. The latter requires observations at the beginning of the sample, which is not available for most countries. To mitigate the issue, I follow Arcand, Berkes et Panizza (2015) and start a decade later, in 1970, instead of 1960, to maximize the sample size. Once turning to panel estimates, this constraint no longer burdens the sample size.

Table A.1 – Various samples in the chapter. (1/4) : Low income.

Country ISO-code	Income group	CROSS-COUNTRY			PANEL		
		Old 1970-2010	New 1970-2010	New 1970-2015	Old 1960-2010	New 1960-2010	New 1960-2015
AFG	Low income						✓
BDI	Low income	✓	✓	✓	✓	✓	✓
BEN	Low income				✓	✓	✓
CAF	Low income				✓	✓	✓
GMB	Low income				✓	✓	✓
HTI	Low income				✓	✓	✓
LBR	Low income				✓	✓	✓
MLI	Low income				✓	✓	✓
MOZ	Low income				✓	✓	✓
MWI	Low income				✓	✓	✓
NER	Low income		✓	✓	✓	✓	✓
NPL	Low income				✓	✓	✓
RWA	Low income		✓	✓	✓	✓	✓
SEN	Low income	✓	✓	✓	✓	✓	✓
SLE	Low income	✓	✓	✓	✓	✓	✓
TGO	Low income		✓	✓	✓	✓	✓
TZA	Low income				✓	✓	✓
UGA	Low income				✓	✓	✓
ZWE	Low income				✓	✓	✓
	Total	3	6	6	18	18	19
	Overall sample total	64	77	74	132	137	140

Table A.2 – Various samples in the chapter. (2/4) : Lower middle income.

Country ISO-code	Income group	CROSS-COUNTRY			PANEL		
		Old 1970-2010	New 1970-2010	New 1970-2015	Old 1960-2010	New 1960-2010	New 1960-2015
ARM	Lower middle inc.				✓	✓	✓
BGD	Lower middle inc.				✓	✓	✓
BOL	Lower middle inc.	✓	✓	✓	✓	✓	✓
CIV	Lower middle inc.	✓	✓	✓	✓	✓	✓
CMR	Lower middle inc.	✓	✓	✓	✓	✓	✓
COG	Lower middle inc.	✓	✓	✓	✓	✓	✓
EGY	Lower middle inc.	✓	✓	✓	✓	✓	✓
GHA	Lower middle inc.	✓	✓	✓	✓	✓	✓
GTM	Lower middle inc.	✓	✓	✓	✓	✓	✓
HND	Lower middle inc.	✓	✓	✓	✓	✓	✓
IDN	Lower middle inc.	✓	✓	✓	✓	✓	✓
IND	Lower middle inc.	✓	✓	✓	✓	✓	✓
JOR	Lower middle inc.				✓	✓	✓
KEN	Lower middle inc.	✓	✓	✓	✓	✓	✓
KGZ	Lower middle inc.				✓	✓	✓
KHM	Lower middle inc.				✓	✓	✓
LAO	Lower middle inc.				✓	✓	✓
LKA	Lower middle inc.	✓	✓	✓	✓	✓	✓
LSO	Lower middle inc.				✓	✓	✓
MAR	Lower middle inc.	✓	✓	✓	✓	✓	✓
MDA	Lower middle inc.					✓	✓
MMR	Lower middle inc.						✓
MNG	Lower middle inc.				✓	✓	✓
MRT	Lower middle inc.				✓	✓	✓
NIC	Lower middle inc.				✓	✓	✓
PAK	Lower middle inc.	✓	✓	✓	✓	✓	✓
PHL	Lower middle inc.	✓	✓	✓	✓	✓	✓
PNG	Lower middle inc.				✓	✓	✓
SDN	Lower middle inc.	✓	✓	✓	✓	✓	✓
SLV	Lower middle inc.	✓	✓	✓	✓	✓	✓
SWZ	Lower middle inc.		✓	✓	✓	✓	✓
SYR	Lower middle inc.	✓	✓		✓	✓	✓
TJK	Lower middle inc.					✓	✓
TUN	Lower middle inc.				✓	✓	✓
UKR	Lower middle inc.				✓	✓	✓
VNM	Lower middle inc.				✓	✓	✓
YEM	Lower middle inc.					✓	✓
ZMB	Lower middle inc.				✓		✓
	Total	18	19	19	34	36	38
	Overall sample total	64	77	74	132	137	140

Table A.3 – Various samples in the chapter. (3/4) : Upper middle income.

Country ISO-code	Income group	CROSS-COUNTRY			PANEL		
		Old 1970-2010	New 1970-2010	New 1970-2015	Old 1960-2010	New 1960-2010	New 1960-2015
ALB	Upper middle inc.				✓	✓	✓
ARG	Upper middle inc.	✓	✓	✓	✓	✓	✓
BGR	Upper middle inc.				✓	✓	✓
BLZ	Upper middle inc.				✓	✓	✓
BRA	Upper middle inc.		✓	✓	✓	✓	✓
BWA	Upper middle inc.				✓	✓	✓
CHN	Upper middle inc.				✓	✓	✓
COL	Upper middle inc.	✓	✓	✓	✓	✓	✓
CRI	Upper middle inc.	✓	✓	✓	✓	✓	✓
DOM	Upper middle inc.	✓	✓	✓	✓	✓	✓
DZA	Upper middle inc.				✓	✓	✓
ECU	Upper middle inc.	✓	✓	✓	✓	✓	✓
FJI	Upper middle inc.	✓	✓	✓	✓	✓	✓
GAB	Upper middle inc.	✓	✓	✓	✓	✓	✓
GUY	Upper middle inc.		✓	✓	✓	✓	✓
HRV	Upper middle inc.				✓	✓	✓
IRN	Upper middle inc.	✓	✓	✓	✓	✓	✓
IRQ	Upper middle inc.		✓	✓		✓	✓
JAM	Upper middle inc.	✓	✓	✓	✓	✓	✓
KAZ	Upper middle inc.				✓	✓	✓
LBY	Upper middle inc.				✓	✓	✓
MEX	Upper middle inc.	✓	✓	✓	✓	✓	✓
MUS	Upper middle inc.				✓	✓	✓
MYS	Upper middle inc.	✓	✓	✓	✓	✓	✓
NAM	Upper middle inc.				✓	✓	✓
PAN	Upper middle inc.		✓	✓	✓	✓	✓
PER	Upper middle inc.	✓	✓	✓	✓	✓	✓
PRY	Upper middle inc.	✓	✓	✓	✓	✓	✓
ROU	Upper middle inc.					✓	✓
RUS	Upper middle inc.				✓	✓	✓
SRB	Upper middle inc.					✓	✓
THA	Upper middle inc.	✓	✓	✓	✓	✓	✓
TON	Upper middle inc.				✓	✓	✓
TUR	Upper middle inc.		✓	✓	✓	✓	✓
VEN	Upper middle inc.	✓	✓	✓	✓	✓	✓
ZAF	Upper middle inc.	✓	✓	✓	✓	✓	✓
	Total	16	21	21	33	36	36
	Overall sample total	64	77	74	132	137	140

Table A.4 – Various samples in the chapter. (4/4) : High income.

Country ISO-code	Income group	CROSS-COUNTRY			PANEL		
		Old 1970-2010	New 1970-2010	New 1970-2015	Old 1960-2010	New 1960-2010	New 1960-2015
AUS	High income	✓	✓	✓	✓	✓	✓
AUT	High income	✓	✓	✓	✓	✓	✓
BEL	High income	✓	✓	✓	✓	✓	✓
BHR	High income				✓	✓	✓
BRB	High income				✓	✓	✓
BRN	High income				✓	✓	✓
CAN	High income	✓	✓		✓	✓	✓
CHE	High income	✓	✓	✓	✓	✓	✓
CHL	High income	✓	✓	✓	✓	✓	✓
CYP	High income				✓	✓	✓
CZE	High income				✓	✓	✓
DEU	High income		✓	✓	✓	✓	✓
DNK	High income	✓	✓	✓	✓	✓	✓
ESP	High income	✓	✓	✓	✓	✓	✓
EST	High income				✓	✓	✓
FIN	High income	✓	✓	✓	✓	✓	✓
FRA	High income	✓	✓	✓	✓	✓	✓
GBR	High income	✓	✓	✓	✓	✓	✓
GRC	High income	✓	✓	✓	✓	✓	✓
HKG	High income				✓	✓	✓
HUN	High income				✓	✓	✓
IRL	High income	✓	✓	✓	✓	✓	✓
ISL	High income	✓	✓	✓	✓	✓	✓
ISR	High income	✓	✓	✓	✓	✓	✓
ITA	High income	✓	✓	✓	✓	✓	✓
JPN	High income	✓	✓	✓	✓	✓	✓
KOR	High income		✓	✓	✓	✓	✓
KWT	High income				✓	✓	✓
LTU	High income				✓	✓	✓
LUX	High income	✓	✓	✓	✓	✓	✓
LVA	High income				✓	✓	✓
MAC	High income				✓	✓	✓
MLT	High income	✓	✓	✓	✓	✓	✓
NLD	High income	✓	✓	✓	✓	✓	✓
NOR	High income	✓	✓	✓	✓	✓	✓
NZL	High income	✓	✓		✓	✓	✓
POL	High income				✓	✓	✓
PRT	High income	✓	✓	✓	✓	✓	✓
QAT	High income				✓	✓	✓
SAU	High income		✓	✓	✓	✓	✓
SGP	High income		✓	✓	✓	✓	✓
SVK	High income				✓	✓	✓
SVN	High income				✓	✓	✓
SWE	High income	✓	✓	✓	✓	✓	✓
TTO	High income	✓	✓	✓	✓	✓	✓
URY	High income	✓	✓	✓	✓	✓	✓
USA	High income	✓	✓	✓	✓	✓	✓
	Total	27	31	29	47	47	47
	Overall sample total	64	77	74	132	137	140

Table A.5 – Correlation matrix.

CROSS-SECTIONAL 1970-2015 NEW sample of 74 observations							
	Growth	Log(in.GDP/cap)	Priv. Credit	(Priv. Credit) ²	Log(school)	Log(infl.)	Log(trade)
Log(imit.GDP/cap)	-0.0101	1					
Private Credit	0.275**	0.706***	1				
(Private Credit) ²	0.189	0.607***	0.953***	1			
Log(school)	0.349***	0.784***	0.657***	0.540***	1		
Log(inflation)	-0.133	-0.239**	-0.505***	-0.465***	-0.127	1	
Log(trade)	0.253**	0.129	0.148	0.034	0.215*	-0.429***	1
Log(gov. cons.)	-0.108	0.524***	0.386***	0.274**	0.361***	-0.284**	0.229*
CROSS-SECTIONAL 1970-2010 OLD sample of 64 observations							
	Growth	Log(in.GDP/cap)	Priv. Credit	(Priv. Credit) ²	Log(school)	Log(infl.)	Log(trade)
Log(imit.GDP/cap)	0.0456	1					
Private Credit	0.282**	0.735***	1				
(Private Credit) ²	0.200	0.659***	0.964***	1			
Log(school)	0.297**	0.816***	0.653***	0.564***	1		
Log(inflation)	-0.241*	-0.254**	-0.531***	-0.498***	-0.153	1	
Log(trade)	0.207	0.149	0.153	0.0602	0.246*	-0.368***	1
Log(gov. cons.)	0.0535	0.564***	0.495***	0.369***	0.466***	-0.362***	0.294**
PANEL 1960-2010 OLD sample of 917 observations							
	Growth	Log(in.GDP/cap)	Priv. Credit	(Priv. Credit) ²	Log(school)	Log(infl.)	Log(trade)
Log(imit.GDP/cap)	0.087***	1					
Private Credit	0.007	0.675***	1				
(Private Credit) ²	-0.034	0.534***	0.929***	1			
Log(school)	0.168***	0.719***	0.511***	0.398***	1		
Log(inflation)	-0.110***	-0.165***	-0.276***	-0.234***	-0.020	1	
Log(trade)	0.059*	0.192***	0.206***	0.141***	0.308***	-0.167***	1
Log(gov. cons.)	-0.128***	0.348***	0.274***	0.187***	0.274***	-0.0913***	0.322***
PANEL 1960-2015 NEW sample of 1,055 observations							
	Growth	Log(in.GDP/cap)	Priv. Credit	(Priv. Credit) ²	Log(school)	Log(infl.)	Log(trade)
Log(imit.GDP/cap)	0.013	1					
Private Credit	-0.008	0.646***	1				
(Private Credit) ²	-0.043	0.510***	0.936***	1			
Log(school)	0.134***	0.706***	0.521***	0.407***	1		
Log(inflation)	-0.095***	-0.164***	-0.291***	-0.262***	-0.060*	1	
Log(trade)	0.055*	0.208***	0.211***	0.138***	0.286***	-0.190***	1
Log(gov. cons.)	-0.089***	0.388***	0.266***	0.189***	0.299***	-0.073**	0.266***

Table A.6 – Summary Statistics.

CROSS-SECTIONAL 1970-2015 NEW SAMPLE	Mean	σ	Obs.	Min	Max
Growth	1.779	1.228	74	-0.951	5.741
Log(init.GDP/cap)	8.255	1.384	74	5.581	10.79
Private Credit	0.465	0.334	74	0.044	1.547
(Private Credit) ²	0.326	0.441	74	0.002	2.394
Log(school)	1.683	0.555	74	-0.213	2.509
Log(inflation)	2.326	1.084	74	0.885	5.710
Log(trade)	4.129	0.549	74	3.013	5.836
Log(gov. cons.)	2.686	0.285	74	2.022	3.358
CROSS-SECTIONAL 1970-2010 OLD SAMPLE	Mean	σ	Obs.	Min	Max
Growth	1.745	1.103	64	-1.225	4.530
Log(init.GDP/cap)	7.789	1.458	64	4.841	10.14
Private Credit	0.479	0.343	64	0.042	1.366
(Private Credit) ²	0.346	0.448	64	0.002	1.865
Log(school)	1.719	0.529	64	0.346	2.502
Log(inflation)	2.386	1.025	64	1.031	5.844
Log(trade)	4.073	0.481	64	3.023	5.358
Log(gov. cons.)	2.673	0.308	64	1.937	3.461
PANEL 1960-2015 NEW SAMPLE	Mean	σ	Obs.	Min	Max
Growth	2.108	2.765	1055	-16.84	14.28
Log(init.GDP/cap)	8.432	1.492	1055	5.139	11.56
Private Credit	0.399	0.366	1055	0.011	2.261
(Private Credit) ²	0.294	0.556	1055	0.001	5.113
Log(school)	2.331	0.667	1055	0.371	3.291
Log(inflation)	2.475	1.378	1055	-2.065	10.06
Log(trade)	4.127	0.641	1055	-1.743	6.047
Log(gov. cons.)	2.643	0.372	1055	1.143	3.772
PANEL 1960-2010 OLD SAMPLE	Mean	σ	Obs.	Min	Max
Growth	2.024	2.766	917	-21.00	13.86
Log(init.GDP/cap)	7.796	1.548	917	4.606	10.89
Private Credit	0.400	0.370	917	0.007	2.698
(Private Credit) ²	0.297	0.576	917	0.000	7.277
Log(school)	2.278	0.671	917	0.265	3.274
Log(inflation)	2.495	1.211	917	-3.564	6.908
Log(trade)	4.118	0.599	917	2.049	6.082
Log(gov. cons.)	2.653	0.390	917	1.169	3.828

2 A Rigorous Between Dimension

Following Dormont (2007), consider two linear operators in $\mathbb{R}^T : \frac{J_T}{T}$ where J_T is the square matrix composed of 1 and $I_T - \frac{J_T}{T}$ where I_T is the identity matrix :

$$\underset{(T,T)}{J_T} = \begin{pmatrix} 1 & 1 & \cdots & 1 \\ 1 & 1 & \cdots & 1 \\ \vdots & \vdots & \ddots & \vdots \\ 1 & 1 & \cdots & 1 \end{pmatrix} \quad \text{and} \quad \underset{(T,T)}{\frac{J_T}{T}} = \begin{pmatrix} \frac{1}{T} & \frac{1}{T} & \cdots & \frac{1}{T} \\ \frac{1}{T} & \frac{1}{T} & \cdots & \frac{1}{T} \\ \vdots & \vdots & \ddots & \vdots \\ \frac{1}{T} & \frac{1}{T} & \cdots & \frac{1}{T} \end{pmatrix} \quad (\text{A.1})$$

Apply operator $\frac{J_T}{T}$ to y_i to obtain :

$$\underset{(T,T)}{\frac{J_T}{T}} \cdot \underset{(T,1)}{y_i} = \begin{pmatrix} \frac{1}{T} & \frac{1}{T} & \cdots & \frac{1}{T} \\ \frac{1}{T} & \frac{1}{T} & \cdots & \frac{1}{T} \\ \vdots & \vdots & \ddots & \vdots \\ \frac{1}{T} & \frac{1}{T} & \cdots & \frac{1}{T} \end{pmatrix} \begin{pmatrix} y_{i1} \\ y_{i2} \\ \vdots \\ y_{iT} \end{pmatrix} = \begin{pmatrix} \frac{1}{T} \sum_{t=1}^T y_{it} \\ \frac{1}{T} \sum_{t=1}^T y_{it} \\ \vdots \\ \frac{1}{T} \sum_{t=1}^T y_{it} \end{pmatrix} = \begin{pmatrix} \bar{y}_i \\ \bar{y}_i \\ \vdots \\ \bar{y}_i \end{pmatrix} = \underset{(T,1)}{(\bar{y}_i)} \quad (\text{A.2})$$

where \bar{y}_i is the individual average of y_{it} for the country i over the observation period. Denote $\underset{(T,1)}{(\bar{y}_i)}$ the vector of the individual means replicated T times. To the vector $\underset{(T,1)}{y_i}$ the transformation $\underset{(T,T)}{\frac{J_T}{T}}$ thus gives the vector $\underset{(T,1)}{(\bar{y}_i)}$.

The second linear operator considered is the « mean deviations » operator : $I_T - \frac{J_T}{T}$. Applying this transformation to the vector $\underset{(T,1)}{y_i}$ leads to :

$$\left(\underset{(T,T)}{I_T - \frac{J_T}{T}} \right) \cdot \underset{(T,1)}{y_i} = \underset{(T,T)}{I_T} \underset{(T,1)}{y_i} - \underset{(T,T)}{\frac{J_T}{T}} \underset{(T,1)}{y_i} = \begin{pmatrix} y_{i1} \\ y_{i2} \\ \vdots \\ y_{iT} \end{pmatrix} - \begin{pmatrix} \bar{y}_i \\ \bar{y}_i \\ \vdots \\ \bar{y}_i \end{pmatrix} = \begin{pmatrix} y_{i1} - \bar{y}_i \\ \vdots \\ y_{it} - \bar{y}_i \\ \vdots \\ y_{iT} - \bar{y}_i \end{pmatrix} = \underset{(T,1)}{(y_{it} - \bar{y}_i)} \quad (\text{A.3})$$

The information available for country i consists of the observations contained in the vector y_i . Based on these operators, the information can be broken down by projection into two orthogonal components. On the one hand, the vector of individual averages (\bar{y}_i) informs us about the average level at which the country i is situated. The *between* operator is defined by :

$$B_{(NT,NT)} = I_N \otimes \frac{J_T}{T} \quad (\text{A.4})$$

On the other hand, the vector $(y_{it} - \bar{y}_i)$ provides, for country i , the fluctuations observed over time around its average level. The *within* operator is defined by :

$$W_{(NT,NT)} = I_N \otimes \left(I_T - \frac{J_T}{T} \right) \quad (\text{A.5})$$

It executes the transformation $I_T - \frac{J_T}{T}$, that is the deviations from the individual means, for each of the components y_i of y .

$$W_{(NT,NT)} = I_N \otimes \left(I_T - \frac{J_T}{T} \right) = \underbrace{(I_N \otimes I_T)}_{=I_{NT}} - \underbrace{\left(I_N \otimes \frac{J_T}{T} \right)}_{=B_{(NT,NT)}} \quad (\text{A.6})$$

Therefore, W can also be defined as :

$$W_{(NT,NT)} = I_{NT} - B_{(NT,NT)} \quad (\text{A.7})$$

The product of the between and within operators is 0. In other words, these operators project onto orthogonal spaces. Concretely, this corresponds to the fact that the means of the deviations from the individual means are equal to 0. Formally :

$$\begin{aligned} WB &= \left(I_N \otimes \left(I_T - \frac{J_T}{T} \right) \right) \left(I_N \otimes \frac{J_T}{T} \right) = I_N I_N \otimes \left(I_T - \frac{J_T}{T} \right) \left(\frac{J_T}{T} \right) \\ &= \underbrace{I_N I_N}_{=I_N} \otimes \left(\underbrace{\left(I_T \frac{J_T}{T} \right)}_{=\frac{J_T}{T}} - \underbrace{\left(\frac{J_T}{T} \frac{J_T}{T} \right)}_{=\frac{J_T}{T}} \right) = I_N \otimes \left(\frac{J_T}{T} - \frac{J_T}{T} \right) = 0 \end{aligned} \quad (\text{A.8})$$

As the cross-country *between*- regressions focus only on the permanent differences in mean levels between countries, it implies to collapse the data in mean to work on a between OLS estimator. In theory, the within and between estimators have zero covariance. They are projections of the model on orthogonal spaces. Since they are linear and orthogonal estimators, the OLS estimator can be rewritten as a weighted average of these two estimators :

$$\begin{aligned}
 \hat{\beta}_{OLS} &= (X'X)^{-1} X'y \\
 &= [X'WX + X'BX]^{-1} X'Wy + X'By \\
 &= \mu\hat{\beta}_W + (1 - \mu)\hat{\beta}_B
 \end{aligned}
 \tag{A.9}$$

where $\mu = (X'X)^{-1}X'WX$ represents the share of the within variance in the overall variance.

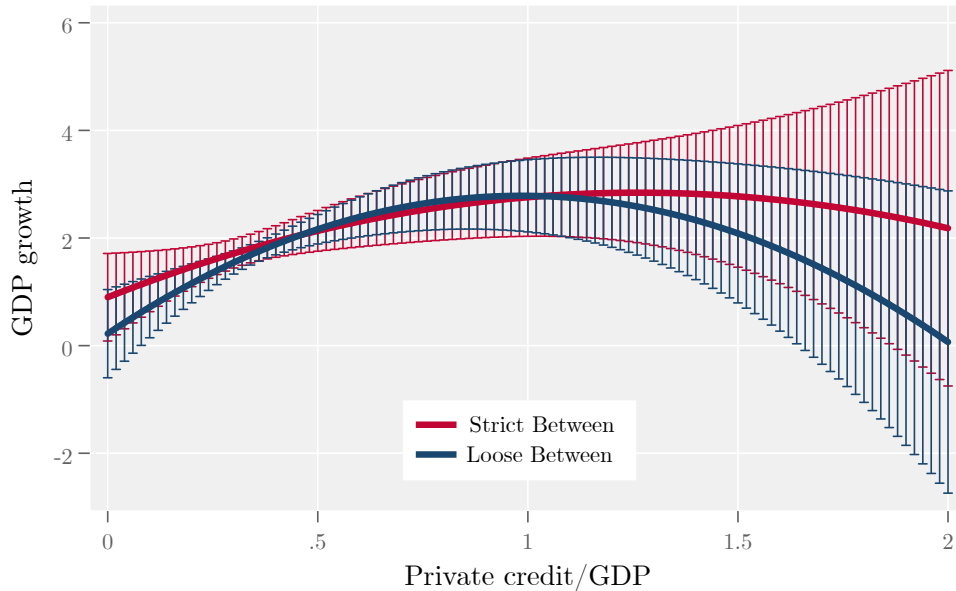


Figure A.1 – Sensitivity of cross-country threshold estimates to the between treatment. Both estimates are based on the new set over 1970-2015 using the two different methodologies presented in equations A.10 and A.11. The coefficients estimates are reported in Columns (2) and (4) of Table I.1.

The between regressions in Arcand, Berkes et Panizza (2015) are based on data first collapsed in mean then receive a log or square treatment :

$$\overline{\Delta y_i} = \alpha + \beta_1 \overline{PC_i} + \beta_2 (\overline{PC_i})^2 + \gamma \ln(\overline{z_i}) + \bar{v}_i \quad (\text{A.10})$$

However, the implementation of the between estimator would then suggest the opposite. In order to work on the strict between dimension, the collapsing of the data in mean has to occur after the data is transformed into logs and squares. The between operator should be applied on pre-treated variables :

$$\overline{\Delta y_i} = \alpha + \beta_1 \overline{PC_i} + \beta_2 \overline{(PC_i^2)} + \gamma \overline{\ln(z_i)} + \bar{v}_i \quad (\text{A.11})$$

Figure A.1 supports the idea that this methodological issue can have large implications. Based on the same sample of 74 countries over 1970-2015, the two plots show that the strict between dimension does not support the threshold conclusion. The SLM test in the last column of Table I.1 page 47 rejects the presence of an inverted U-shape relationship between finance and growth. A Loose data treatment enhances the overfitting of a threshold.

3 Additional Cross-Country Robustness Checks

3.1 Sample Dependency

The evidence presented in section 3 page 45 are consistent with three observation driving the curvature and thereby the threshold estimate. These observations are the United-States (USA), Japan (JPN), and Switzerland (CHE). Both Japan (JPN) and Switzerland (CHE) display high $Dfbeta$ statistics (Belsley, Kuh et Welsch, 1980). However, as the $Dfbeta$ statistic works by dropping one observation at a time, the United-States (USA) does not display an outstanding statistic as it is caught between the other two observations. The $Dfbeta$ statistic fails to grasp multiple outliers at once.

As expected, the linear and quadratic terms for private credit turn insignificant when dropping these three observations. The SLM test indicates that the threshold estimate is no longer statistically relevant. The regression in section 3 emphasizes the dependency of the non-linear conclusion over a long period on a few observations driving the results.

To strengthen the argument that the whole non-monotonic conclusion is sample sensitive, I conduce two additional robustness checks. The first one consists of removing a handful of countries at a time based on their initial GDP per capita in 1970. Groups include five countries of similar development levels. Each column presents one estimate of the overall sample minus one group of five observations.

This exercise checks whether or not removing a few developing (especially low-income) countries could reinforce the non-monotonic conclusion and recreate a curvature. Estimates from Table A.7 emphasize that it is not the case. Dropping the low-income countries in Column (1) returns an insignificant threshold, rejecting the non-monotonic conclusion. Dropping other groups unveil some interesting patterns. First, the SLM test barely supports a non-monotonic fit with a p-value close to 0.1. However, dropping a group of 5 observations in Columns (1), (6), and (15) discard the presence of a threshold. These additional estimates highlight the general dependency of the non-linear conclusion over a long period on a few observations driving the results.

Table A.8 presents a similar exercise, this time grouping countries based on their average private credit ratio over 1970-2015. Dropping groups one by one unveil once

Table A.7 – Sequential removal of observations sorted by initial GDP per capita.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Log(init. GDP/capita)	-0.713*** (0.168)	-0.759*** (0.183)	-0.747*** (0.162)	-0.765*** (0.149)	-0.784*** (0.145)	-0.666*** (0.165)	-0.776*** (0.153)	-0.770*** (0.154)	-0.803*** (0.151)	-0.774*** (0.162)	-0.711*** (0.102)	-0.759*** (0.154)	-0.740*** (0.153)	-0.739*** (0.158)	-0.733*** (0.165)
Private Credit	4.725** (1.666)	4.780** (1.572)	4.515* (1.811)	4.837** (1.795)	6.562*** (1.609)	4.249* (1.628)	4.772** (1.649)	4.571** (1.659)	5.453*** (1.561)	4.722* (1.783)	4.838** (1.775)	4.817** (1.672)	5.812** (1.912)	4.790** (1.625)	4.506** (1.577)
(Private Credit) ²	-2.214* (1.082)	-2.284* (0.998)	-2.310* (1.129)	-2.471* (1.174)	-3.409** (1.027)	-2.025 (1.031)	-2.400* (1.074)	-2.263* (1.063)	-2.586* (1.029)	-2.454* (1.081)	-2.435* (1.108)	-2.364* (1.078)	-3.222* (1.355)	-2.362* (1.048)	-2.031* (0.984)
Log(school)	1.294** (0.389)	1.277* (0.527)	1.611*** (0.356)	1.687*** (0.346)	1.431*** (0.347)	1.239** (0.376)	1.556*** (0.365)	1.552*** (0.368)	1.420*** (0.364)	1.584*** (0.352)	1.408*** (0.394)	1.469*** (0.366)	1.392*** (0.358)	1.484*** (0.381)	1.442*** (0.374)
Log(inflation)	0.062 (0.160)	0.008 (0.153)	-0.047 (0.154)	-0.021 (0.162)	0.024 (0.124)	0.034 (0.153)	-0.003 (0.174)	-0.023 (0.153)	0.011 (0.172)	-0.002 (0.157)	-0.001 (0.156)	-0.006 (0.169)	0.015 (0.161)	-0.021 (0.158)	0.007 (0.156)
Log(trade)	0.334 (0.280)	0.283 (0.263)	0.237 (0.272)	0.163 (0.282)	0.150 (0.241)	0.339 (0.280)	0.255 (0.272)	0.238 (0.278)	0.238 (0.260)	-0.005 (0.267)	0.172 (0.281)	0.221 (0.279)	0.246 (0.273)	0.181 (0.300)	0.268 (0.296)
Log(gov. cons.)	-0.819 (0.561)	-0.619 (0.569)	-0.821 (0.575)	-1.194 (0.613)	-1.197* (0.501)	-0.704 (0.614)	-0.872 (0.556)	-0.787 (0.558)	-0.818 (0.579)	-0.628 (0.549)	-0.949 (0.582)	-0.910 (0.624)	-0.970 (0.588)	-0.797 (0.584)	-1.011 (0.581)
Observations	69	69	69	69	69	69	69	69	69	69	69	69	69	69	70
R ²	0.47	0.45	0.49	0.53	0.57	0.41	0.51	0.51	0.52	0.47	0.47	0.49	0.50	0.48	0.50

dGrowth/dPC=0	107%	105%*	98%*	0.98%*	96%*	105%	99%*	101%*	105%*	96%*	99%*	102%*	90%*	101%*	111%
90% Feller CI	[85%-265%]	[83%-197%]	[77%-205%]	[79%-202%]	[81%-126%]	[80%-291%]	[80%-189%]	[81%-208%]	[87%-176%]	[77%-158%]	[79%-185%]	[81%-202%]	[74%-152%]	[80%-193%]	[87%-269%]
SLM (p-value)	0.13	0.09	0.08	0.08	0.02	0.13	0.08	0.09	0.08	0.06	0.08	0.09	0.05	0.09	0.15

Notes: This table reports the results of a set of cross-country OLS regressions in which the dependent variable is the average real GDP per capita growth rate. The robustness exercise consists of removing five observations at a time for countries sorted by their initial GDP per capita in 1970. Each column present one estimate of the overall sample minus the following observations:

- Column 1 : BDI, IND, PAK, RWA, SLE
- Column 2 : IDN, KEN, LKA, NER, TGO
- Column 3 : CMR, EGY, SDN, SEN, THA
- Column 4 : GHA, MAR, PHL, PRY, SWZ
- Column 5 : BOL, COG, DOM, HND, IRQ
- Column 6 : CIV, GTM, GUY, KOR, MYS
- Column 7 : COL, ECU, FJI, PER, SLV
- Column 8 : CRI, JAM, MLT, PAN, TUR
- Column 9 : BRA, CHL, MEX, URU, ZAF
- Column 10 : ARG, GAB, IRN, SGP, TTO
- Column 11 : ESP, GRC, IRL, PRT, VEN
- Column 12 : FIN, GBR, ISL, ISR, ITA
- Column 13 : AUT, BEL, DEU, FRA, JPN
- Column 14 : AUS, NLD, SAU, SWE, USA
- Column 15 : CHE, DNK, LUX, NOR

The SLM test provides p-value for the relevance of the estimated threshold. Robust Windmeijer standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.

Table A.8 – Sequential removal of observations sorted by average private credit ratio.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Log(int. GDP/capita)	-0.748*** (0.142)	-0.780*** (0.166)	-0.707*** (0.170)	-0.830*** (0.148)	-0.774*** (0.164)	-0.677*** (0.164)	-0.766*** (0.171)	-0.758*** (0.151)	-0.783*** (0.155)	-0.762*** (0.156)	-0.678*** (0.147)	-0.741*** (0.156)	-0.743*** (0.175)	-0.779*** (0.151)	-0.768*** (0.146)
Private Credit	6.120*** (1.594)	5.176** (1.906)	5.245*** (1.746)	4.807*** (1.613)	4.531*** (1.586)	4.729*** (1.678)	4.966*** (1.593)	4.995*** (1.661)	4.433* (1.680)	5.026** (1.710)	4.254* (1.625)	4.636*** (1.665)	4.455* (2.017)	4.665*** (1.697)	3.408 (2.812)
(Private Credit) ²	-3.321** (1.061)	-2.527* (1.164)	-2.646* (1.142)	-2.316* (1.022)	-2.220* (1.018)	-2.240* (1.082)	-2.321* (1.047)	-2.579* (1.097)	-2.250* (1.049)	-2.503* (1.105)	-2.007 (1.065)	-2.298* (1.063)	-2.251 (1.164)	-2.393* (1.047)	-0.473 (2.450)
Log(school)	1.638*** (0.336)	1.298** (0.454)	1.412*** (0.406)	1.511*** (0.380)	1.487*** (0.379)	1.253** (0.386)	1.433*** (0.370)	1.575*** (0.352)	1.623*** (0.386)	1.475*** (0.366)	1.291*** (0.364)	1.466*** (0.389)	1.538*** (0.400)	1.586*** (0.352)	1.494*** (0.351)
Log(inflation)	-0.086 (0.131)	-0.008 (0.151)	0.035 (0.191)	-0.031 (0.152)	0.005 (0.155)	0.044 (0.153)	0.151 (0.154)	-0.010 (0.158)	-0.076 (0.162)	0.004 (0.170)	0.022 (0.159)	-0.009 (0.157)	-0.021 (0.158)	-0.037 (0.149)	0.025 (0.146)
Log(Trade)	0.162 (0.238)	0.151 (0.282)	0.151 (0.287)	0.284 (0.258)	0.256 (0.276)	0.359 (0.270)	0.396 (0.273)	0.235 (0.260)	0.252 (0.266)	0.231 (0.272)	0.243 (0.311)	0.168 (0.285)	0.172 (0.322)	0.014 (0.266)	0.183 (0.257)
Log(gov. cons.)	-1.327*** (0.459)	-0.747 (0.588)	-1.040 (0.629)	-0.698 (0.575)	-0.633 (0.604)	-0.731 (0.556)	-0.798 (0.576)	-1.137* (0.564)	-0.819 (0.573)	-0.907 (0.623)	-0.793 (0.560)	-0.925 (0.576)	-0.791 (0.592)	-0.583 (0.564)	-0.964 (0.551)
Observations	69	69	69	69	69	69	69	69	69	69	69	69	69	69	70
R ²	0.58	0.44	0.48	0.53	0.49	0.47	0.51	0.53	0.50	0.48	0.43	0.46	0.44	0.47	0.54
dGrowth/dPC=0	92%***	102%***	99%***	104%***	102%***	105%***	107%***	97%***	98%***	100%***	106%***	100%***	99%***	97%***	360%***
90% Feller CI	[77%-124%]	[83%-193%]	[80%-176%]	[83%-193%]	[81%-204%]	[83%-247%]	[85%-222%]	[78%-167%]	[77%-192%]	[81%-185%]	[81%-383%]	[80%-201%]	[79%-225%]	[78%-164%]	-
SLM (p-value)	0.02	0.08	0.07	0.09	0.09	0.12	0.12	0.06	0.08	0.08	0.15	0.09	0.09	0.06	-

Notes : This table reports the results of a set of cross-country OLS Between regressions in which the dependent variable is the average real GDP per capita growth rate. The robustness exercise consists of removing five observations at a time for countries sorted by their average private credit level between 1970 and 2015. Each column present one estimate of the overall sample minus the following observations :

Column 1 : GH, IRQ, RVA, SDN, SLE
 Column 2 : BDI, CMR, COG, GAB, NER
 Column 3 : ARG, GTM, LKA, PER, SWZ
 Column 4 : ECU, MEX, PRY, TGO, TUR
 Column 5 : DOM, JAM, KEN, PAK, SEN
 Column 6 : CIV, GRI, IDN, IND, URY
 Column 7 : BOL, COL, EGY, GUY, VEN
 Column 8 : HND, IRN, MAR, PHI, SLV
 Column 9 : BEL, BRA, FJI, SAU, TTO
 Column 10 : CHI, FIN, GRC, ISR, PAN
 Column 11 : AUS, FRA, IRL, ITA, KOR
 Column 12 : AVT, DNK, ISL, MIT, NOR
 Column 13 : LUX, MYS, NLD, PRT, THA
 Column 14 : DEU, ESP, GBR, SGP, SWE
 Column 15 : CHE, JPN, USA, ZAF

The SLM test provides *p*-value for the relevance of the estimated threshold. Robust Windmeijer standard errors in parentheses. *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

again some striking patterns. First, the SLM test barely supports a non-monotonic fit with a p-value close to 0.10. However, dropping a group of 5 observations in Columns (6), (7), (11), and (15) discard the presence of a threshold. Recall that, up to Column (15), the various samples still include the United-States (USA), Japan (JPN), and Switzerland (CHE). This additional exercise emphasizes the overall dependency of the non-linear conclusion over a long period on a few observations driving the results.

3.2 Bad Controls and Overcontrolling

As the growth empirics flourished around cross-country regressions in the 90s, Levine et Renelt (1992) documented that growth regressions were generally not robust to variation in the set of conditioning variables. Durlauf, Johnson et Temple (2005) find that as many as 145 distinct variables have been included in growth regressions in published papers.

The lack of a unified framework to guide empirical work on the matter leaves the economist with numerous control variables to chose from due to decades of growth theories. The moderate sample size calls for some necessary parsimony. The various regressions in this first chapter are conducted based on a set of policy and nonpolicy control variables correlated with growth performance across countries, common to growth empiric literature :

- the logarithm of initial GDP per capita to capture convergence and provide a dynamic feature in the panel estimates (Barro, 1991 ; Bun et Sarafidis, 2015),
- the average years of education gathered from Barro et Lee (2013) to account for human capital,
- a measure of trade openness (Frankel et Romer, 1999), computed as exports plus imports divided by GDP and World Bank (gathered from WDI, 2018),
- the log of the inflation rate¹ (Barro, 1995 ; Rousseau et Wachtel, 2002),
- the log of government consumption normalized by GDP to account for fiscal adjustments (Alesina et Wacziarg, 1998 ; Sala-i-Martin, Doppelhofer et Miller, 2004).

1. To deal with possible negative value using log of this variable, we apply the inverse hyperbolic sine transformation : $\ln(\text{infl}) \equiv \ln\left(\text{infl} + \sqrt{\text{infl}^2 + 1}\right)$. The data is (gathered from WDI, World Bank, 2018).

This set of control variables represent arguably a reasonable conditioning set. However, it is a legitimate concern to wonder if the finance-growth relationship is robust when the conditioning set of variables in the regression changes. As emphasized in Levine et Renelt (1992, p.942), “only a few findings can withstand slight alteration in the list of explanatory variables.” *Overcontrolling* or *bad control* can occur if the explanatory variables themselves depend on financial development and credit. Based on the partial correlation matrix A.5, it appears to be the case with nearly all the control variables (except for Trade).

To account for *overcontrolling* or *bad control*, I implement two mainstream techniques : first, a sequential inclusion of the control variables to check parameter stability, presented in Table A.9. Second, a Bayesian model averaging (BMA), often used to impose parsimony in the cross-country growth regression, presented in Tables A.10 and A.11.

Sequential inclusion of the control variables. Table A.9 presents various control combinations within the estimation from column (2) of Table I.1, with all the available observations from the new dataset, including the three “outliers”, and without the methodological correction presented in the Appendix section 2 page 186. First of all, the point estimate of the initial GDP per capita in 1970 is always statistically significant and stable. Excluding this measure for convergence from the model dampens the preciseness of the threshold estimate (columns (3) and (5)). While school appeals as a strong determinant, other controls lack a significant association with average growth. In line with Levine et Renelt (1992), it is striking to notice that including Trade, with a statistically insignificant point estimate in column (9), dismisses the relevance of a threshold. The SLM test returns a higher p -value. However, further completing the policy set with government consumption in column (10), albeit not significantly associated with the dependent variable, improves the threshold estimates.

Bayesian model averaging. Tables A.10 and A.11 present the Bayesian model averaging (BMA) on the same setup. BMA provides a mechanism for accounting for model uncertainty² when deriving parameter estimates (Leamer, 1978).

2. For a detailed description of the literature on model averaging, see Hoeting et al. (1999).

Table A.9 – Overcontrol & Bad controls : sequential inclusion of the control variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Private Credit	3.780*** (1.271)	6.635*** (1.499)	2.613 (1.595)	3.839*** (1.296)	3.093** (1.364)	5.690*** (1.809)	4.644*** (1.307)	4.527*** (1.361)	4.352*** (1.389)	4.908*** (1.627)
(Private Credit) ²	-2.202** (0.868)	-3.313*** (0.881)	-1.671* (0.953)	-2.213** (0.869)	-1.721* (0.982)	-3.289*** (1.225)	-2.267*** (0.717)	-2.229*** (0.716)	-2.030** (0.797)	-2.432** (1.048)
Log(init. GDP/capita)		-0.499*** (0.140)					-0.867*** (0.141)	-0.866*** (0.141)	-0.853*** (0.148)	-0.752*** (0.152)
Log(school)			0.457 (0.385)				1.605*** (0.329)	1.627*** (0.358)	1.557*** (0.381)	1.460*** (0.362)
Log(inflation)				0.027 (0.106)				-0.026 (0.118)	0.025 (0.149)	0.003 (0.153)
Log(trade)					0.337 (0.257)				0.191 (0.292)	0.224 (0.267)
Log(gov. cons.)						-1.650** (0.631)				-0.865 (0.559)
Observations	74	74	74	74	74	74	74	74	74	74
R ²	0.133	0.277	0.154	0.134	0.153	0.244	0.457	0.457	0.462	0.488
dGrowth/dPC=0	86%**	100%***	78%	87%**	90%	86%**	103%**	101%**	107%	101%*
90% Feller CI	[72%–122%]	[88%–121%]	–	[71%–126%]	[71%–541%]	[74%–117%]	[88%–128%]	[84%–129%]	[84%–170%]	[81%–181%]
SLM (<i>p</i> -value)	0.03	0.01	–	0.03	0.12	0.03	0.03	0.03	0.12	0.08

Notes .This table reports the results of a set of cross-country OLS Between regressions in which the dependent variable is the average real GDP per capita growth rate. The robustness exercise consists of a sequential inclusion of the control variables present the estimation from column (2) of Table 1.1, with all the available observations from the new dataset, including the three “outliers”, and without the methodological correction presented in the Appendix section 2 page 186. The SLM test provides *p*-value for the relevance of the estimated threshold. Robust Windmeijer standard errors in parentheses. *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Table A.10 – *Overcontrol & Bad controls* : Bayesian Model Averaging (1/2).

		N. obs. = 74				
		N. core = 3				
		N. auxiliary = 5				
		Coef. (μ)	Std. Err. (σ)	t	PIP	$\mu \pm \sigma$
BMA estimates						
NEW sample						
1970-2015						
Cons.		5.153	1.221	4.22	1.00	3.931 6.375
Private Credit		4.797	1.403	3.42	1.00	3.394 6.200
(Private Credit) ²		-2.379	0.925	-2.57	1.00	-3.304 -1.454
Log(init. GDP/capita)		-0.821	0.149	-5.50	1.00	-0.970 -0.671
Log(school)		1.544	0.342	4.51	1.00	1.201 1.887
Log(inflation)		-0.003	0.043	-0.07	0.11	-0.046 0.040
Log(trade)		0.027	0.106	0.26	0.15	-0.079 0.134
Log(gov. cons.)		-0.281	0.471	-0.60	0.35	-0.753 0.189

Notes : this table provides the estimated coefficients and their standard errors, which are the mean and standard deviation of the posterior distribution, t ratios, posterior inclusion probability PIP, and one-standard error bands. Estimation results for the core and the auxiliary parameters are displayed in the upper and lower panels of the table, respectively. An auxiliary regressor is a robust control variable if the t ratio on its coefficient is higher than one in absolute value (Magnus, 2002). Alternatively, Raftery (1995) and Masanjala et Papageorgiou (2008) suggest a rough guideline that a posterior inclusion probability greater than 0.5 can be an inclusion threshold.

The underlying idea of BMA is to compute a weighted average of the conditional estimates across all possible combination of variables, as each of them provides some information about the regression parameter. The weight given to each model and the conditional estimates of its parameters is determined following a Bayesian inference. It combines prior beliefs on the unknown elements of the model with the additional information arising from the data.³

Consider a model with core (X_1) and auxiliary variables (X_2) of the form :

$$y = X_1\beta_1 + X_{2i}\beta_{2i} + \varepsilon_i \quad i = 1, \dots, I$$

BMA first estimates the parameter of interest conditional on each model among all possible combinations of auxiliary variables (for k controls, $I = 2^k$ models \mathcal{M}_i are estimated). Then, it computes the unconditional estimates as a weighted average of the

3. BMA assigns equal prior probability $\mathbb{P}(\mathcal{M}_i) = 2^{-k}$ to each model. For more details, see De Luca et Magnus (2011).

Table A.11 – *Overcontrol & Bad controls* : Bayesian Model Averaging (2/2).

		N. obs. = 74				
		N. core = 1				
		N. auxiliary = 7				
	Coef. (μ)	Std. Err. (σ)	t	PIP	$\mu \pm \sigma$	
BMA estimates						
NEW sample						
1970-2015						
Cons.	4.970	1.270	3.91	1.00	3.700	6.240
Private Credit	3.684	2.077	1.77	0.93	1.607	5.761
(Private Credit) ²	-1.660	1.332	-1.25	0.73	-2.993	-0.327
Log(init. GDP/capita)	-0.806	0.151	-5.33	1.00	-0.958	-0.655
Log(school)	1.624	0.366	4.43	1.00	1.258	1.991
Log(inflation)	-0.009	0.058	-0.17	0.13	-0.068	0.049
Log(trade)	0.047	0.139	0.34	0.19	-0.091	0.186
Log(gov. cons.)	-0.226	0.433	-0.52	0.30	-0.660	0.206

Notes : this table provides the estimated coefficients and their standard errors, which are the mean and standard deviation of the posterior distribution, t ratios, posterior inclusion probability PIP, and one-standard error bands. Estimation results for the core and the auxiliary parameters are displayed in the upper and lower panels of the table, respectively. An auxiliary regressor is a robust control variable if the t ratio on its coefficient is higher than one in absolute value (Magnus, 2002). Alternatively, Raftery (1995) and Masanjala et Papageorgiou (2008) suggest a rough guideline that a posterior inclusion probability greater than 0.5 can be an inclusion threshold.

conditional estimates :

$$\hat{\beta}_1 = \sum_{i=1}^I \lambda_i \cdot \hat{\beta}_{1i}$$

I conduct two experiments. First, in Table A.10, the exercise consists in holding the financial proxies as core variables to be included in the model, with other controls considered as auxiliary regressors, where lies model uncertainty. Table A.10 provides the estimated coefficients and their standard errors, which are the mean and standard deviation of the posterior distribution, t ratios, posterior inclusion probability, and one-standard error bands. Estimation results for the core and the auxiliary parameters are displayed in the upper and lower panels of the table, respectively.⁴

An auxiliary regressor is a robust control variable if the t ratio on its coefficient is higher than one in absolute value (Magnus, 2002).⁵ Alternatively, Raftery (1995) and

4. The tables do not report p -values of the t ratios for testing the significance of the estimated regression parameter. The reason is that the Bayesian counterpart of a p -value cannot be easily computed because the distributions are not Gaussian (De Luca et Magnus, 2011).

5. “The intuition behind this result is also related to a well-known property of \bar{R}^2 (the adjusted R^2), which rises if and only if the t ratio associated with an added regressor is greater than one in absolute

Masanjala et Papageorgiou (2008) suggest a rough guideline that a posterior inclusion probability greater than 0.5 can be an inclusion threshold. From these guidelines, both the initial value of GDP per capita in 1970 and the human capital proxy appear as robust control variables. Both have a posterior inclusion probability of 1.00 and a high absolute t ratio. However, the BMA method discards the other three control variables.

The second exercise reported in Table A.11, consists in considering all the variables as auxiliary regressors with model uncertainty to check for the relevance of the financial proxies. Similar conclusions arise regarding the previous auxiliary variables. The finance proxies both have a relatively high posterior inclusion probability. However, the quadratic term has a low absolute t ratio of 1.25, high enough for inclusion but sufficient to underline an uncertain relevance.

value.” (De Luca et Magnus, 2011, p.531)

4 Identification Through Heteroscedasticity

Reliable causal inference of financial depth on economic growth requires instruments based identification strategy. The instruments should display a strong relationship with the endogenous explanatory variables. If this relationship is only weak, instrumental variable estimators are severely biased (see Murray, 2006; Mikusheva, 2013, for a survey). Weak instruments also produce finite sample distributions that are non-normal. All things considered, if instruments are weak, “point estimates, hypothesis tests, and confidence intervals are unreliable” (Stock, Wright et Yogo, 2002).

Studies focusing on causal inference on the finance-growth relationship have first used econometric methods that mobilize external instruments. “Legal origins” (Glaeser et Shleifer, 2002) have been widely used to instrument various finance proxies (Levine, Loayza et Beck, 2000). However, as Bazzi et Clemens (2013) pointed out, legal origins may have been too widely used to be safely considered as a reliable instrument. In addition to being an instrument for various finance proxies, legal origins are also an instrument for corruption, tax rates and over-regulation (Friedman et al., 2000), for the formalism degree of the legal procedure (Djankov et al., 2003), for inflation and the inequality of land ownership (Lundberg et Squire, 2003), or the relative size of the small and medium enterprise sector (Beck et al., 2008). Each one of these studies suggesting the instrument is invalid in any other specification. Other identification strategies have consisted of using lagged variables as internal instruments in panel regressions or the differences-in-differences setup with industry-level data (Rajan et Zingales, 1998).

To address the various endogeneity problems in cross-country regressions, Arcand, Berkes et Panizza (2015) resort to an identification strategy through second moments. The latter exploits the heteroscedasticity in the model’s residuals to construct a set of internal instruments. Rigobon (2003) developed this methodology to identify causal relationships through heteroskedasticity. For the sake of presenting the intuition, I draw on a generic version of the finance-growth model :

$$\Delta y_i = \beta_1 PC_i + \gamma_1 X_i + \varepsilon_{1i} \tag{A.12}$$

Without a stretch, assume there is an endogeneity issue (of reverse causality for example) :

$$PC_i = \beta_2 \Delta y_i + \gamma_2 X_i + \varepsilon_{2i} \quad (\text{A.13})$$

The covariance of the reduced form can be estimated and consists of 3 equations in 4 unknowns $(\beta_1, \beta_2, \nu_{\varepsilon_1}, \nu_{\varepsilon_2})$:

$$\Omega = \frac{1}{(1 - \beta_1 \beta_2)^2} \begin{bmatrix} \nu_{\varepsilon_1} + \beta_1^2 \nu_{\varepsilon_2} & \beta_2^2 \nu_{\varepsilon_1} + \beta_1^2 \nu_{\varepsilon_2} \\ \beta_2^2 \nu_{\varepsilon_1} + \beta_1^2 \nu_{\varepsilon_2} & \beta_2^2 \nu_{\varepsilon_1} + \nu_{\varepsilon_2} \end{bmatrix} \quad (\text{A.14})$$

Splitting the sample into two parts characterized by different empirical covariance will create 6 equations in 8 unknowns. However, if the regressions coefficients are stable across groups, this boils down to 6 equations in 6 unknowns.

Arcand, Berkes et Panizza (2015) use the estimator developed by Lewbel (2012), generalizing Rigobon's intuition without requiring a discrete regime. It requires a couple of assumptions. Assume that X_i is a set of exogenous variables such that :

$$\mathbb{E}(\varepsilon_{1i}|X_i) = \mathbb{E}(\varepsilon_{2i}|X_i) = \mathbb{E}(\varepsilon_{1i}\varepsilon_{2i}|X_i) = 0 \quad (\text{A.15})$$

The heteroscedasticity in the data ($cov(X_i, \varepsilon_{2i}^2) \neq 0$) enables to use $X_i \varepsilon_{2i}$ as an instrument for the finance proxy. This instrument has nice properties since it is both uncorrelated with ε_{1i} and correlated with PC_i (the presence of heteroscedasticity guaranties that the instrument is correlated with PC_i through its correlation with ε_{2i}).

There is no economic intuition behind this identification strategy. It is a mathematical artifice. The downside, as emphasized by Lewbel (2012), is that "the resulting identification is based on higher moments, and so is likely to provide less reliable estimates than identification based on standard exclusion restrictions." This identification strategy should be viewed as a spare wheel and used with much caution.

Table A.12 – Misleading cross-country IH regressions, complete Table I.2.

Data Period	IH-Between			IH-Strict Between	
	(1) Old 1970-2010	(2) New 1970-2010	(3) New 1970-2015	(4) New 1970-2010	(5) New 1970-2015
Private Credit	8.849*** (1.937)	8.883*** (2.577)	9.002*** (2.015)	-0.157 (3.674)	-0.110 (3.191)
(Private Credit) ²	-4.457*** (1.117)	-4.259*** (1.282)	-4.312*** (1.026)	-0.098 (1.497)	-0.048 (1.256)
Log(init. GDP/capita)	-0.702*** (0.146)	-0.736*** (0.149)	-0.788*** (0.137)	-0.449*** (0.153)	-0.529*** (0.138)
Log(school)	0.115 (0.136)	0.776 (0.478)	0.899** (0.378)	1.563** (0.612)	1.699*** (0.553)
Log(inflation)	0.169 (0.216)	0.227 (0.176)	0.172 (0.143)	-0.165 (0.405)	-0.270 (0.350)
Log(trade)	-0.867* (0.481)	0.141 (0.225)	0.185 (0.202)	-0.062 (0.302)	0.043 (0.274)
Log(gov. cons.)	-0.785* (0.408)	-0.902** (0.454)	-1.267*** (0.436)	-0.034 (0.448)	-0.303 (0.435)
Constant	4.551*** (1.365)	5.166*** (1.554)	6.182*** (1.442)	3.672* (1.959)	4.517*** (1.684)
Observations	64	77	74	77	74
N. instruments	10	10	10	10	10
Kleibergen-Paap LM test (<i>p</i> -val)	0.12	0.05	0.06	0.18	0.13
Cragg-Donald Wald statistic	3.05	2.08	2.33	0.88	0.96
H_0 : <i>t</i> -test size >10% (<i>p</i> -val)	1.00	1.00	1.00	1.00	1.00
H_0 : <i>t</i> -test size >25% (<i>p</i> -val)	1.00	1.00	1.00	1.00	1.00
H_0 : relative OLS bias >10% (<i>p</i> -val)	1.00	1.00	1.00	1.00	1.00
H_0 : relative OLS bias >30% (<i>p</i> -val)	0.41	0.76	0.67	0.99	0.99
Kleibergen-Paap Wald statistic	5.19	4.28	4.78	1.06	1.11
H_0 : <i>t</i> -test size >10% (<i>p</i> -val)	1.00	1.00	1.00	1.00	1.00
H_0 : <i>t</i> -test size >25% (<i>p</i> -val)	0.81	0.94	0.88	1.00	1.00
H_0 : relative OLS bias >10% (<i>p</i> -val)	0.91	0.98	0.95	1.00	1.00
H_0 : relative OLS bias >30% (<i>p</i> -val)	0.03	0.11	0.06	0.98	0.98
Hansen test (<i>p</i> -value)	0.46	0.28	0.46	0.35	0.18
dGrowth/dPC=0	99%***	104%***	104%***	-80%	-115%
90% Fieller CI	[88%–117%]	[92%–121%]	[93%–120%]	–	–
SLM (<i>p</i> -value)	<0.01	<0.01	<0.01	–	–

Notes : This table reports the results of a set of cross-country IV regressions in which the dependent variable is the average real GDP per capita growth rate. The identification strategy rests on the estimator developed by Rigobon (2003) and Lewbel (2012), and relies on heteroscedasticity-constructed internal instruments (IH). While the first column provides a benchmark of the typical non-linear result from the old dataset, the subsequent columns report estimates based on the new dataset expanding the period and country coverage. Column (2) is based on the new dataset with the same time coverage as column (1) but with additional countries. Column (3) expands the coverage up to 2015. Columns (4-5) incorporate a slight methodological correction. The SLM test provides *p*-value for the relevance of the estimated threshold. Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5 Lights on Timing Conventions

Section 4.1 provides a presentation on the specifics underlying the commonly chosen five-year spell length. First, the use of longer periods would significantly reduce the number of degrees of freedom, which is problematic when implementing dynamic panel data procedures. Secondly, five-year periods, as emphasized by Calderon, Chong et Loayza (2002), follows the endogenous growth literature (e.g. Caselli, Esquivel et Lefort, 1996; Easterly, Loayza et Montiel, 1997; Benhabib et Spiegel, 2000; Forbes, 2000) where such period length is believed to purge out business-cycle fluctuations which could induce a negative coefficient on private credit. Indeed, the empirical growth literature usually averages out data over five-year spells in order to measure the steady-state relationship between the variables. Smoothing out data series supposedly removes useless variation from the data, enabling precise parameter estimates. Indeed, Loayza et Rancière (2006) find that short-run surges in private credit appear to be a good predictor of both banking crises and slow growth. In the long run, a higher level of private credit is associated with higher economic growth. This tension between short-term and long-term effects justifies the use of low-frequency data to abstract from business-cycles. Finally, this is conveniently suited to the specifics of System-GMM, as it requires a short panel characterized by large N and small T dimensions.

The use of initial values refers back to the empirical growth literature of the 1990s, where this technique allowed to circumvent (in a limited way) the lack of reliable econometric tools to draw a causal inference. We owe this to Barro (1991), later used in the finance-growth literature by King et Levine (1993). Both use some value in 1960 (per capita GDP or financial development) to estimate change in growth rate over the subsequent 30-year period. The initial value then works as a predictor in a cross-country OLS framework, allowing to control for simultaneity.

Switching to panel data changes the game. Model (I.2), would suffer from a significant downside. Indeed, the transposition of the theoretical model to the empirical study with a panel of countries results in a substantial loss of observations. The systematic use of initial values (in t) in a non-overlapping setting lead to only one observation every

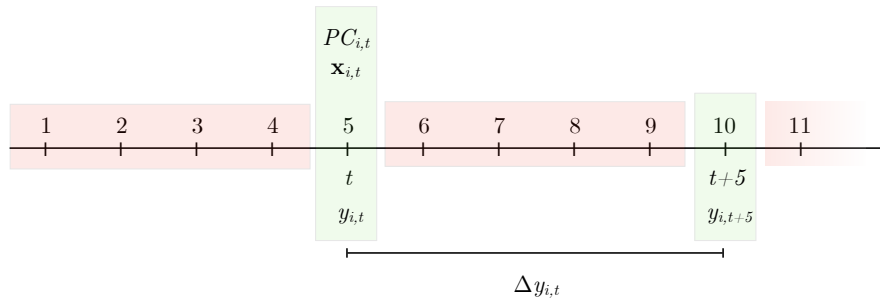


Figure A.2 – Timing conventions in generic dynamic finance-growth models such as (I.2). This timing convention is common to most finance-growth threshold empirical exercise based on panel data. See for example the work of (Cecchetti et Kharroubi, 2012; Arcand, Berkes et Panizza, 2015; Sahay et al., 2015a; Benczur, Karagiannis et Kvedaras, 2019; Cheng, Chien et Lee, 2020)

five years. Hence, figure A.2 shows that a literal transposition implies a substantial informational (efficiency) loss as well as a consistency loss. Indeed, keeping only 20% of the data (materialized by the green areas) would expose the coefficient estimates to bias as it could mismeasure the true explanatory variables. Secondly, initial values are not immune to the potential business-cycle phenomena previously discussed.

The work of Caselli, Esquivel et Lefort (1996) is among the first attempts to use the GMM framework to estimate a Solow growth model. They make use of the Barro (1991) method, initially created for cross-section data, by adapting it for a panel framework with the GMM estimator of Arellano et Bond (1991). It remains a dynamic panel with the dependent variable lagged on a 5-year horizon. Already at this stage, the problem related to extensive use of initial value has been raised. In their seminal paper, Caselli, Esquivel et Lefort (1996), decided to work on averaged data for the left-hand-side variable (average annual growth rate of per capita GDP), but distinguished between state and control variables for the right-hand-side variables. Controls are averaged over the 5-year intervals (government consumption, inflation rate, trade openness). In contrast, only states variables are taken at their initial value (initial level of per capita GDP, average number of years of schooling). It enables us to catch the convergence effect and to control for simultaneity. Therefore all variables do not enter with the same treatment.

A strand of the literature has gone further and has abandoned this initial values except for the convergence variable, considering it preferable to keep more observations through data averaged over sub-periods, while controlling for endogeneity biases by

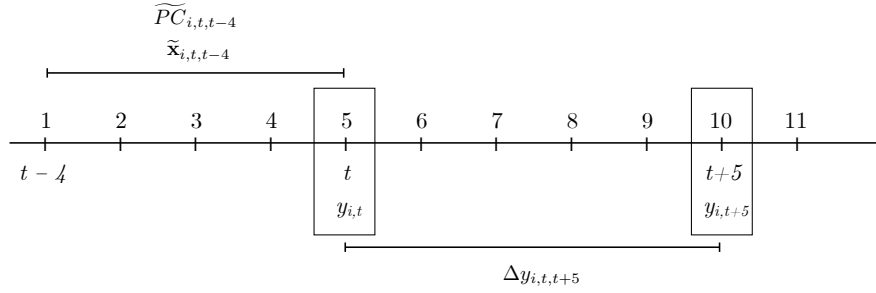


Figure A.3 – Alternative data coverage of model (A.16).

properly instrumenting the explanatory variables.⁶ Moreover, as is now common in this line of research (Levine, 2005; Baltagi, Demetriades et Law, 2009), control variables enter the model with a lag treatment to avoid endogeneity stemming from simultaneous relationships. However, this does not fully rule out simultaneity bias as expectations about future growth conditions may affect the current financial sector. It is intended to dampen this bias as much as possible. In this line, the safest approach would be to retain the following data processing :

$$\Delta y_{i,t,t+k} = \lambda y_{i,t} + \beta_1 \widetilde{PC}_{i,t,t-k+1} + \beta_2 \widetilde{PC}_{i,t,t-k+1}^2 + \gamma \widetilde{\mathbf{x}}_{i,t,t-k+1} + \nu_{it+k} \quad (\text{A.16})$$

$$\nu_{it+k} = \mu_i + \lambda_{t+k} + \varepsilon_{i,t+k}$$

where $\widetilde{PC}_{i,t,t-k+1}$ denote the average value of variable PC between $t - k + 1$ and t , then for the typical 5-year spells, this consists in averaging the financial depth proxy from $t - 4$ to t . To clarify and allow comparison, Figure A.3 provides a time table for equation (A.16), emphasizing the broader use of data.

6. See, for examples Benhabib et Spiegel (2000), Beck et Levine (2004), Rioja et Valev (2004), Cecchetti et Kharroubi (2012), Beck, Degryse et Kneer (2014) et Law et Singh (2014).

Table A.13 – Sequential anchoring of the five-year spells, complete Table I.3.

	(1)	(2)	(3)	(4)	(5)
Data	Old	Old	Old	Old	Old
Coverage	1960-2010	1961-2011	1962-2007	1963-2008	1964-2009
Number of spells	10	10	9	9	9
Private Credit	3.621** (1.718)	0.171 (1.824)	0.780 (1.877)	0.084 (1.689)	1.971 (1.688)
(Private Credit) ²	-2.018*** (0.727)	-0.882 (0.774)	-0.749 (0.889)	-0.523 (0.782)	-1.418* (0.852)
Log(init. GDP/capita)	-0.725** (0.310)	-0.198 (0.304)	-0.135 (0.306)	-0.409 (0.341)	-0.600** (0.278)
Log(school)	2.275*** (0.616)	2.368*** (0.641)	1.893*** (0.638)	2.810*** (0.678)	2.974*** (0.633)
Log(inflation)	-0.273 (0.209)	-0.244 (0.160)	-0.264 (0.162)	-0.408** (0.182)	-0.165 (0.193)
Log(trade)	1.096** (0.509)	1.193*** (0.458)	0.806* (0.447)	0.962* (0.567)	1.214*** (0.386)
Log(gov. cons.)	-1.479** (0.739)	-1.767*** (0.564)	-1.669*** (0.543)	-1.455** (0.573)	-1.936*** (0.657)
N. instruments	318	318	254	254	254
N. countries	133	133	134	133	133
Observations	917	916	811	829	858
AR(2) (<i>p</i> -value)	0.11	0.08	0.23	0.51	0.91
Hansen test (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
dGrowth/dPC=0	90%**	10%	52%	8%	69%
90% Fieller CI	[43%–113%]	–	–	–	[0%–124%]
SLM (<i>p</i> -value)	0.03	0.46	0.40	0.48	0.19

Notes : This table reports the results of a set of panel regressions consisting of non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. The first column reports the best-attempted replication of the typical threshold result from the yearly version of the old dataset. Column (2) provides point estimates with a one-year forward shift for the starting point of each spell. The subsequent columns continue shifting forward by one year the beginning of the five-year spells. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.14 – Sequential anchoring of the five-year spells, complete Table I.4.

	(1)	(2)	(3)	(4)	(5)
Data	New	New	New	New	New
Coverage	1960-2015	1961-2016	1962-2012	1963-2013	1964-2015
Number of spells	11	11	10	10	10
Private Credit	-0.170 (1.480)	2.851* (1.465)	1.085 (1.408)	0.940 (1.610)	1.027 (1.800)
(Private Credit) ²	-0.256 (0.703)	-1.217* (0.699)	-1.062* (0.612)	-0.901 (0.679)	-0.698 (0.721)
Log(init. GDP/capita)	-0.814*** (0.238)	-1.137*** (0.292)	-0.553** (0.238)	-0.266 (0.317)	-0.835** (0.372)
Log(school)	3.446*** (0.548)	2.829*** (0.493)	2.585*** (0.467)	1.573*** (0.565)	2.852*** (0.672)
Log(inflation)	-0.156 (0.125)	-0.186 (0.147)	-0.269* (0.145)	-0.404** (0.176)	-0.210 (0.200)
Log(trade)	0.530 (0.429)	0.690* (0.392)	0.502 (0.458)	0.601 (0.402)	1.178*** (0.434)
Log(gov. cons.)	-1.245*** (0.434)	-1.247** (0.591)	-1.155 (0.715)	-1.008* (0.581)	-0.867 (0.629)
N. instruments	388	388	318	318	318
N. countries	140	140	138	138	138
Observations	1,055	1,085	965	970	987
AR(2) (<i>p</i> -value)	0.40	0.01	0.11	0.56	0.85
Hansen test (<i>p</i> -value)	1.00	1.00	1.00	1.00	1.00
dGrowth/dPC=0	–	117%*	51%	52%	73%
90% Fieller CI	–	[86%–426%]	[0%–95%]	–	–
SLM (<i>p</i> -value)	–	0.06	0.34	0.37	0.33

Notes : This table reports the results of a set of panel regressions consisting of non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. Each column presents one possible anchoring for the five-year spells in the new dataset. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6 Dynamic Panel GMM Estimation

6.1 General Presentation

As is now common in the empirical growth literature, the regression equation is dynamically specified by including the initial level of per capita GDP as an independent explanatory variable. Focus on the following model in order to simplify the presentation of the estimation procedure :

$$\begin{aligned}\Delta y_{i,t} &= \lambda y_{i,t-1} + \beta_1 PC_{i,t-1} + \beta_2 PC_{i,t-1}^2 + \gamma \mathbf{x}_{i,t-1} + \nu_{it} \\ \nu_{it} &= \mu_i + \lambda_t + \varepsilon_{i,t}\end{aligned}\tag{A.17}$$

where the subscripts i and t refer to cross section unit and time period. The panel has dimension $N \times T$. Considering the focus on non-overlapping spells, the model (A.17) is equivalent to model (I.2) where $t = 1, \dots, T$ would be normalized to refers to 5-year non-overlapping time-spans. Here, $\Delta y_{i,t}$ is the backward looking first-differenced logarithm of per capita GDP, accounting for growth of per capita GDP. $PC_{i,t}$ is the level of private credit over GDP ratio used as a proxy for financial development. $\mathbf{x}_{i,t}$ is the set of control variables presented in section 2.1. Finally, ν_{it} still follows a two-way error component model where μ_i , λ_t and $\varepsilon_{i,t}$ are respectively the country-specific effect, the period-specific effect and the error term.

Equation (A.17) displays two sources of persistence over time : the presence of the lagged dependent variable among the regressors and the heterogeneity among countries both produce autocorrelation. The model allows for endogenous covariates, as often the case in economic inquiry. For instance, financial depth variables are most likely not strictly exogenous but somehow simultaneously determined with our outcome variable of interest, growth rate of per capita GDP. Due to the various endogeneity problems arising in growth regressions, least-squares based inference methods (i.e., fixed effects estimators) can be biased and inconsistent.

The estimation method relies on dynamic panel *System-GMM* estimator introduced by Arellano et Bover (1995) and Blundell et Bond (1998). This GMM inference method

has been applied extensively in economic growth and finance literature. It improves upon pure cross-country work in several respects. Indeed, it deals with the dynamic component of the regression specification. It controls for unobserved time- and country-specific effects and accounts for some endogeneity in the variables. Therefore, it allows for a causal interpretation of the results.

To do so, Arellano et Bond (1991) suggest first to rearrange (A.17) as :

$$y_{i,t} = (\lambda + 1) y_{i,t-1} + \beta_1 PC_{i,t-1} + \beta_2 PC_{i,t-1}^2 + \gamma \mathbf{x}_{i,t-1} + \lambda_t + \nu_{it} \quad (\text{A.18})$$

$$\nu_{it} = \mu_i + \varepsilon_{i,t}$$

then to rewrite the level equation (A.17) into first differences (following the procedure of Anderson et Hsiao, 1981) in order to wipe out the unobserved country-specific effect and thereby any correlation that could stem from unchanging common driving forces. Then differentiating leads to :

$$\begin{aligned} \Delta y_{i,t} = (\lambda + 1) (y_{i,t-1} - y_{i,t-2}) + \beta_1 (PC_{i,t-1} - PC_{i,t-2}) + \beta_2 (PC_{i,t-1}^2 - PC_{i,t-2}^2) \\ + \gamma (\mathbf{x}_{i,t-1} - \mathbf{x}_{i,t-2}) + (\lambda_t - \lambda_{t-1}) + (\varepsilon_{i,t} - \varepsilon_{i,t-1}) \end{aligned} \quad (\text{A.19})$$

The three equations (A.17), (A.18) and (A.19) all achieve the same coefficients estimates. It is clear from looking at equation (A.18) that estimating (A.17) is equivalent to estimating a dynamic panel model with a lagged dependent variable on the right-hand side. The difference transformation of equation (A.17) would bring an appealing read of the results, relating changes in growth with changes in the financial sector, which is suited to answer the question “how growth responds to change of financial sector?”

To account for the very likely endogeneity of the explanatory variables (reflected by the correlation between these variables and the error term), and to deal with the correlation between the differenced lagged dependent variable $(y_{i,t-1} - y_{i,t-2})$ with the new error term $(\varepsilon_{i,t} - \varepsilon_{i,t-1})$, this method requires the use of internal instruments. At this point, the use of a fixed-effect estimator would produce biased estimations as its consistency would require an infinite number of periods T . Indeed, the within transformation induces

a correlation between the lagged dependent and the error term, thus creating a bias of order T^{-1} , referred to as Nickell's bias (Nickell, 1981). Chapter 2 further deals with this limitation (see section 3.2 page 95).

Under a couple of assumptions, a suited set of instruments would be the use of lagged levels of regressors. For such instruments to be valid, the error term $\varepsilon_{i,t}$ need to be serially uncorrelated, that is $\mathbb{E}(\varepsilon_{i,t}\varepsilon_{i,s}) = 0$ for $t \neq s$, and the lags of the explanatory variable has to be at least weakly exogenous. This means we assume the variables to be correlated with current and previous realization of the error term $\varepsilon_{i,t}$, however, future realizations of $\varepsilon_{i,t}$ should not affect current values of explanatory variables, that is :

$$\begin{aligned} \mathbb{E}(\mathbf{x}_{i,t}\varepsilon_{i,s}) &= 0 && \text{only for } t < s \\ \mathbb{E}(PC_{i,t}\varepsilon_{i,s}) &= 0 && \text{only for } t < s \end{aligned}$$

This estimation method relaxes the common assumption of strong exogeneity, allowing for reversed causality and simultaneity which is a usual concern in growth regressions. Also, it allows for some correlation between the unobserved country-specific effect μ_i and the explanatory variables, so long that changes in the latter are uncorrelated with the former country-specific effect, that is $\mathbb{E}(\Delta\mathbf{x}_{i,t}\mu_i) = 0$.

Under the previous assumptions, the following moment conditions are relevant :

$$\mathbb{E}[y_{i,t-s}\Delta\varepsilon_{i,t}] = 0 \quad \text{for } s \geq 2 ; t = 3, \dots, T \quad (\text{A.20})$$

$$\mathbb{E}[\mathbf{x}_{i,t-s}\Delta\varepsilon_{i,t}] = 0 \quad \text{for } s \geq 2 ; t = 3, \dots, T \quad (\text{A.21})$$

$$\mathbb{E}[PC_{i,t-s}\Delta\varepsilon_{i,t}] = 0 \quad \text{for } s \geq 2 ; t = 3, \dots, T \quad (\text{A.22})$$

$$\mathbb{E}[PC_{i,t-s}^2\Delta\varepsilon_{i,t}] = 0 \quad \text{for } s \geq 2 ; t = 3, \dots, T \quad (\text{A.23})$$

where $\Delta\varepsilon_{i,t} = \varepsilon_{i,t} - \varepsilon_{i,t-1}$. The GMM estimator relying only on the previous moment conditions is known as the *Difference-GMM estimator*. Its good asymptotic properties (consistency), however, do not hold in finite sample. Alonso-Borrego et Arellano (1999) and Blundell et Bond (1998) have documented a major shortcoming with this difference estimator. When the lagged dependent and the explanatory variables are persistent over

time, which is often the case with macroeconomic variables such as private credit,⁷ trade openness or per capita GDP, the lagged levels of the variable become weak instruments for the regression equation in difference. Monte Carlo experiments show that the presence of such weak instruments induces bias and poor precision in parameter estimates in finite samples. Moreover, this is exacerbated by over time persistence, the importance of the specific-effect, and the shortness of the time dimension. Also, Griliches et Hausman (1986) have shown that differencing exacerbate measurement error bias.

To deal with this, Blundell et Bond (1998) propose the *System-GMM estimator*, combining a set of moment conditions based on the level and difference equations, which produces increases in both consistency and efficiency. The country-specific factor is still holding in the level equation, which implies a new set of instruments to control for it.

The appropriate set of instruments for the added regression in levels is the set of lagged differences of the corresponding variables. Again, to be valid moment conditions, this requires a mild stationary restriction to hold (see Bun et Sarafidis, 2015). The levels of the explanatory variables may be correlated with the country-specific effect, yet the differences of these variables cannot be correlated to the latter. This assumption implies a stationary property as follow :

$$\mathbb{E} [y_{i,t+p} \cdot \mu_i] = \mathbb{E} [y_{i,t+q} \cdot \mu_i] \quad \forall p, q \quad (\text{A.24})$$

$$\mathbb{E} [\mathbf{x}_{i,t+p} \cdot \mu_i] = \mathbb{E} [\mathbf{x}_{i,t+q} \cdot \mu_i] \quad \forall p, q \quad (\text{A.25})$$

$$\mathbb{E} [PC_{i,t+p} \cdot \mu_i] = \mathbb{E} [PC_{i,t+q} \cdot \mu_i] \quad \forall p, q \quad (\text{A.26})$$

$$\mathbb{E} [PC_{i,t+p}^2 \cdot \mu_i] = \mathbb{E} [PC_{i,t+q}^2 \cdot \mu_i] \quad \forall p, q \quad (\text{A.27})$$

7. Baltagi, Demetriades et Law (2009) emphasize that financial development indicators based on asset measures are likely to be persistent over time. The size of a banking system at a given moment is closely linked to its state in the years that preceded it. Similarly, flow variables such as bank credit, are most certainly persistent. The supply and demand for loans depend on a set of clients and the size of the bank's balance sheet, which have little reason to experience large year-to-year fluctuations.

Therefore, additional moment conditions for the level equation are given by :

$$\mathbb{E}[(y_{i,t-s} - y_{i,t-s-1}) \cdot (\mu_i + \varepsilon_{i,t})] = 0 \quad \text{for } s = 1 ; t = 3, \dots, T \quad (\text{A.28})$$

$$\mathbb{E}[(\mathbf{x}_{i,t-s} - \mathbf{x}_{i,t-s-1}) \cdot (\mu_i + \varepsilon_{i,t})] = 0 \quad \text{for } s = 1 ; t = 3, \dots, T \quad (\text{A.29})$$

$$\mathbb{E}[(PC_{i,t-s} - PC_{i,t-s-1}) \cdot (\mu_i + \varepsilon_{i,t})] = 0 \quad \text{for } s = 1 ; t = 3, \dots, T \quad (\text{A.30})$$

$$\mathbb{E}\left[(PC_{i,t-s}^2 - PC_{i,t-s-1}^2) \cdot (\mu_i + \varepsilon_{i,t})\right] = 0 \quad \text{for } s = 1 ; t = 3, \dots, T \quad (\text{A.31})$$

This implies that the first-differenced variables are free from individual effects. As we already have several moment conditions based on lagged levels for the difference specification, only the most recent difference is used in the previous additional moment conditions for the levels specification (that is $s = 1$). Arellano et Bover (1995) show that other moment conditions based on further lags would be redundant.

Furthermore, Arellano et Bond (1991) present two variants of the estimator. A one-step system estimator that assumes homoscedastic errors, and a two-step version which uses the first-step errors to construct heteroscedastic-consistent standard errors (eg. White, 1982). The rationale for this two-step version is that one obtains the most efficient GMM estimator when using the weighting matrix corresponding to the variance-covariance of the moment conditions. The latter being unknown, Arellano et Bond (1991) as well as Arellano et Bover (1995) suggest the following two-step procedure. The first-step estimates are based on an initial weight matrix independent of the estimated parameters. The second step compiles the first-step estimates and uses them to compute a new weighting matrix, allowing a reestimation of the parameters. This procedure proves to be asymptotically more efficient with regard to coefficients estimates, yet tend to report severely downward biased standard errors (type-I error). To circumvent this problem, Windmeijer (2005) propose a finite sample correction, allowing for more accurate inference.

To sum up, it works as follow. First, write the growth equation in a dynamic form in the level of per capita GDP. Second, take the first difference to wipe out any country-specific effects. Third, instrument the explanatory variables in difference with lagged level values. Fourth, instrument the explanatory variables in level with lagged difference

values. This finally creates a system of overidentifying restrictions that can eliminate the inconsistency arising from the endogenous part of the right-hand side variables.

6.2 Hypothesis and Testing

The consistency of the System-GMM estimator lies within two conditions. First, in the validity of the moment conditions : whether lagged values of the explanatory variables are valid instruments in the growth regression. Second, in the validity of the assumption of no serial correlation of the error term ε_{it} . It is crucial to address these two conditions by applying the usual two specification tests proposed by Arellano et Bond (1991), Arellano et Bover (1995) and Blundell et Bond (1998).

6.2.1 Second order autocorrelation test AR(2)

The first assumption is the absence of autocorrelation : $\mathbb{E}(\varepsilon_{i,t}\varepsilon_{i,s}) = 0$ for $t \neq s$. The consistency of the estimator relies upon this assumption, which is key for a set of instruments to be valid.

If the $\varepsilon_{i,t}$ are shown to be correlated of order 1, then in the difference equation, $y_{i,t-2}$ would be endogenous to $\Delta\varepsilon_{i,t} = \varepsilon_{i,t} - \varepsilon_{i,t-1}$, making it a potentially invalid instrument (same goes for $\mathbf{X}_{i,t-2}$). One would then have to start the instruments set with the third lag and so on with order-2 serial correlation. So the first thing to check is the autocorrelation.

To this purpose, Arellano et Bond (1991) develop a AR(2) test. As the full disturbance ν_{it} is necessarily autocorrelated because of the two-way fixed effects. The concerns are for the disturbance term $\varepsilon_{i,t}$, as the very design of the model will wipe out the country-specific effects as a source of trouble through first differencing.

The trick to handle a test for the autocorrelation while ruling out fixed effects is to focus on the residuals in difference. However, since $\Delta\varepsilon_{i,t}$ and $\Delta\varepsilon_{i,t-1}$ are mathematically related through the shared $\varepsilon_{i,t-1}$, one would necessarily expect first-order negative serial correlation in difference. Hence, testing for AR(1) is uninformative. Only a test looking at second-order serial correlation in difference would account for the first-order serial correlation in level. The rationale behind this lies on the intuition that a test of second-

order serial correlation of the differenced error term would detect the correlation between $\varepsilon_{i,t-1}$ and $\varepsilon_{i,t-2}$ in $\Delta\varepsilon_{i,t}$ and $\Delta\varepsilon_{i,t-2}$, respectively.

The rule then becomes checking for serial correlation of order $l + 1$ in difference to account for its counterpart of order l in level. Moreover, we assume the absence of cross-individual correlation among the errors. Time dummies would prevent from contemporaneous correlation, which could logically appear as the most common cause of correlation is across individuals. Failing to reject the null hypothesis of the absence of second-order serial correlation will lead to the conclusion that the original error term $\varepsilon_{i,t}$ is serially uncorrelated, allowing us to use the corresponding moment conditions.

Some also see a sign of the absence of bias resulting from business-cycles effects, as this would take the form of first order correlation in the level equation (Caselli, Esquivel et Lefort, 1996).

6.2.2 Hansen J test of overidentification

The second assumption we ought to scrupulously test is the overall validity of the moment conditions upon which the System-GMM is built. In a just-identified model, conducting such a test would prove to be impossible (Wooldridge, 2010). However, in an overidentified model, we can turn to the Hansen (1982) J statistics. It relies on the minimized value of the GMM criterion function used for estimation and is usually presented as a test assessing the joint validity of the full instrument set. Under the null of the joint validity of all instruments, the sample analogs of the moment conditions used in the estimation process all have zero expectation.

The J statistic⁸ is defined as follow :

$$J = N^{-1} \left[(\mathbf{Z}_i^{\text{sys}})' \hat{\boldsymbol{\nu}}_i^+ \right]' S^{-1} (\mathbf{Z}_i^{\text{sys}})' \hat{\boldsymbol{\nu}}_i^+ \quad (\text{A.32})$$

where S is the estimate of $\mathbb{V} \left[(\mathbf{Z}_i^{\text{sys}})' \hat{\boldsymbol{\nu}}_i^+ \right]$ and follows a χ^2 distribution with $(m^{\text{sys}} - K)$ degrees of freedom, m^{sys} being the degree of overidentification (total number of

8. The Hansen test is adequate when the estimation is performed using the heteroscedastic weighting matrix, as is the case of the two-step System-GMM estimator. In the homoscedastic setup, the J test becomes the former Sargan (1958) statistics.

instruments) and K the number of right-hand-side variables. The Hansen test is robust to heteroscedasticity. Failure to reject the null of the validity of the overidentifying restriction gives support to the model.

This test ensures that the instruments set is not associated with per capita GDP growth beyond their ability to explain variation in the financial sector (and other control variables). The focus here is made on the exogeneity of the instruments only, not on their relevance (potential for weak instruments remains even if the null is not rejected).

However, emphasizing the importance of testing the overidentifying restriction to assess the validity of the moment conditions is somehow misleading. Parentes et Silva (2012) stress that whether or not the overidentifying restrictions are valid does not always provide information on the absence of correlation between the instruments and the errors. The Hansen test should be viewed as a test checking for whether or not all the instruments identify the same vector of parameters (Hausman, 1983). As a test for coherency rather than validity.

This point calls for further assessments of the relevance of the instruments through some weak instrument test. Bazzi et Clemens (2013) provide guidelines for such test.

7 Valid or Not Valid, That Is the Question

“Judging by current practice, many researchers do not fully appreciate that popular implementations of [System-GMM] estimators can by default generate results that simultaneously are invalid yet appear valid. The potential for type-I errors—false positives—is therefore substantial.”

Roodman (2009a, p.1)

The default implementation of the System-GMM estimator generates sets of instruments whose number increases particularly quickly with the time dimension of the panel. This dramatic increase (somehow pandemic) of the instrument count is often referred to as *instruments proliferation*. Several problems stem from excessive proliferation, such as : overfitting of endogenous variable, weakened test for over-identifying restrictions, biased two-step variance estimators, imprecise estimates of the optimal weighting matrix.⁹

7.1 The Instrument Count

7.1.1 A Simple setup

In order to have a clearer insight on this problem of instruments proliferation, and for simplicity, focus on the construction of the instruments sets for the simplified form of model (I.2) for ease of exposition :

$$y_{i,t} = \mathbf{X}'_{i,t-1} \boldsymbol{\Omega} + \lambda_t + \nu_{it} \quad (\text{A.33})$$

where $\mathbf{X}'_{i,t}$ is the stacked set of regressors and lagged dependent variables such that :

$$\underset{(K+1;1)}{\mathbf{X}'_{i,t-1}} = \begin{pmatrix} y_{i,t-1} \\ PC_{i,t-1} \\ PC^2_{i,t-1} \\ \mathbf{x}_{i,t-1} \end{pmatrix} ; \quad \underset{(K+1;1)}{\boldsymbol{\Omega}} = \begin{pmatrix} (\lambda + 1) \\ \beta_1 \\ \beta_2 \\ \gamma \end{pmatrix} \quad (\text{A.34})$$

The model displays the lagged dependent variable and K additional controls as variables. With strictly exogenous regressors $\mathbf{x}_{i,t}$ we would have $[x'_{i,1}, x'_{i,2}, \dots, x'_{i,T}]$ as

9. This problem is usually signaled by the singularity of the matrix during computation.

valid instruments for each period. However, the aforementioned weak exogeneity assumption ($\mathbb{E}(\mathbf{x}_{i,t}\varepsilon_{i,s}) = 0$ for $t < s$) reduces the set of extra valid instruments available to $[x'_{i,1}, x'_{i,2}, \dots, x'_{i,s-2}]$ at period s . The control variables enter the model lagged one period (initial value of the 5 year period), which is the classical treatment for endogenous variables. Therefore, the lagged dependent variable and the lagged control variables produces the same number of available instruments, enabling us to group the two category of variable into $\mathbf{X}'_{i,t}$ as we will display the matrices.

7.1.2 Difference-GMM

First, looking at the Difference-GMM part of the system, the moment conditions (A.20-A.23) can be reformulated in a more compact form as :

$$\mathbb{E} \left[\left(\mathbf{Z}_i^{\text{dif}} \right)' \Delta \bar{\nu}_i \right] = \mathbf{0} \quad (\text{A.35})$$

where $\mathbf{Z}_i^{\text{dif}}$ refers to the $(T-1) \times m^{\text{dif}}$ sparse instrument matrix given by :

$$\mathbf{Z}_i^{\text{dif}} = \begin{bmatrix} 0 & 0 & 0 & 0 & 0 & 0 & \dots & 0 & \dots & 0 \\ \mathbf{X}'_{i,1} & 0 & 0 & 0 & 0 & 0 & \dots & 0 & \dots & 0 \\ 0 & \mathbf{X}'_{i,1} & \mathbf{X}'_{i,2} & 0 & 0 & 0 & \dots & 0 & \dots & 0 \\ 0 & 0 & 0 & \mathbf{X}'_{i,1} & \mathbf{X}'_{i,2} & \mathbf{X}'_{i,3} & \dots & 0 & \dots & 0 \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots & \ddots & \vdots & & \vdots \\ 0 & 0 & 0 & 0 & 0 & 0 & \dots & \mathbf{X}'_{i,1} & \dots & \mathbf{X}'_{i,T-2} \end{bmatrix} \quad (\text{A.36})$$

and $\Delta \bar{\nu}_i$ is the $(T-1)$ vector

$$\Delta \bar{\nu}_i = \begin{bmatrix} \Delta \nu_{i,2} \\ \Delta \nu_{i,3} \\ \vdots \\ \Delta \nu_{i,T} \end{bmatrix} \quad (\text{A.37})$$

The standard instruments set for the moment conditions of the difference part of the System-GMM estimator has the specificity of including separate instruments for each period (Holtz-Eakin, Newey et Rosen, 1988).

In period 2, variables from period 1 are valid instruments, in period 3, variables from both periods 1 and 2 are valid. So on up to period T , where all variables from period 1 to $T - 2$ are valid instruments. After first differencing, the transformed variables being instrumented necessarily begin at $t = 2$. Then, matrix (A.36) also starts at $t = 2$ for which only its first observation lacks instruments. Indeed, $\Delta y_{i,2} = y_{i,2} - y_{i,1}$ cannot be instrumented, whereas $\Delta y_{i,3} = y_{i,3} - y_{i,2}$ can with $y_{i,1}$. Therefore, the first row displays only zeros.

This type of instruments is referred to as GMM-style instruments. Such instrument processing can generate moment conditions prolifically, with the instrument count being quadratic in T , the time dimension of the panel. In this case, the total number of moment conditions generated with the default implementation of the GMM estimator with T periods would be :

$$m^{\text{dif}} = (1 + 2 + \dots + T - 2) (K + 1) = \frac{(T - 1)(T - 2)}{2} (K + 1) \quad (\text{A.38})$$

The first chapter's setup, closely following Arcand, Berkes et Panizza (2015), has six left-hand side variables (including the financial depth variable). For $T = 6$ the difference equation generates 70 moment conditions, 105 for $T = 7$, 147 for $T = 8$, 196 for $T = 9$, 252 for $T = 10$ and 315 for $T = 11$, which is the largest time span available based on 5-year spells over 1960-2015. Furthermore, this is just for the difference part of the system-GMM.

7.1.3 Level-GMM

In addition, several moment conditions arise from the level equation instrumented with lagged differences. Moment conditions (A.24-A.27) can then be rewritten as :

$$\mathbb{E} \left[\left(\mathbf{Z}_i^{\text{lev}} \right)' \bar{\mathbf{v}}_i \right] = \mathbf{0} \quad (\text{A.39})$$

where $\mathbf{Z}_i^{\text{lev}}$ refers to the $(T-1) \times m^{\text{lev}}$ instrument matrix given by :

$$\mathbf{Z}_i^{\text{lev}} = \begin{bmatrix} 0 & 0 & \cdots & 0 \\ \Delta \mathbf{X}'_{i,2} & 0 & \cdots & 0 \\ 0 & \Delta \mathbf{X}'_{i,3} & \cdots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \cdots & \Delta \mathbf{X}'_{i,T-1} \end{bmatrix} ; \quad \Delta \mathbf{X}_{i,t} \equiv \begin{pmatrix} \Delta y_{i,t} \\ \Delta \mathbf{x}_{i,t} \end{pmatrix} \quad (\text{A.40})$$

and $\bar{\nu}_i$ is the following $(T-1)$ vector

$$\bar{\nu}_i = \begin{bmatrix} \nu_{i,2} \\ \nu_{i,3} \\ \vdots \\ \nu_{i,T} \end{bmatrix} \quad (\text{A.41})$$

This second part of the system still produces some instruments to add to the count. Unlike the instrumentation of the difference equation, only the most recent differences are used to instrument the level part. Indeed, Blundell et Bond (1998) show that using deeper lags would be redundant with respect to the moment conditions already retained in equation A.35. Thus, the extra instruments are

$$m^{\text{lev}} = (T-2)(K+1) \quad (\text{A.42})$$

7.1.4 System-GMM

The full set of moment conditions (A.35) and (A.39) can then be rewritten as the following moment condition :

$$\mathbb{E} \left[(\mathbf{Z}_i^{\text{sys}})' \bar{\nu}_i^+ \right] = \mathbf{0} \quad (\text{A.43})$$

where $\mathbf{Z}_i^{\text{sys}}$ refers to the $2(T-1) \times m^{\text{sys}}$ full instrument matrix given by :

$$\mathbf{Z}_i^{\text{sys}} = \begin{bmatrix} \mathbf{Z}_i^{\text{dif}} & 0 \\ 0 & \mathbf{Z}_i^{\text{lev}} \end{bmatrix} \quad (\text{A.44})$$

and where $\bar{\nu}_i^+ = [\Delta\bar{\nu}_i, \bar{\nu}_i]'$ is a $2(T-1)$ vector.

The total count of instruments¹⁰ generated by this default implementation of the System-GMM estimator would be :

$$m^{\text{sys}} = m^{\text{dif}} + m^{\text{lev}} = \frac{(T+1)(T-2)}{2} (K+1) \quad (\text{A.45})$$

The structure of the instrument matrix is such that instrument proliferation is done primarily through the moment conditions on the difference equation. As we have already mentioned, Blundell et Bond (1998) have shown that in the presence of variables whose persistence is a notable characteristic, the level instruments are often weak instruments for the difference variables. Thus, there is a non-negligible risk of profiling weak instruments.

Moreover, as shown by equation (A.45), the proliferation is primarily due to the moment conditions on the difference equation $O(T^2)$, as opposed to only $O(T)$ for the level part. Thus, moving from difference to System-GMM remains the same for instrument count. Since the dimension of the GMM-type instrument matrix grows exponentially both as the number of periods and regressors expands :

“[...] the phenomenon of moment condition proliferation is far from being a theoretical construct and arises naturally in many empirical econometric settings, and [...] is endemic to dynamic panel data models in which instruments are generated from lagged dependent variables and the time span of the panel is of moderate size” (Han et Phillips, 2006, p. 149).

7.2 Instrument Proliferation

The dangers associated with a high count of instruments relative to the number of cross-sectional observations are widely documented in the applied literature. Andersen et Sorensen (1996) are among the first to address the choice of the number of instruments to include in an estimation procedure. The usual theory is that an increasing number

10. To be precise, we would have to add the additional moment conditions arising from the time fixed effect, threaded with dummies variables, used as exogenous controls and instrumented in an IV fashion. However, compared to the moment conditions arising from the GMM-style instruments, these extra conditions are small.

of instruments improves the properties of the estimator. However, they show this proves to be an unreliable guideline in small samples, as the inclusion of an excessive number of moment conditions results in more pronounced bias. Thus “the use of additional information can be harmful” (Andersen et Sorensen, 1996, p. 329).

Performing an extensive set of Monte Carlo experiments for a dynamic panel data model, Ziliak (1997, p. 419) also finds that “bias in GMM is quite severe as the number of moment conditions expands, outweighing the gains in efficiency.” He concludes that one is better off lowering the instrument count to work around overfitting bias.

When it comes to the number of overidentifying restrictions, Koenker et Machado (1999) point out an antagonism between what theory advocates and what practice permits. In theory, “the more, the merrier”. Additional moment conditions should increase asymptotic precision. However, this still requires that the instrument count remains finite. Confronting the data, the problem usually surges as this count can grow without bound relative to the sample size.

Alonso-Borrego et Arellano (1999) also come to the observation that the dynamic panel model generates many over-identifying restrictions even for moderate values of T . Moreover, they conclude that, as the number of instruments increases with T , the quality of such extra instruments is often poor because they tend to be only weakly correlated with first-differenced endogenous variables. This has been one of the rationales for implementing the System-GMM, which complements the moment conditions from the difference equation with extra conditions in level.

However, the burden of instrument proliferation and weak instruments lies in the laps of the overidentifying restrictions generated by the difference part of the system. There are, therefore, severe doubts about the possibility of conducting reliable estimates using the System-GMM estimator in the context of a proliferation of instruments. Switching to either Difference-GMM or Level-GMM would not be of much help, as the System-GMM estimator, despite generating more instruments, is the least biased of them (Hayakawa, 2007).

Using Monte Carlo experiments, Bowsher (2002) focuses on the effect of the dimensionality of the moment conditions on the finite sample properties of overidentifying

restriction tests. The concern here is to assess the ability to test the validity of the difference and system moment conditions given data sets of the size usually encountered in the empirical literature.

The work of Bowscher (2002) is of crucial importance as it puts forward several key conclusions. Firstly, “the Hansen test is found to have no power in panels of dimensions that are commonly encountered in empirical work” (p. 211). Indeed, the test based on the full instrument set is “found to have a zero null rejection frequency and ‘no power’ in panels where (N, T) would not be judged unusual by the standards of data sets currently available” (p. 220). Using a AR(1) model with $\delta = 0, 4$, the following (N, T) pairs : (125, 16), (85, 13), (70, 12), and (40, 10) were associated with zero null rejection frequencies (using the 10% nominal critical value). Notice that this is exacerbated by the additional control variable, further raising the instrument count. Therefore, in what follows, pairs of (N, T) such as (100, 10) or (130, 11) should be viewed with much caution.

In other words, with instrument proliferation due to the use of the full instrument set available, the Hansen test never rejects, which is a critical source of concern. Moreover, in such an instrument proliferation setup, the Hansen test is shown to have no power to detect the serial correlation in the error term. Secondly, and fortunately, the problem can be avoided by working with a smaller set of instruments, offering substantial gains in the properties of the test.

Alvarez et Arellano (2003) highlight the risk of settings where too many instruments produce overfitting and undesirable closeness to OLS coefficients. They demonstrate that in some cases, when the high count of overidentifying restrictions is associated with a large T , the closeness to OLS (within-group estimator) becomes desirable as Nickell’s bias tend to zero as T tends to infinity.

However, as the empirical growth literature works with period average, the time dimension becomes large enough to create too many instruments (for now at most 11 periods of 5 years), yet not large enough to safely consider that T tends to infinity. Han et Phillips (2006) explore the effect of proliferating moment conditions and emphasize that the estimation of the true parameter genuinely depends on both the number and the quality of the moment conditions.

By aggregating a vast body of research in this area, Roodman (2009a) provides a clear synthesis of the dangers associated with the proliferation of instruments. We can thus draw the following points of convergences :

Overfitting endogenous variables This caveat is a classical one that arises in various instrumental variable setup. Requesting a large number of instruments can over-fit instrumented variables, and fail to expunge their endogenous components. It creates an undesirable bias of coefficient estimates towards those from non-instrumented estimators, namely plain vanilla OLS. For intuition, if the number of instruments equals the number of observations, then the second stage of a 2SLS is identical to OLS. A broad rule of thumbs is to keep the instrument count below the number of cross-sectional observations. Too many instruments will necessarily dampen the large-sample results.

Imprecise estimates of the optimal weighting matrix This problem relates to feasible efficient GMM techniques requiring sample moments to estimate an optimal weighting matrix for the overidentifying restrictions.

The two-step variant of the System-GMM uses a weighting matrix that is the inverse of an estimate of S , the covariance of the moment such that $S = \mathbb{V} [(\mathbf{Z}_i^{\text{sys}})' \hat{\boldsymbol{\nu}}_i^+]$ where $\hat{\boldsymbol{\nu}}_i^+$ is the empirical errors extracted from the first stage.

In this case, the proliferation of instruments surges through the fact that the number of moment and cross-moment to be estimated in S is quadratic with respect to the number of instruments, which mechanically means that it is quartic in T . It is a function of estimated fourth moments for the optimal weighting matrix.

As (Hayashi, 2000, p. 215) underlines, “it takes a substantially larger sample size to estimate fourth moments reliably than to estimate first and second moments. So we would expect the efficient GMM estimator to have poorer small-sample properties than the GMM estimators that do not use fourth moments” for the weighting matrix.

A common symptom of the instrument proliferation is a singular estimate of S . In the latter case, the second step calls for a generalized inverse of S . Note that this does not affect the consistency of the System-GMM estimator, only its efficiency

(Arellano et Bond, 1991 ; Windmeijer, 2005). This illustrates, once again, how a large number of instruments may drive us far from the ideal world of theory.

Downward-bias in two-step standard errors Coefficients standard errors in two-step GMM tend to be severely downward biased in an environment prone to instrument proliferation. However, Windmeijer (2005) has provided a corrected version of these standard errors. Again, this illustrates the downside of too many instruments, dragging the empirics far from theory.

Weak Hansen test of instrument validity Probably the most harmful consequence of instrument proliferation, the Hansen is severely vitiated to a point where it never rejects. The issue seems to come from the fact that somehow, the largest moments (those that are the least satisfied) also get the least weight in S^{-1} , which can be the source of a false appearance of a valid fit.

High p-value on this overidentifying restriction test is considered as the cornerstone of researchers' arguments for the validity of estimated results. Roodman (2009b) emphasizes that a perfect Hansen statistic of 1.000 is a telltale sign of instrument proliferation.

Closely related to this issue, the difference-in-Hansen test is also weakened by a high instrument count. This is of much concern as this test is used to probe the assumption (A.24) needed for the validity of the extra moment conditions leading to the System-GMM. This assumption is far from being obvious. In the empirical study of economic growth models, it is easy to imagine a systematic relationship between a country's fixed effect and its distance from its conditional steady state at the beginning of the period.

The aforementioned researches have a common conclusion : the GMM estimator that uses the full set of moment conditions available can be severely biased, especially when the instruments are weak, and the number of moment conditions is large relative to N (see Baltagi, 2013, ch. 8 for an extensive survey). This downside clearly calls for a modified version of the GMM setup, with reduced sets of moment conditions that lead to estimates with reasonable finite sample properties.

8 Reducing the Dimensionality of the Moment Conditions

Looking into these issues is not just mere persnickety. Many empirical studies since the 2000s have paid particular attention to the problem of proliferation of instruments. Far from being incidental, this question must be central in order to determine the quality of the results as well as the conclusions and recommendations that are based on them.

The literature has come up with several ways around the issue of instrument proliferation. This section goes on first presenting these strategies and then drawing a panorama of the finance-growth literature on this issue. There are commonly three major strategies to alleviate the instrument proliferation problem : the lag-depth truncation, the collapsing of the instrument set, and less commonly the use of principal components of the instruments.

8.1 The Lag-Depth Truncation

As previously mentioned, the unconstrained implementation of the Arrelano and Bond estimator makes extensive use of lagged instruments, even in a simple model. Because of the particular structure of the instrument matrix (A.44), this contributes significantly to instrument proliferation. Moreover, Alonso-Borrego et Arellano (1999) remark that the most distant instruments are generally those which offer the weakest correlation and are therefore less relevant. Combining these two findings, a natural solution, often considered, is to reduce the dimensionality of the instrument matrix by using only a subset of available instruments (Bowsher, 2002 ; Okui, 2009 ; Roodman, 2009b).

Lag-depth truncation has become a common practice, to the point that the widely used textbook by Wooldridge (2010, p. 372) directly recommends that “it might be better to use just a couple of lags.” This first technique still generates separate instruments for each period, but the number per period is now capped. We only exploit the moment conditions for a certain lag depth such that we can rewrite moment condition (A.19) as follow :

$$\mathbb{E}[\mathbf{X}_{i,t-s}\Delta\varepsilon_{i,t}] = 0 \quad \text{for } 2 \leq s < M ; t = 3, \dots, T \quad (\text{A.46})$$

where M is the maximum lag depth considered. This way, the instrument count becomes

Table A.15 – Lag-depth truncation in System-GMM panel regressions extending Table I.5.

Instrument matrix : GMM-type	(1)	(2)	(3)	(4)
Number of lags : 1	1960-1995	1960-2000	1960-2005	1960-2010
Private Credit	8.876** (3.824)	5.408** (2.478)	2.089 (2.001)	2.694 (2.025)
(Private Credit) ²	-4.266* (2.195)	-2.149* (1.266)	-1.224 (1.057)	-1.970** (0.952)
Log(init. GDP/cap.)	-0.164 (0.452)	-0.390 (0.444)	-0.448 (0.421)	-0.317 (0.305)
Log(school)	0.768 (0.936)	1.534* (0.834)	2.273** (0.970)	2.016*** (0.745)
Log(inflation)	-0.253 (0.242)	-0.188 (0.203)	-0.457** (0.189)	-0.393** (0.198)
Log(trade)	1.990** (0.861)	2.067*** (0.755)	1.126* (0.683)	1.291* (0.759)
Log(gov. cons.)	-4.110*** (1.218)	-3.268*** (0.737)	-2.291*** (0.757)	-2.474*** (0.594)
Number of instruments	77	92	107	122
Number of countries	107	127	131	133
Observations	549	675	798	917
AR(2) (<i>p</i> -value)	0.63	0.20	0.29	0.10
Hansen test (<i>p</i> -value)	0.41	0.44	0.42	0.51
Hansen test exclud. level IV	0.83	0.63	0.30	0.23
<i>Diff-in-Hansen</i>	0.15	0.29	0.56	0.76
Hansen test exclud. $\ln(\text{GDP})_{t-1}$	0.25	0.39	0.40	0.43
<i>Diff-in-Hansen</i>	0.85	0.55	0.46	0.63
dGrowth/dPC=0	104%**	126%*	85%	68%
90% Fieller CI	[80%–239%]	[80%–1443%]	–	–
SLM (<i>p</i> -value)	0.04	0.09	0.17	0.16

Notes : This table reports the results of a set of panel regressions consisting of ten non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). The SLM test provides *p*-value for the relevance of the estimated threshold. Windmeijer standard errors in parentheses. This table extends the regression presented in column (2) of Table I.5 page 57. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

linear in T . For example, limiting the maximum lag depth of the left-hand-side variables to $M = 3$ (the depth is $\tau = 1$) gives the following instrument matrix for the difference equation :

$$\left\{ \mathbf{Z}_i^{\text{dif}} \right\}^L = \begin{bmatrix} 0 & 0 & 0 & \cdots & 0 \\ \mathbf{X}'_{i,1} & 0 & 0 & \cdots & 0 \\ 0 & \mathbf{X}'_{i,2} & 0 & \cdots & 0 \\ 0 & 0 & \mathbf{X}'_{i,3} & \cdots & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & 0 & \cdots & \mathbf{X}'_{i,T-2} \end{bmatrix} \quad (\text{A.47})$$

The lag-depth truncation technique reduces the instrument count to

$$\left\{ m^{\text{dif}} \right\}^L = m^{\text{dif}} - \frac{(T-2-\tau)(T-1-\tau)}{2} (K+1) \quad (\text{A.48})$$

When we restrict the lag depth to $\tau = 1$, the number of moment conditions generated by the difference equation can be simplified to $\left\{ m^{\text{dif}} \right\}_{\tau=1}^L = (T-2)(K+1)$, which is clearly linear in T and far much smaller than the unrestricted m^{dif} . On the other hand, the level part of the System-GMM remains unaffected, as it already uses only one lagged difference due to redundancy. The total number of instrument is then $\left\{ m^{\text{sys}} \right\}^L = \left\{ m^{\text{dif}} \right\}^L + m^{\text{lev}}$, which in the particular case of $\tau = 1$ gives

$$\left\{ m^{\text{sys}} \right\}_{\tau=1}^L = (T-2)(K+1) \quad (\text{A.49})$$

Table A.15 implements the lag-depth truncation technique, with only 1 lag.

8.2 The Collapsing of the Instrument Set

The early work of Calderon, Chong et Loayza (2002) has tackled the issue of instrument proliferation and provides a widely used method of limiting the possibility of overfitting by restricting the dimensionality of the instrument set in such ways to make the instrument count also linear in T .

This method is commonly referred to as *collapsing*. Whereas limiting the lag depth still rely on a different set of instruments for each period. Collapsing works around with

moment conditions applied such that each of them corresponds to all available periods. It is done by imposing a different type of moment conditions :

$$\mathbb{E}[\mathbf{X}_{i,t-s}\Delta\varepsilon_{i,t}] = 0 \quad \text{for } s \geq 2 ; \overline{t=3, \dots, T} \quad (\text{A.50})$$

$$\mathbb{E}[(\mathbf{X}_{i,t-s} - \mathbf{X}_{i,t-s-1}) \cdot (\mu_i + \varepsilon_{i,t})] = 0 \quad \text{for } s = 1 ; \overline{t=3, \dots, T} \quad (\text{A.51})$$

where this translate into the instrument sets (A.36) and (A.40) with “collapsed” blocks as follow :

$$\left\{ \mathbf{Z}_i^{\text{dif}} \right\}^C = \begin{bmatrix} 0 & 0 & 0 & \cdots & 0 \\ \mathbf{X}'_{i,1} & 0 & 0 & \cdots & 0 \\ \mathbf{X}'_{i,2} & \mathbf{X}'_{i,1} & 0 & \cdots & 0 \\ \mathbf{X}'_{i,3} & \mathbf{X}'_{i,2} & \mathbf{X}'_{i,1} & \cdots & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ \mathbf{X}'_{i,T-2} & 0 & 0 & \cdots & \mathbf{X}'_{i,1} \end{bmatrix} ; \quad \left\{ \mathbf{Z}_i^{\text{lev}} \right\}^C = \begin{bmatrix} 0 \\ \Delta \mathbf{X}'_{i,2} \\ \Delta \mathbf{X}'_{i,3} \\ \vdots \\ \Delta \mathbf{X}'_{i,T-1} \end{bmatrix} \quad (\text{A.52})$$

The collapsed instrument matrix has the apparent advantage of reducing drastically the instrument count to :

$$\{m^{\text{sys}}\}^C = \{m^{\text{dif}}\}^C + \{m^{\text{lev}}\}^C = (T - 1)(K + 1) \quad (\text{A.53})$$

This technique has the potential advantage of retaining more information than the previous technique, as no lags are dropped. At this time, one should keep in mind that more information can be harmful if these additional lags reveal to be weak instruments. Therefore, it is quite possible to combine both techniques, the instrument count then becoming invariant to T .

Table A.16 implements the collapsed instrument matrix, with all available lags.

8.3 Principal Components of the Instruments

A more recent line of research has focused on replacing the numerous instruments by their principal components (PCA). The main advantage is the ability to reduce the

Table A.16 – Collapsed matrix in System-GMM panel regressions extending Table I.5.

Instrument matrix : COLLAPSED	(1)	(2)	(3)	(4)
Number of lags : ALL	1960-1995	1960-2000	1960-2005	1960-2010
Private Credit	5.658 (4.707)	3.519 (2.704)	1.814 (2.541)	0.689 (2.972)
(Private Credit) ²	-2.721 (2.687)	-0.980 (1.578)	-1.580 (1.614)	-0.882 (1.390)
Log(init. GDP/cap.)	0.535 (0.807)	-0.295 (0.608)	-0.439 (0.524)	-0.957* (0.525)
Log(school)	0.188 (1.526)	1.850 (1.224)	2.501** (1.217)	3.738*** (1.040)
Log(inflation)	0.118 (0.351)	-0.198 (0.294)	-0.716** (0.351)	-0.875** (0.377)
Log(trade)	4.527** (1.994)	3.261** (1.321)	3.496** (1.557)	3.532** (1.437)
Log(gov. cons.)	-4.707*** (1.657)	-3.912*** (1.370)	-2.812** (1.315)	-1.452 (1.227)
Number of instruments	49	57	65	73
Number of countries	107	127	131	133
Observations	549	675	798	917
AR(2) (<i>p</i> -value)	0.82	0.18	0.15	0.022
Hansen test (<i>p</i> -value)	0.86	0.96	0.46	0.19
Hansen test exclud. level IV	0.89	0.95	0.39	0.20
<i>Diff-in-Hansen</i>	0.44	0.69	0.60	0.33
Hansen test exclud. $\ln(\text{GDP})_{t-1}$	0.80	0.88	0.30	0.16
<i>Diff-in-Hansen</i>	0.69	0.96	0.84	0.47
dGrowth/dPC=0	104%	179%	57%	39%
90% Fieller CI	–	–	–	–
SLM (<i>p</i> -value)	0.21	0.41	0.30	0.47

Notes : This table reports the results of a set of panel regressions consisting of ten non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). The SLM test provides *p*-value for the relevance of the estimated threshold. Windmeijer standard errors in parentheses. This table extends the regression presented in column (3) of Table I.5 page 57. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

number of instruments in a minimally arbitrary way (Kapetanios et Marcellino, 2010; Bai et Ng, 2010; Bontempi et Mammi, 2012). Indeed, this strategy has an advantage over others of being statistically founded and purely data-driven. Table A.17 implements this techniques with PCA of all available lags.

Documenting the trouble with instrument proliferation, Mehrhoff (2009) concludes that untransformed PCA displays significant biases in Monte Carlo simulation. However, it performs reasonably well when the instrument matrix is collapsed and/or truncated before factorization. This study draws a clear recommendation to collapse the instrument when using the PCA alternative. Table A.17 provides results with a collapsed PCA instrument matrix.

However, such implementation can induce a very low degree of overidentification, which can often result in underidentification. Vigilance is required when implementing this instrument reduction technique because a very low degree of over-identification can also be problematic as to the reliability of the specification tests.

8.4 How Much Is Too Much ?

So far, this central question has received neither a definitive nor a precise answer. A precise rule does not exist. Assessing the relevance of the results becomes difficult.

A rule of thumbs has emerged in practice. It states that the instrument count should not exceed the number of cross-sectional observations (i.e., the number of countries in the panel). However, since this threshold has no particular theoretical basis, sticking to it can be misleading as this does not safeguard from the potential type-I error mentioned above.

At this stage, the absence of formal a procedure to determine how far one should go reducing the number of moment conditions should not be view as a drag. Roodman (2009a, p. 27) stresses that both “results and specification tests should be *aggressively* tested for sensitivity to reductions in the number of instruments.” The first chapter follows this recommendation, insisting on the aggressive aspect of the reduction as a test of the results’ robustness.

Figure A.4 summarizes the tension related to the proliferation of instruments as well

Table A.17 – Principal components of the instruments in System-GMM panel regressions extending Table I.5.

Instrument matrix : GMM-type	(1)	(2)	(3)	(4)
Number of lags : PCA of all	1960-1995	1960-2000	1960-2005	1960-2010
Private Credit	1.153 (6.360)	-3.454 (4.755)	-3.702 (3.700)	-3.267 (2.107)
(Private Credit) ²	-0.387 (3.905)	2.999 (3.087)	1.591 (2.395)	0.924 (0.964)
Log(init. GDP/cap.)	-0.939 (1.032)	-1.053 (0.844)	-1.144** (0.552)	-0.853 (0.541)
Log(school)	3.762** (1.778)	4.704*** (1.410)	5.532*** (1.187)	5.568*** (1.438)
Log(inflation)	-0.882** (0.428)	-0.713* (0.390)	-1.146*** (0.348)	-1.024*** (0.394)
Log(trade)	0.355 (1.542)	1.267 (1.223)	0.196 (0.864)	1.235 (0.876)
Log(gov. cons.)	-2.314 (1.448)	-2.140 (1.304)	-1.640* (0.896)	-2.242** (0.995)
Number of instruments	31	37	44	51
Number of countries	107	127	131	133
Observations	549	675	798	917
AR(2) (<i>p</i> -value)	0.60	0.11	0.17	0.04
Hansen test (<i>p</i> -value)	0.38	0.58	0.57	0.07
dGrowth/dPC=0	149%	58%	116%	117%
90% Fieller CI	–	–	–	–
SLM (<i>p</i> -value)	0.48	0.30	0.31	0.34
PCA <i>R</i> ²	0.85	0.85	0.86	0.86

Notes : This table reports the results of a set of panel regressions consisting of ten non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). The SLM test provides *p*-value for the relevance of the estimated threshold. Windmeijer standard errors in parentheses. This table extends the regression presented in column (5) of Table I.5 page 57. *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Table A.18 – Collapsed PCA matrix in System-GMM panel regressions extending Table I.5.

Instrument matrix : COLLAPSED	(1)	(2)	(3)	(4)
Number of lags : PCA of all	1960-1995	1960-2000	1960-2005	1960-2010
Private Credit	104.882 (265.847)	-24.750 (87.708)	-13.010 (21.416)	-15.834 (10.411)
(Private Credit) ²	-64.455 (175.999)	7.713 (31.598)	-4.964 (12.315)	4.660 (3.357)
Log(init. GDP/cap.)	3.970 (23.704)	2.229 (9.365)	10.144 (14.618)	1.591 (1.917)
Log(school)	-31.655 (102.238)	1.011 (13.196)	-3.355 (10.699)	4.872** (1.910)
Log(inflation)	2.847 (8.106)	2.126 (5.985)	-0.091 (2.985)	-1.819*** (0.561)
Log(trade)	-1.938 (23.563)	-0.764 (16.113)	17.636 (18.920)	3.370 (2.977)
Log(gov. cons.)	-2.780 (12.314)	29.218 (86.217)	-14.819 (13.846)	-1.652 (7.501)
Number of instruments	14	15	17	19
Number of countries	107	127	131	133
Observations	549	675	798	917
AR(2) (<i>p</i> -value)	0.76	0.67	0.28	0.02
Hansen test (<i>p</i> -value)	–	–	0.90	<0.01
dGrowth/dPC=0	81%	160%	–	170%
90% Fieller CI	–	–	–	–
SLM (<i>p</i> -value)	0.36	0.44	–	0.14
PCA <i>R</i> ²	0.81	0.80	0.81	0.83

Notes : This table reports the results of a set of panel regressions consisting of ten non-overlapping five-year spells. The dependent variable is the average real GDP per capita growth rate. All regressions contain time fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). The SLM test provides *p*-value for the relevance of the estimated threshold. Windmeijer standard errors in parentheses. This table extends the regression presented in column (6) of Table I.5 page 57. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

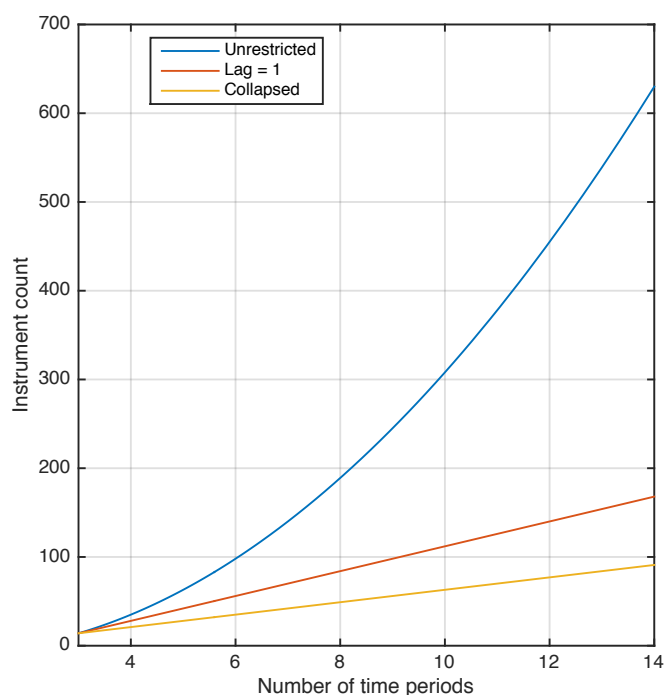


Figure A.4 – Instrument proliferation and remedies. Instrument counts from equations (A.45), (A.49) and (A.52) with the same number of variables as the baseline regressions in chapter 1 ($K = 6$).

as the ability of the proposed solutions. Thus, focussing on the same number of variables over the range of periods considered in the ABP study, where $T = 10$ at most, the unrestricted instrument matrix creates as much as 309 moment conditions.¹¹

Reducing the number of lags to one or using a collapsed instrument matrix can drastically reduce the number of instruments (to 112 and 63 moment conditions, respectively). Each method has its specificity. The first maintains a matrix structure identical to the original Arellano et Bond estimator but by reducing the number of occurrences. The second modifies the structure of the matrix, which is similar to the one used by the 2SLS estimator. The combination of the lag depth restriction and the collapsed form provides an even more drastic reduction in the number of overidentifying restrictions. Many empirical studies have paid particular attention to the issue of instrument proliferation and handled it with a severe reduction of the instrument matrix dimensionality.

11. It is evident from the instrument count that the tendency for the number of moments to become very large as T increases is even more pronounced as one adds regressors to the model.

An anthology of papers shows concerns and particular attention to limiting the instrument counts. Indeed, the issue of the proliferation of instruments was already a matter of interest among the first lines of research papers on the links between finance and growth using the System-GMM estimator.

The first attempt to apply the dynamic panel GMM estimator to the finance-growth nexus can be found in the two seminal papers of Levine, Loayza et Beck (2000) and Beck, Levine et Loayza (2000). Both demonstrate a thorough appreciation of the dangers of instrument proliferation. The authors rely here on the rule of thumbs based on the number of cross-sectional observations already mentioned to rule out any doubts about the quality of the results.

In order to reduce the number of overidentifying restrictions, they only retain the first available lagged level for the difference part of the System-GMM. Hence, they resort to the second solution previously mentioned with $\tau = 1$.

Using Monte Carlo experiments, Bowsher (2002) shows that a simple procedure of reduction of the number of lags improves the properties of the conventional test. His advice is to restrict the number of lags when implementing System-GMM. Studying the linkage between financial development and trade openness, Baltagi, Demetriades et Law (2009) work with a subset of instruments to circumvent “convincing evidence that too many moment conditions introduce bias.” They first restrict the moment conditions to a maximum of two lags and work, in a second time, with just one lag to further reduce the instrument count in order to ensure it stays below the number of cross-sectional observations. This approach has the advantage of gradually controlling for the robustness to the reduction in the lag depth.

Working on the potential harm of finance for economic growth, Law et Singh (2014) also choose to reduce the instrument count to prevent spurious results using a lag depth of one. In their study assessing the presence of a threshold in the process of international financial integration, Kose, Prasad et Taylor (2009) also resort to the System-GMM estimator with a lag depth of one to limit as much as possible the overfitting.

The early System-GMM application of Calderon, Chong et Loayza (2002) is also an excellent example of the severe concern toward instrument proliferation. It also provides

a useful example of ingenuity in finding a solution to this problem. Indeed, they came up with a very different solution to deal with the excessive dimensionality of the moment conditions as the time span is lengthening. This solution is the previously mentioned *collapsing* method. Notice that it appears quite early in the literature. It underlines the importance given by researchers to this question from the very beginning of empirical exercises. Beck et Levine (2004) present with much detail the pitfalls of the unrestricted implementation of System-GMM in the finance-growth setup. They make an application of the alternative system estimator of Calderon, Chong et Loayza (2002) to avoid the overfitting problem along with the uninformative tests coming with it.

Working on foreign direct investments with System-GMM estimator, Carkovic et Levine (2005) make use of both techniques to reduce the instrument count : a collapsed instrument matrix with a lag depth of 1. This case is compelling as it is less common, although nothing prevents it from being used. It makes it possible to overcome the problem more firmly. However, this may alter the properties of the instruments. This case deserves to be considered in a complementary way to other approaches to reduce the number of instruments.

The PCA option has been the subject of fewer applications in the literature on financial development so far. It is only recently that studies have seized this tool. Revisiting finance-growth nexus, Beck, Georgiadis et Straub (2014) pay careful attention to limit the instrument count. To do so, they follow the recommendations made by Mehrhoff (2009) by combining a collapsed matrix with principal components of the instruments.

This panorama of the research shows that from its earliest stages, it has managed to grasp the issue and provide answers to wave concern and draw causal conclusions, departing from the default implementation of the estimator.

To emphasize that the thumb rule is not an absolute shield safeguarding us from any spurious conclusions, Roodman (2009a) conduces a replication work to assess the robustness of the seminal work of Levine, Loayza et Beck (2000) to a more severe reduction in the number of instruments. As we said earlier, this study is among the first to use a System-GMM estimator to assess the causal impact of finance on the growth of economic activity. This study had the merit of addressing the issue of instrument proliferation.

Nevertheless, after a meticulous work of reducing the number of instruments and extending the testing part, Roodman (2009a, p. 25) recast the results and concludes that “it is therefore hard to rule out reverse causation as the source of the [Levine, Loayza et Beck] panel results.” It definitely illustrates that even compliance with the rule of thumbs of fewer instruments than the number of cross-sectional observations is not enough to insure against the risk of spurious results.

9 Too Many Weak Instruments ?

So far, we have thoroughly addressed the undetected invalidity of the instrument set. Besides, another problem lurks below the surface. Valid instruments must meet two conditions. First, the instruments should affect growth only through the explanatory variable. This is the exogeneity condition :¹² $\mathbb{E}(Z'u) = 0$.

It was the primary source of concerns resulting from instrument proliferation. Secondly, the instruments must also correlate well with the variable of interest. This is the relevance condition : $\mathbb{E}(Z'X) \neq 0$.

Reliable causal inference of financial depth on growth requires the instruments to display a strong relationship with the endogenous explanatory variables. When this relationship is only weak, it is well known that instrumental variable estimators are severely biased (for a survey, see Murray, 2006 ; Mikusheva, 2013). Weak instruments also produce finite sample distributions that are non-normal. All things considered, if instruments are weak, then “point estimates, hypothesis tests, and confidence intervals are unreliable” (Stock, Wright et Yogo, 2002).

Studies focusing on causal inference on the finance-growth relationship have first used econometric methods that mobilize external instruments. “Legal origins” (Glaeser et Shleifer, 2002) have been widely used to instrument various finance proxies (Levine, Loayza et Beck, 2000). However, as Bazzi et Clemens (2013) pointed out, legal origins may have been too widely used to be safely considered as a reliable instrument. In addition to being an instrument for various finance proxies, legal origins are also an instrument for corruption, tax rates and over-regulation (Friedman et al., 2000), for the formalism degree of the legal procedure (Djankov et al., 2003), for inflation and the inequality of land ownership (Lundberg et Squire, 2003), or the relative size of the small and medium enterprise sector (Beck et al., 2008). Each one of these studies suggesting the instrument is invalid in any other specification.

Fortunately, the advances in econometric techniques, with the advent of dynamic pa-

12. The true error term is unobservable in applied settings. Therefore, any test of overidentifying restriction, which tests the coherency of the instrument set identifying the same vector of parameters, rest on the assumption that at least one instrument is valid (Parentes et Silva, 2012).

nel GMM estimators relying on internal instruments, have opened new perspectives of identification for causal inference. Because of its good performances relative well-known weak instruments problem in Difference-GMM, the System-GMM estimator became the preferred estimator in the estimation of growth models using country-level panel data (Blundell, Bond et Windmeijer, 2001). This estimator is, however, both a brilliant solution and a considerable pitfall, as it is far from being immune to the problem of weak instruments (Hayakawa, 2009; Bun et Windmeijer, 2010)

Figuring how much of the variation in the endogenous variables is explained by the internal instruments is a crucial aspect that often remains unexplored through applications of this econometric technique. Durlauf, Johnson et Temple (2005) emphasize that “many applications of instrumental variable procedures in the empirical growth literature [are] undermined by the failure to address properly the question of whether these instruments are valid.” Providing an answer to this question proves to be less straightforward in dynamic panel GMM regressions because a standardized test does not exist yet Stock et Yogo (2002). The question of instrument strength is often alleviated on the ground that it is less relevant with the System-GMM estimator constructed to deal with the limitation of the initial framework of Arellano et Bond (1991).

In light of the finance-growth literature, the observations and conclusions of Bazzi et Clemens (2013) are accurate : “in practice, most applications of System-GMM simply assume that instruments are strong. [...] Instrument strength is an empirical question that can and should be directly tested in dynamic panel growth regressions.” Two handy techniques enable such an investigation. The first one is easily implemented and provides a quick answer to the question of instrument validity. It is the minimalist R^2 tests. The second is less straight forward but implements a probabilistic lens to the question. It is the 2SLS analogs.

9.1 A Minimal Test of Instrument Strength

Table A.19 displays a simple test of weakness of instruments suggested by Blundell et Bond (2000). The test consists of running a simple regression of the change in variable X on the lagged level of X . The R^2 of this regression provides us with information

Table A.19 – Minimalist R^2 test of instrument strength in the System-GMM instrument setup.

Diff-GMM Instruments						Lev-GMM Instr.	
	L(2/2)	L(2/3)	L(2/4)	L(2/5)	L(2/6)		$\Delta.L(2)$
$\Delta \ln(\text{GDP})$	>0.01	0.01	0.02	0.03	0.04	$\ln(\text{GDP})$	0.05
ΔPC	>0.01	0.02	0.02	0.03	0.02	PC	0.21
$\Delta \ln(\text{Private credit})$	0.03	0.02	0.02	0.02	0.06	$\ln(\text{Private credit})$	0.06
$\Delta \text{Private credit}^2$	>0.01	0.01	0.01	0.01	>0.01	Private credit^2	0.30
$\Delta \ln(\text{gov. cons.})$	0.07	0.06	0.08	0.08	0.07	$\ln(\text{gov_cons})$	0.05
$\Delta \ln(\text{trade})$	0.03	0.02	0.02	0.01	0.02	$\ln(\text{trade})$	>0.01
$\Delta \ln(\text{inflation})$	0.04	0.03	0.02	0.03	0.06	$\ln(\text{inflation})$	0.03
$\Delta \ln(\text{school})$	0.29	0.42	0.46	0.48	0.44	$\ln(\text{school})$	0.06

Notes : Blundell et Bond (2000) suggest this simple test to asses the weakness of the instruments. For the Difference-GMM instruments, it consists in a regression of the first-differenced variable on their lagged level. The R^2 statistics provides simple information about the general fit of such instruments. In the same manner, for the Level-GMM instruments, I regress the variable in level with the lagged difference.

on the adequacy between the difference variables instrumented with lagged levels in the Difference-GMM. Likewise, for an extension to the System-GMM, this operation is repeated running a simple regression of the level of X on the once lagged difference of X . Thus the low R^2 for most of the variables used as instruments emphasize here an underlying problem of weak instruments. This question will be addressed in more detail in the following.

9.2 The 2SLS Analogs

The second technique is the “2SLS analogs” approach advanced by Bun et Windmeijer (2010) and Hayakawa (2009) and presented by Bazzi et Clemens (2013). By demonstrating that “the System-GMM estimator is clearly a combination of the GMM differenced estimator and a GMM levels estimator,” Blundell, Bond et Windmeijer (2001) provide the basis for this “2SLS analogs” technique since both System-GMM estimator and system 2SLS estimator follow similar settings. They are a combination of the differenced part and the levels part of the system. This combination being linear for the system 2SLS estimator :

$$\hat{\Omega}^{\text{sys}} = \delta \hat{\Omega}^{\text{dif}} + (1 - \delta) \hat{\Omega}^{\text{lev}} \quad (\text{A.54})$$

Table A.20 – Weak instruments in dynamic panel regressions, extending Table I.6.

Estimator	GMM-SYS	Difference equation		Levels equation	
		2SLS	2SLS	2SLS	2SLS
Collapsed IV matrix	NO	NO	YES	NO	YES
	(1)	(2)	(3)	(4)	(5)
Private Credit	3.628** (1.726)	-5.110** (2.161)	1.380 (4.020)	4.247** (2.028)	16.220 (121.16)
(Private Credit) ²	-2.021*** (1.726)	0.536 (0.825)	-2.278 (1.896)	-2.765*** (0.996)	-11.390 (81.01)
Log(init. GDP/cap.)	-0.728** (0.310)	-9.709*** (1.495)	-6.667*** (2.535)	-0.541 (0.396)	3.874 (32.100)
Log(school)	2.270*** (0.615)	-4.600** (1.914)	-1.548 (2.735)	2.539*** (0.817)	-5.528 (60.503)
Log(inflation)	-0.273 (0.210)	-0.392*** (0.129)	-0.510** (0.256)	-0.567** (0.267)	-0.737 (3.817)
Log(trade)	1.087** (0.511)	3.395*** (0.783)	3.657 (2.250)	-0.181 (0.673)	-17.273 (147.17)
Log(gov. cons.)	-1.461** (0.742)	-1.805** (0.851)	0.585 (1.826)	-2.053** (0.865)	-8.790 (40.162)
Observations	917	780	780	917	917
N. countries	133	130	130	133	133
N. instruments	318	261	57	65	16
IV : Lagged levels	YES	YES	YES	NO	NO
IV : Lagged differences	YES	NO	NO	YES	YES
Country FE	YES	YES	YES	NO	NO
Time FE	YES	YES	YES	YES	YES
Hansen test (<i>p</i> -value)		0.67	0.12	0.01	–
Kleibergen-Paap LM test (<i>p</i> -value)		0.286	0.465	0.518	0.894
Cragg-Donald Wald statistic		0.89	0.68	0.83	0.002
H_0 : <i>t</i> -test size > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
H_0 : <i>t</i> -test size > 25% (<i>p</i> -value)		1.000	1.000	1.000	1.000
H_0 : relative OLS bias > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
H_0 : relative OLS bias > 30% (<i>p</i> -value)		1.000	1.000	1.000	0.999
Kleibergen-Paap Wald statistic		3.17	0.85	1.15	0.002
H_0 : <i>t</i> -test size > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
H_0 : <i>t</i> -test size > 25% (<i>p</i> -value)		1.000	1.000	1.000	1.000
H_0 : relative OLS bias > 10% (<i>p</i> -value)		1.000	1.000	1.000	1.000
H_0 : relative OLS bias > 30% (<i>p</i> -value)		0.614	1.000	1.000	0.999

Notes : This table reports the results of a set of minimally arbitrary weak instrument test opening the “black box” of the System-GMM estimator. The panel regressions are based on ten non-overlapping five-year spells and contain time fixed effects. The dependent variable is the average real GDP per capita growth rate. While the first column simply reproduce the baseline result from the old dataset for 1960-2010 (see Table I.5 page 57, column (1)), the subsequent columns report the decomposition of the System-GMM following the “2SLS analogs” of Bazzi et Clemens (2013). Windmeijer standard errors in parentheses. This table is an extended version of Table I.6 page 61. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

where $\hat{\Omega}^{\text{dif}}$ and $\hat{\Omega}^{\text{lev}}$ are the 2SLS points estimates from the first-differenced and levels estimators respectively, and the weighting parameter is :

$$\delta = \frac{\hat{\pi}'_{\text{dif}} Z'_{\text{dif}} Z_{\text{dif}} \hat{\pi}_{\text{dif}}}{\hat{\pi}'_{\text{dif}} Z'_{\text{dif}} Z_{\text{dif}} \hat{\pi}_{\text{dif}} + \hat{\pi}'_{\text{lev}} Z'_{\text{lev}} Z_{\text{lev}} \hat{\pi}_{\text{lev}}} \quad (\text{A.55})$$

with $\hat{\pi}_{\text{dif}}$ and $\hat{\pi}_{\text{lev}}$ are the OLS estimates of the first stage regression coefficients using the instruments Z_{dif} and Z_{lev} in the levels and first-differenced equation, respectively. The system estimator is a weighted average of the difference and levels equation.

Since weak instrument tests are available within the 2SLS setup, carrying out the equivalent regression using 2SLS with the reconstructed GMM-type instrument matrix permits “simple and transparent tests of instruments’ strength in a closely related setting” (Bazzi et Clemens, 2013). Using this methodology, which extends the analytical setup advanced by Bun et Windmeijer (2010) to multiple endogenous variables, researchers can detect the (usual) presence of weak instruments in the Difference-GMM setup. It also enables us to examine whether the additional level moment conditions used in the System-GMM estimator are immune from the weak instrument problem. Table A.20 reports point estimates for this exercise along with various specification tests for the typical threshold from the old dataset.

Test for underidentification and weak instruments from Table A.20 are the following. For the underidentification the tables report the p -values for the Kleibergen et Paap (2006) heteroskedasticity robust version of the Lagrange-Multiplier (LM) test. The null hypothesis is that the structural equation is underidentified. A rejection of the null indicates that the smallest canonical correlation between the endogenous variables and the instruments is nonzero.

Since the nonzero correlation condition is not enough, Table A.20 controls for weak-instrument by reporting the weak-instruments Wald statistics based on Cragg et Donald (1993), and its non-*iid* robust analog by Kleibergen et Paap (2006). The latter is more appropriate than the former because most setup uses heteroskedasticity-robust standard errors—these tests assess whether the instruments jointly explain enough variation to identify unbiased causal effects.

These tests are complemented with the additional diagnostics proposed by Stock et Yogo (2002) and Yogo (2004). First, p -values for the null hypotheses that the bias in the estimates on the endogenous variable is greater than 10% or 30% of the OLS bias. Second, p -values for the null hypotheses that the actual size of the t -test that the coefficient estimates equal zero at the 5% significance level is greater than 15% or 25% the coefficient estimates. Critical values for the Kleibergen-Paap Wald statistic have not been tabulated as it depends on the specifics of the *iid* assumption's violation. Therefore, following Baum, Schaffer et Stillman (2007) and Bazzi et Clemens (2013), the critical values tabulated for the Cragg-Donald statistic are applied to the Kleibergen-Paap statistic. Stock et Yogo (2002) only provide critical values for up to 3 endogenous variables and 100 instruments. Nevertheless, as the number of instruments increases beyond 30 or 40, the rate of decay of the critical values is very slow. Also, Bazzi et Clemens underlines that the Stock et Yogo procedure is consistent for any number of instruments and "has good power, especially when the number of instruments is large."

These tests should be viewed as heuristic in the present setting. Nonetheless, they are informative and preferable to the alternative of simply assuming that the instruments are strong.

10 Damaging Impact of Finance on Growth

Table A.21 – Alternative estimates : the damaging impact of financial deepening, Difference-GMM estimates, 1 lag IV matrix.

Difference-GMM	(1)	(2)	(3)	(4)
Dataset	Old	Old	New	New
Period	1960-2010	1980-2010	1960-2015	1980-2015
Log(initial GDP/capita)	-2.051 (1.493)	-3.059 (2.297)	-2.718** (1.141)	-3.863** (1.646)
Private Credit	-4.904*** (1.607)	-6.890*** (2.247)	-3.818** (1.500)	-8.540*** (2.035)
Log(School)	-2.202* (1.166)	-2.538 (2.973)	-2.316** (1.019)	-3.923 (2.764)
Log(Government consumption)	-0.501 (1.065)	-0.760 (1.401)	-2.188* (1.245)	-1.742 (1.299)
Log(Trade openness)	4.101*** (1.079)	4.911* (2.890)	2.072 (1.321)	1.460 (1.262)
Log(Inflation rate)	-0.198 (0.236)	-0.303 (0.297)	-0.286 (0.272)	-0.220 (0.220)
Observations	784	527	436	658
N. Countries	130	130	95	136
N. Instruments	99	47	63	60
AR(2) test (<i>p</i> -value)	0.29	0.06	0.87	0.36
Hansen test (<i>p</i> -value)	0.39	0.23	0.53	0.10
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. Difference-GMM estimations with the instruments set restricted to one lag. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.22 – Alternative estimates : the damaging impact of financial deepening, Difference-GMM estimates, collapsed IV matrix.

Difference-GMM	(1)	(2)	(3)	(4)
Dataset	Old	Old	New	New
Period	1960-2010	1980-2010	1960-2015	1980-2015
Log(initial GDP/capita)	-3.539** (1.564)	-9.579*** (3.574)	-3.245* (1.969)	-5.178* (2.888)
Private Credit	-2.592 (1.704)	-8.361** (3.906)	-5.671** (2.860)	-7.035*** (2.456)
Log(School)	-0.745 (2.021)	-8.483 (5.510)	-2.339 (2.355)	-3.716 (4.268)
Log(Government consumption)	1.583 (1.393)	0.796 (2.070)	0.370 (2.373)	-1.245 (1.448)
Log(Trade openness)	2.800 (2.414)	6.191 (3.795)	1.563 (2.554)	-0.103 (1.659)
Log(Inflation rate)	-0.540 (0.330)	-0.591 (0.380)	-0.650 (0.465)	-0.378 (0.249)
Observations	784	527	436	658
N. Countries	130	130	95	136
N. Instruments	57	29	39	36
AR(2) test (<i>p</i> -value)	0.06	0.17	0.93	0.66
Hansen test (<i>p</i> -value)	0.14	0.01	0.09	0.01
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. Difference-GMM estimations with a collapsed instruments set. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.23 – Alternative estimates : the damaging impact of financial deepening, OLS-FE estimates.

OLS-FE	(1)	(2)	(3)	(4)
Dataset	Old	Old	New	New
Period	1960-2010	1980-2010	1960-2015	1980-2015
Log(initial GDP/capita)	-3.537*** (0.549)	-5.488*** (0.661)	-5.602*** (0.618)	-5.778*** (0.896)
Private Credit	-1.795*** (0.442)	-2.474*** (0.581)	-1.485*** (0.470)	-2.051*** (0.506)
Log(School)	-1.801** (0.703)	-1.613* (0.928)	-0.913 (0.995)	-1.060 (2.052)
Log(Government consumption)	-1.763*** (0.468)	-1.393*** (0.513)	0.079 (0.612)	1.471* (0.846)
Log(Trade openness)	2.426*** (0.517)	2.198*** (0.435)	1.037* (0.585)	0.073 (0.814)
Log(Inflation rate)	-0.201* (0.106)	-0.349*** (0.111)	-0.203* (0.111)	-0.145 (0.124)
Observations	917	660	824	637
N. Countries	133	133	140	140
R^2	0.271	0.321	0.282	0.258
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

11 Non-linearities & Institutions

Table A.24 – Summary Statistics.

PANEL 1960-2015 NEW SAMPLE	Obs.	Mean	σ	Min	Max
Growth	1,055	2.108	2.765	-16.84	14.28
Log(init.GDP/cap)	1,055	8.432	1.492	5.139	11.56
Private Credit	1,055	0.399	0.366	0.011	2.261
(Private Credit) ²	1,055	0.294	0.556	0.001	5.113
Log(school)	1,055	2.331	0.667	0.371	3.291
Log(inflation)	1,055	2.475	1.378	-2.065	10.06
Log(trade)	1,055	4.127	0.641	-1.743	6.047
Log(gov. cons.)	1,055	2.643	0.372	1.143	3.772
WORLDWIDE GOVERNANCE INDICATORS 1996-2015 (Kraay, Kaufmann et Mastruzzi, 2010)					
Worldwide Governance Index (6 dimensions)	254	0.440	5.457	-9.643	11.36
Voice and Accountability	254	0.066	0.950	-1.858	1.733
Political Stability and Abs. of Violence/Terrorism	254	-0.069	.931	-2.675	1.595
Government Effectiveness	254	0.137	0.993	-1.629	2.241
Regulatory Quality	254	0.180	0.928	-2.236	1.876
Rule of Law	254	0.073	1.000	-1.818	1.968
Control of Corruption	254	0.051	1.037	-1.387	2.358
ECONOMIC FREEDOM OF THE WORLD 1975-2015 (Gwartney, Lawson et Norton, 2008)					
Economic Freedom Index	694	6.305	1.308	1.99	9.07
Credit market regulation	733	7.241	2.219	0	10
Regulation	673	6.349	1.239	2.473	9.381
QUALITY OF GOVERNANCE 2000-2015 (Teorell et al., 2020)					
State Fragility Index	336	8	6.154	0	24
Functioning of Government	308	61.82	9.287	32.8	88.6
Rule of Law	308	54.61	18.91	10	90
Economic Freedom Index	308	55.84	19.34	0	95
SYSTEMIC BANKING CRISES 1960-2015 (Laeven et Valencia, 2018)					
Systemic Banking Crisis (100 events)	1,055	0.094	0.293	0	1
FINANCIAL LIBERALIZATION 1975-2010 (Abiad, Detragiache et Tressel, 2010)					
Financial Reform Index	410	0.585	0.286	0	1

Table A.25 – Institution interaction, Log(initial GDP/capita).

Estimator :	DIFF-GMM		OLS-FE		HPT-FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Period :	1960-2015	1980-2015	1960-2015	1980-2015	1960-2015	1980-2015
Log(initial GDP/capita)	-3.169*** (0.806)	-2.310* (1.206)	-3.922*** (0.397)	-5.399*** (0.708)	-4.135*** (0.618)	-5.569*** (0.472)
Private Credit	4.358 (5.827)	13.810 (9.053)	10.719*** (2.645)	12.298*** (3.339)		
Log(initial GDP/capita) × Private Credit	-0.790 (0.582)	-1.784** (0.860)	-1.184*** (0.267)	-1.375*** (0.325)		
Private Credit IF Log(initial GDP/capita) < τ					8.201*** (2.492)	0.595 (0.626)
Private Credit IF Log(initial GDP/capita) $\geq \tau$					-1.401*** (0.497)	-1.866*** (0.448)
Log(School)	-2.441*** (0.844)	0.621 (1.620)	-1.585** (0.636)	-1.113 (0.935)	2.143*** (0.754)	0.027 (0.640)
Log(Inflation rate)	-0.176 (0.178)	0.134 (0.245)	-0.123 (0.088)	-0.206** (0.099)	-0.233** (0.103)	-0.243*** (0.086)
Log(Trade openness)	1.132 (0.793)	1.633 (1.239)	1.507*** (0.419)	1.535*** (0.426)	0.817* (0.438)	1.586*** (0.403)
Log(Government consumption)	-0.915 (0.854)	0.134 (1.445)	-1.052** (0.429)	-0.755 (0.534)	-2.159*** (0.496)	-1.106*** (0.413)
Observations	915	658	1055	798	352	546
N. Countries	137	136	140	140	32	78
R^2	–	–	0.28	0.31	0.41	0.39
N. Instruments	128	68	–	–	–	–
AR(2) test (p -value)	0.653	0.069	–	–	–	–
Hansen test (p -value)	0.330	0.034	–	–	–	–
Threshold τ	5.51	7.74	9.05	8.95	7.31	9.95
Threshold 90% CI	[2.80-8.22]	[5.03-10.45]	[8.63-9.48]	[8.30-9.58]	[7.24-7.32]	[9.91-9.96]
Threshold-effect F -test (p -value)	–	–	–	–	0.06	0.01
Country FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

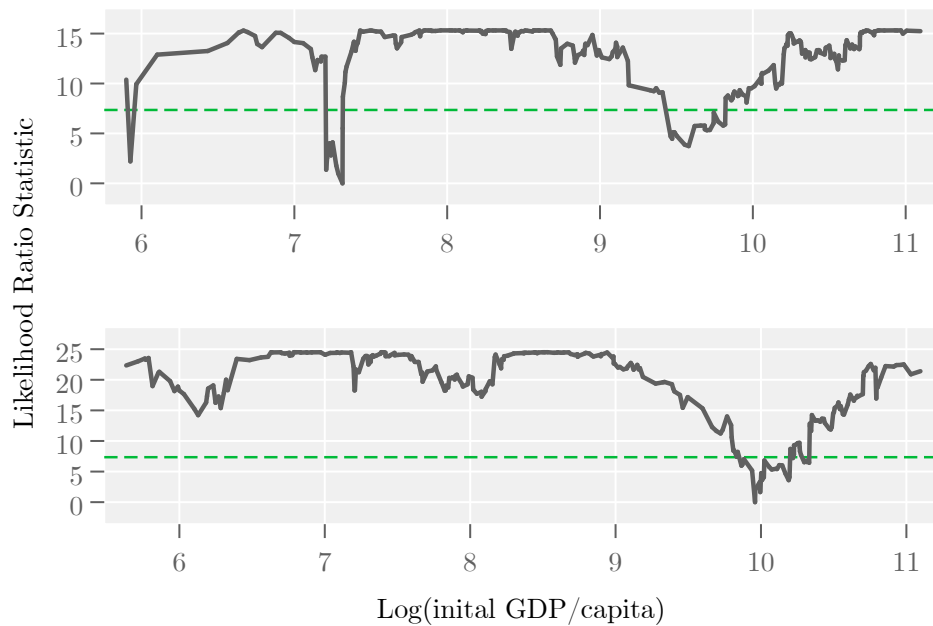


Figure A.5 – The LR statistic from Table A.25 columns (5) and (6) is constructed as $LR = \frac{[SSR(\tau) - SSR(\hat{\tau})]}{\hat{\sigma}^2}$, where $\hat{\tau} = \arg \min SSR(\tau)$; SSR is the sum of squared residuals obtained by estimating the model for different values of the threshold variable. The green dashed line denotes the critical value (7.35) at the 95% confidence level for the LR statistic. Note that the LR statistic seems to identify several possible thresholds for the regression displayed in Table A.25 column (5). For robustness, I check a multiple threshold regression based on the same setup. The single threshold model displayed in column (5) returns a F -stat of 25.07 with the associated p -value of 0.0075. In contrast, the double thresholds model returns a F -stat of 10.18 with a much higher p -value of 0.31. Therefore, I reported the single threshold estimates.

Table A.26 – Institution controls, Worldwide Governance Indicators from Kraay, Kaufmann et Mastruzzi (2010).

	1995-2015 NEW dataset DIFFERENCE-GMM							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(initial GDP/capita)	2.018 (3.913)	1.924 (5.181)	1.826 (4.004)	-0.457 (4.870)	2.296 (3.925)	-0.056 (4.779)	0.486 (3.242)	0.302 (5.269)
Private Credit	-5.130*** (1.962)	-4.341* (2.302)	-5.133** (2.015)	-3.323 (2.650)	-3.873* (1.988)	-4.308** (2.044)	-3.724* (1.975)	-2.207 (2.447)
Log(School)	0.130 (5.513)	-1.179 (5.791)	-0.458 (4.896)	0.070 (6.024)	-0.371 (5.691)	-0.206 (5.669)	2.102 (5.495)	-3.339 (6.955)
Log(Inflation rate)	0.930 (0.598)	0.861 (0.937)	0.826 (0.527)	1.115 (0.731)	0.897 (0.655)	1.300 (0.915)	0.681 (0.553)	1.642* (0.955)
Log(Trade openness)	2.434 (2.184)	3.314 (3.156)	2.850 (1.882)	3.809 (2.483)	2.439 (2.272)	4.044* (2.414)	0.628 (2.437)	4.492 (4.101)
Log(Government consumption)	9.259* (4.936)	8.173 (5.684)	9.128* (4.665)	9.466** (4.466)	7.517 (4.788)	9.770 (6.349)	6.576 (4.239)	11.315 (6.970)
Worldwide Governance Index (6 dimensions)		-0.148 (1.115)						
Voice and Accountability			-1.109 (2.411)					
Political Stability and Absence of Violence/Terrorism				1.793 (2.321)				
Government Effectiveness					-1.738 (4.303)			
Regulatory Quality						2.687 (3.115)		
Rule of Law							-4.029 (2.716)	
Control of Corruption								4.804 (4.782)
Observations	377	253	254	253	254	254	254	254
N. Countries	135	134	134	134	134	134	134	134
N. Instruments	20	20	20	20	20	20	20	20
AR(2) test <i>p</i> -value	0.156
Hansen test <i>p</i> -value	0.259	0.094	0.138	0.234	0.071	0.123	0.079	0.348

Notes : Set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The sample starts in 1995 due to data availability from the Worldwide Governance Indicators (Kraay, Kaufmann et Mastruzzi, 2010). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.27 – Institution interaction, Worldwide Governance Indicators from Kraay, Kaufmann et Mastruzzi (2010).

1995-2015 NEW dataset DIFFERENCE-GMM	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(initial GDP/capita)	2.018 (3.913)	2.931 (4.863)	1.281 (4.169)	1.325 (4.046)	0.878 (3.949)	1.975 (4.159)	0.724 (4.655)	2.845 (4.745)
Private Credit	-5.130*** (1.962)	-8.605 (6.070)	-4.931* (2.958)	-5.018** (2.024)	-2.974 (4.698)	-7.143 (4.483)	-4.283 (4.578)	-7.325* (4.064)
Log(School)	0.130 (5.513)	1.570 (5.409)	0.925 (5.481)	0.932 (5.877)	-0.020 (5.796)	1.055 (5.140)	0.876 (5.472)	0.658 (5.078)
Log(Inflation rate)	0.930 (0.598)	1.001* (0.576)	0.898 (0.605)	0.951 (0.585)	1.004 (0.641)	0.984* (0.590)	0.992* (0.600)	1.043* (0.536)
Log(Trade openness)	2.434 (2.184)	2.526 (2.441)	3.095 (2.260)	3.136 (2.217)	3.044 (2.636)	2.096 (2.395)	2.939 (2.571)	2.402 (2.373)
Log(Government consumption)	9.259* (4.936)	8.880* (4.935)	8.843* (4.583)	8.485* (4.329)	8.717* (4.637)	8.753* (4.974)	8.730* (4.825)	9.253* (4.949)
Private Credit × Worldwide Governance Index		0.626 (0.842)						
Private Credit × Voice and Accountability			0.707 (2.662)					
Private Credit × Political Stability				1.815 (1.781)				
Private Credit × Government Effectiveness					-0.987 (2.792)			
Private Credit × Regulatory Quality						2.012 (2.951)		
Private Credit × Rule of Law							0.188 (2.653)	
Private Credit × Control of Corruption								2.588 (2.950)
Observations	377	253	254	253	254	254	254	254
N. Countries	135	134	134	134	134	134	134	134
N. Instruments	20	20	20	20	20	20	20	20
AR(2) test <i>p</i> -value	0.156
Hansen test <i>p</i> -value	0.259	0.145	0.090	0.216	0.107	0.100	0.079	0.146

Notes : Set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.28 – Institution controls, Worldwide Governance Indicators from Kraay, Kaufmann et Mastruzzi (2010).

1995-2015 NEW dataset OLS-FE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(initial GDP/capita)	-4.614*** (0.805)	-6.132*** (1.193)	-6.612*** (1.114)	-7.169*** (1.192)	-5.603*** (1.183)	-5.710*** (1.153)	-6.154*** (1.210)	-6.449*** (1.126)
Private Credit	-2.606*** (0.515)	-1.912*** (0.647)	-1.893*** (0.646)	-1.754*** (0.664)	-1.832*** (0.657)	-1.675** (0.651)	-1.772*** (0.660)	-1.971*** (0.664)
Log(School)	-0.224 (2.515)	0.731 (3.091)	0.881 (3.078)	1.345 (2.834)	0.757 (3.116)	0.873 (3.026)	0.864 (2.996)	0.985 (3.106)
Log(Inflation rate)	-0.268** (0.123)	-0.108 (0.114)	-0.095 (0.117)	-0.099 (0.119)	-0.106 (0.112)	-0.116 (0.114)	-0.111 (0.115)	-0.113 (0.111)
Log(Trade openness)	0.958 (0.609)	1.523* (0.852)	1.420 (0.881)	1.338 (0.930)	1.535* (0.845)	1.463* (0.842)	1.535* (0.853)	1.442* (0.861)
Log(Government consumption)	0.325 (0.686)	0.559 (0.834)	0.671 (0.807)	0.631 (0.810)	0.408 (0.811)	0.540 (0.805)	0.461 (0.814)	0.616 (0.818)
Worldwide Governance Index		-0.189 (0.138)						
Voice and Accountability			-0.355 (0.583)					
Political Stability and Absence of Violence				0.536 (0.425)				
Government Effectiveness					-1.712** (0.657)			
Regulatory Quality						-1.587*** (0.481)		
Rule of Law							-1.351** (0.677)	
Control of Corruption								-0.664 (0.669)
Observations	517	392	393	392	393	393	393	393
N. Countries	140	139	139	139	139	139	139	139

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The sample starts in 1995 due to data availability from the Worldwide Governance Indicators (Kraay, Kaufmann et Mastruzzi, 2010). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.29 – Institution interaction, Worldwide Governance Indicators from Kraay, Kaufmann et Mastruzzi (2010).

1995-2015 NEW dataset OLS-FE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(initial GDP/capita)	-4.614*** (0.805)	-6.717*** (1.115)	-7.008*** (1.087)	-6.723*** (1.142)	-6.715*** (1.126)	-6.716*** (1.112)	-6.570*** (1.123)	-6.555*** (1.138)
Private Credit	-2.606*** (0.515)	-1.833* (1.090)	-1.158 (0.902)	-1.976*** (0.728)	-1.695 (1.088)	-1.691* (0.996)	-2.211** (1.090)	-2.163** (0.969)
Log(School)	-0.224 (2.515)	0.933 (3.124)	0.682 (3.108)	1.051 (3.147)	0.926 (3.093)	0.918 (3.101)	1.047 (3.136)	1.003 (3.101)
Log(Inflation rate)	-0.268** (0.123)	-0.099 (0.116)	-0.110 (0.115)	-0.096 (0.116)	-0.096 (0.116)	-0.098 (0.115)	-0.098 (0.117)	-0.096 (0.117)
Log(Trade openness)	0.958 (0.609)	1.385 (0.920)	1.428 (0.911)	1.361 (0.923)	1.358 (0.911)	1.357 (0.907)	1.317 (0.911)	1.320 (0.908)
Log(Government consumption)	0.325 (0.686)	0.604 (0.832)	0.604 (0.829)	0.651 (0.827)	0.606 (0.829)	0.611 (0.826)	0.662 (0.820)	0.660 (0.820)
Private Credit × Worldwide Governance Index								
Private Credit × Voice and Accountability			-0.918* (0.549)					
Private Credit × Political Stability				0.254 (0.414)				
Private Credit × Government Effectiveness					-0.169 (0.554)			
Private Credit × Regulatory Quality								
Private Credit × Rule of Law							0.313 (0.594)	
Private Credit × Control of Corruption								0.304 (0.485)
Observations	517	392	393	392	393	393	393	393
N. Countries	140	139	139	139	139	139	139	139

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The sample starts in 1995 due to data availability from the Worldwide Governance Indicators (Kraay, Kaufmann et Mastruzzi, 2010). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.30 – Institution controls, from the Economic Freedom of the World database Gwartney, Lawson et Norton (2008).

Estimator :	DIFF-GMM				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1960-2015 NEW dataset								
Log(initial GDP/capita)	-2.780*** (0.784)	-3.482*** (1.010)	-3.149*** (0.890)	-3.437*** (1.016)	-4.116*** (0.406)	-4.562*** (0.484)	-4.365*** (0.463)	-4.300*** (0.459)
Private Credit	-4.423*** (1.075)	-2.434*** (0.857)	-3.615*** (1.068)	-3.230*** (1.041)	-1.108*** (0.408)	-1.149*** (0.403)	-1.166*** (0.415)	-1.349*** (0.408)
Log(School)	-2.253*** (0.815)	-1.010 (0.842)	-1.395 (0.888)	-0.946 (1.312)	-0.932 (0.660)	-0.307 (0.632)	-0.945 (0.750)	-0.108 (0.682)
Log(Inflation rate)	-0.202 (0.172)	0.123 (0.229)	-0.252 (0.187)	-0.143 (0.191)	-0.116 (0.090)	-0.109 (0.098)	-0.168 (0.113)	-0.211** (0.103)
Log(Trade openness)	1.286 (1.134)	1.085 (0.942)	1.311* (0.776)	0.936 (0.892)	1.692*** (0.432)	1.662*** (0.492)	1.936*** (0.506)	2.005*** (0.495)
Log(Government consumption)	-1.513* (0.842)	0.451 (0.834)	-0.640 (0.971)	0.561 (1.036)	-0.978** (0.428)	-0.928** (0.414)	-0.578 (0.545)	-0.768 (0.487)
Economic Freedom Index		0.978*** (0.317)				0.678*** (0.143)		
Credit market regulations			0.055 (0.126)				0.019 (0.066)	
Regulation				0.537* (0.278)				0.228* (0.124)
Observations	915	696	734	673	1055	827	865	804
N. Countries	137	120	120	120	140	131	131	131
N. Instruments	111	116	116	116	–	–	–	–
AR(2) test <i>p</i> -value	0.796	0.800	0.811	0.301	–	–	–	–
Hansen test <i>p</i> -value	0.294	0.177	0.255	0.258	–	–	–	–

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.31 – Institution interaction, from the Economic Freedom of the World database Gwartney, Lawson et Norton (2008).

Estimator :	DIF-F-GMM				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1960-2015 NEW dataset								
Log(initial GDP/capita)	-2.780*** (0.784)	-2.627*** (0.895)	-2.342** (1.025)	-2.363** (1.115)	-4.116*** (0.406)	-4.229*** (0.461)	-4.306*** (0.462)	-4.185*** (0.453)
Private Credit	-4.423*** (1.075)	1.633 (4.079)	1.585 (2.606)	0.692 (3.821)	-1.108*** (0.408)	-1.411 (1.612)	0.817 (1.096)	-0.690 (1.619)
Log(School)	-2.253*** (0.815)	-1.916* (1.129)	-2.098** (0.949)	-1.439 (1.282)	-0.932 (0.660)	-0.101 (0.628)	-1.049 (0.765)	-0.139 (0.702)
Log(Inflation rate)	-0.202 (0.172)	-0.338 (0.211)	-0.232 (0.188)	-0.282 (0.177)	-0.116 (0.090)	-0.286*** (0.094)	-0.186* (0.103)	-0.253** (0.099)
Log(Trade openness)	1.286 (1.134)	1.941** (0.789)	1.377 (0.850)	1.186 (0.838)	1.692*** (0.432)	1.854*** (0.498)	1.934*** (0.488)	2.055*** (0.489)
Log(Government consumption)	-1.513* (0.842)	-0.713 (0.951)	-0.970 (0.861)	-0.141 (0.961)	-0.978** (0.428)	-1.098** (0.421)	-0.647 (0.539)	-0.867* (0.475)
Private Credit		-0.757 (0.537)				0.038 (0.213)		
× Economic Freedom Index			-0.711** (0.309)				-0.243** (0.121)	
Private Credit								
× Credit market regulation								
Private Credit				-0.567 (0.463)				
× Regulation								-0.081 (0.195)
Observations	915	696	734	673	1055	827	865	804
N. Countries	137	120	120	120	140	131	131	131
N. Instruments	111	116	116	116				
AR(2) test <i>p</i> -value	0.796	0.799	0.834	0.332	–	–	–	–
Hansen test <i>p</i> -value	0.294	0.365	0.252	0.229	–	–	–	–

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.32 – Institution controls, from the Quality of Governance database Teorell et al. (2020).

Estimator :	DIFF-GMM					OLS-FE				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1960-2015 NEW dataset										
Log(initial GDP/capita)	-2.780*** (0.784)	-1.540 (1.818)	0.771 (1.863)	1.562 (2.038)	0.748 (1.787)	-4.116*** (0.406)	-5.191*** (0.844)	-5.369*** (1.117)	-5.063*** (1.002)	-5.242*** (1.010)
Private Credit	-4.423*** (1.075)	-4.334*** (1.842)	-6.731*** (1.470)	-6.404*** (1.653)	-6.142*** (1.789)	-1.108*** (0.408)	-2.405*** (0.571)	-2.711*** (0.617)	-2.341*** (0.582)	-2.486*** (0.650)
Log(School)	-2.253*** (0.815)	2.991 (2.363)	3.995*** (1.846)	2.778 (1.928)	4.732*** (1.823)	-0.932 (0.660)	1.795 (1.142)	2.741** (1.159)	2.472** (1.161)	2.753** (1.153)
Log(Inflation rate)	-0.202 (0.172)	0.136 (0.251)	0.447* (0.246)	0.352 (0.261)	0.550** (0.218)	-0.116 (0.090)	-0.276** (0.137)	-0.173 (0.138)	-0.189 (0.138)	-0.177 (0.138)
Log(Trade openness)	1.286 (1.134)	-0.101 (1.198)	0.036 (1.617)	0.791 (1.625)	0.164 (1.452)	1.692*** (0.432)	0.774 (0.557)	1.005 (0.650)	1.107* (0.615)	1.119* (0.635)
Log(Government consumption)	-1.513* (0.842)	-0.688 (1.458)	1.455 (1.912)	1.096 (2.632)	1.741 (1.941)	-0.978** (0.428)	-0.191 (0.638)	-0.223 (0.701)	-0.286 (0.776)	-0.252 (0.758)
State Fragility Index		-0.362* (0.214)					-0.177** (0.082)			
Economic Freedom Index			0.014 (0.089)					0.022 (0.033)		
Financial Freedom				-0.036 (0.024)					-0.019* (0.010)	
Investment Freedom					-0.015 (0.026)					-0.004 (0.009)
Observations	915	340	312	312	312	1055	472	447	447	447
N. Countries	137	125	126	126	126	140	132	135	135	135
N. Instruments	111	41	41	41	41	-	-	-	-	-
AR(2) test <i>p</i> -value	0.796	0.963	0.369	0.410	0.296	-	-	-	-	-
Hansen test <i>p</i> -value	0.294	0.056	0.058	0.089	0.102	-	-	-	-	-

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.33 – Institution interactions, from the Quality of Governance database Teorell et al. (2020).

Estimator :	DIFF-GMM					OLS-FE				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1960-2015 NEW dataset										
Log(initial GDP/capita)	-2.780*** (0.784)	0.118 (1.622)	0.424 (2.059)	0.270 (2.027)	0.618 (1.989)	-4.116*** (0.406)	-4.732*** (0.828)	-5.318*** (0.997)	-5.453*** (0.966)	-5.370*** (0.961)
Private Credit	-4.423*** (1.075)	-6.368*** (1.985)	-5.610 (8.020)	-3.336 (4.083)	-6.448 (4.589)	-1.108*** (0.408)	-2.596*** (0.562)	-0.906 (2.913)	-0.647 (1.260)	-1.760 (1.326)
Log(School)	-2.253*** (0.815)	5.463** (2.195)	4.094** (1.884)	3.161 (2.188)	3.667* (2.109)	-0.932 (0.660)	2.506** (1.115)	2.688** (1.169)	2.606** (1.163)	2.711** (1.149)
Log(Inflation rate)	-0.202 (0.172)	0.081 (0.308)	0.557** (0.268)	0.418 (0.277)	0.519* (0.271)	-0.116 (0.090)	-0.261* (0.136)	-0.183 (0.137)	-0.183 (0.136)	-0.183 (0.137)
Log(Trade openness)	1.286 (1.134)	-0.038 (1.245)	0.811 (1.573)	0.519 (1.665)	1.108 (1.734)	1.692*** (0.432)	0.938* (0.559)	1.091* (0.632)	0.995 (0.616)	1.116* (0.638)
Log(Government consumption)	-1.513* (0.842)	-0.299 (1.932)	1.532 (2.187)	1.288 (2.124)	1.803 (2.806)	-0.978** (0.428)	-0.108 (0.688)	-0.286 (0.752)	-0.298 (0.742)	-0.243 (0.743)
Private Credit × State Fragility Index		-0.077 (0.384)					0.038 (0.117)			
Private Credit × Economic Freedom Index			-0.007 (0.104)					-0.023 (0.036)		
Private Credit × Financial Freedom				-0.039 (0.048)					-0.025* (0.014)	
Private Credit × Investment Freedom					0.000 (0.055)					-0.010 (0.013)
Observations	915	340	312	312	312	1055	472	447	447	447
N. Countries	137	125	126	126	126	140	132	135	135	135
N. Instruments	111	41	41	41	41	-	-	-	-	-
AR(2) test p -value	0.796	0.514	0.325	0.320	0.422	-	-	-	-	-
Hansen test p -value	0.294	0.032	0.083	0.056	0.100	-	-	-	-	-

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.34 – Financial development growth effects with regions.

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1) Old	(2) Old	(3) New	(4) New	(5) Old	(6) Old	(7) New	(8) New
Log(initial GDP/capita)	-2.828*** (1.032)	-3.747** (1.508)	-4.006*** (0.967)	-5.791*** (1.434)	-3.648*** (0.545)	-5.809*** (0.662)	-7.143*** (0.888)	-7.554*** (1.044)
Private Credit	-4.941*** (1.259)	-7.872*** (2.016)	-5.542*** (1.206)	-8.394*** (2.081)	-2.280*** (0.477)	-2.894*** (0.692)	-2.125*** (0.670)	-3.132*** (0.756)
Private Credit × Medium-high income countries	1.807 (2.311)	-0.274 (5.094)	1.733 (2.064)	2.810 (2.472)	1.613** (0.794)	0.829 (1.029)	2.495** (0.978)	1.726 (1.853)
Private Credit × Medium-low & Low income countries	12.515** (5.731)	15.961*** (6.130)	3.037 (3.374)	6.896 (4.756)	4.169*** (1.528)	5.375*** (1.292)	5.868*** (1.745)	6.388*** (2.419)
Log(School)	-3.343** (1.412)	-2.285 (2.053)	-3.958*** (1.134)	-5.573** (2.236)	-2.252*** (0.665)	-2.069** (0.933)	-1.104 (0.899)	0.840 (1.008)
Log(Government consumption)	-0.910 (0.996)	-0.060 (1.046)	-1.293 (0.891)	-1.139 (1.092)	-1.853*** (0.471)	-1.562*** (0.505)	-0.988* (0.556)	-0.482 (0.614)
Log(Trade openness)	3.435*** (1.131)	3.166 (1.946)	1.072 (0.939)	2.476** (1.201)	2.303*** (0.503)	2.057*** (0.423)	1.654*** (0.414)	1.231** (0.549)
Log(Inflation rate)	-0.306 (0.187)	-0.326 (0.278)	-0.215 (0.179)	-0.194 (0.245)	-0.214** (0.102)	-0.325*** (0.108)	-0.261** (0.109)	-0.203** (0.101)
Observations	784	527	915	658	917	660	666	487
N. Countries	130	130	137	136	133	133	136	135
R ²	–	–	–	–	0.28	0.35	0.33	0.32
N. Instruments	129	61	132	78				
AR(2) test (<i>p</i> -value)	0.264	0.078	0.864	0.195				
Hansen test (<i>p</i> -value)	0.262	0.693	0.511	0.076				

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. While the first four columns present Difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.35 – Financial development growth effects controlling for banking crises (Laeven et Valencia, 2018).

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Old 1960-2010	Old 1980-2010	New 1960-2015	New 1980-2015	Old 1960-2010	Old 1980-2010	New 1980-2015	New 1990-2015
Log(initial GDP/capita)	-2.384* (1.217)	-3.524 (2.336)	-3.431*** (0.858)	-3.161** (1.524)	-3.688*** (0.554)	-5.263*** (0.663)	-6.482*** (0.896)	-6.887*** (1.089)
Private Credit	-2.874** (1.182)	-3.049* (1.586)	-4.764*** (1.121)	-5.253*** (1.972)	-1.355*** (0.433)	-1.870*** (0.591)	-1.678*** (0.525)	-1.851*** (0.689)
Systemic Banking Crisis	-1.906*** (0.651)	-1.935** (0.882)	-2.534*** (0.485)	-1.917*** (0.726)	-1.232*** (0.238)	-1.201*** (0.273)	-1.285*** (0.258)	-0.832*** (0.257)
Log(School)	-1.920 (1.187)	-1.009 (3.148)	-4.100*** (1.022)	-2.241 (2.855)	-1.882*** (0.662)	-1.624* (0.892)	-0.656 (0.869)	0.965 (1.068)
Log(Government consumption)	-0.951 (0.945)	-0.745 (1.196)	-2.146*** (0.802)	-1.946* (1.069)	-1.620*** (0.507)	-1.412*** (0.506)	-0.934 (0.571)	-0.420 (0.661)
Log(Trade openness)	3.481*** (1.117)	3.505 (2.395)	1.373 (0.979)	0.374 (1.171)	2.417*** (0.560)	1.844*** (0.443)	1.455*** (0.412)	1.022* (0.540)
Log(Inflation rate)	-0.238 (0.229)	-0.262 (0.344)	-0.138 (0.146)	-0.204 (0.190)	-0.152 (0.106)	-0.318*** (0.109)	-0.258** (0.106)	-0.225** (0.108)
Observations	739	527	915	658	872	660	666	487
N. Countries	130	130	137	136	133	133	136	135
R ²	–	–	–	–	0.29	0.35	0.37	0.32
N. Instruments	107	54	122	69	–	–	–	–
AR(2) test <i>p</i> -value	0.301	0.173	0.753	0.200	–	–	–	–
Hansen test <i>p</i> -value	0.441	0.060	0.504	0.062	–	–	–	–

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. While the first four columns present Difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.36 – Financial development growth effects controlling for financial liberalization (Abiad, Detragiache et Tresselt, 2010).

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1) Old 1960-2010	(2) Old 1980-2010	(3) New 1960-2015	(4) New 1980-2015	(5) Old 1960-2010	(6) Old 1980-2010	(7) New 1980-2015	(8) New 1990-2015
Log(initial GDP/capita)	-2.935 (2.094)	-0.852 (2.751)	-4.060*** (1.414)	-3.321 (2.766)	-4.035*** (0.771)	-4.901*** (0.796)	-5.425*** (0.790)	-6.426*** (1.438)
Private Credit	-2.941** (1.445)	-8.030*** (2.824)	-6.823*** (1.816)	-8.057*** (2.834)	-1.564** (0.598)	-2.285*** (0.641)	-1.877*** (0.530)	-2.780*** (0.661)
Financial Reform Index	4.182** (1.769)	3.923 (2.994)	1.682 (1.795)	0.609 (2.350)	2.869*** (0.956)	2.109* (1.078)	2.036** (0.872)	3.178*** (0.974)
Log(School)	-2.035* (1.155)	-6.561* (3.584)	-3.479*** (1.622)	-2.210 (3.410)	-1.519 (0.930)	-1.865** (0.931)	0.100 (0.814)	0.263 (1.136)
Log(Government consumption)	-1.947* (1.166)	-2.814** (1.163)	-1.592 (1.165)	-2.122* (1.185)	-2.190*** (0.701)	-2.736*** (0.682)	-1.579*** (0.461)	-1.223* (0.640)
Log(Trade openness)	3.225*** (1.046)	0.956 (1.297)	1.489 (1.589)	1.092 (1.492)	1.987*** (0.586)	2.447*** (0.465)	1.599*** (0.535)	1.601** (0.749)
Log(Inflation rate)	0.001 (0.221)	-0.373 (0.447)	-0.087 (0.230)	-0.158 (0.260)	-0.141 (0.144)	-0.435*** (0.149)	-0.270* (0.149)	-0.191 (0.145)
Observations	319	263	410	350	400	344	431	308
N. Countries	79	79	81	81	81	81	81	81
R ²	–	–	–	–	0.32	0.40	0.35	0.33
N. Instruments	72	39	87	54	–	–	–	–
AR(2) test <i>p</i> -value	0.230	0.114	0.549	0.200	–	–	–	–
Hansen test <i>p</i> -value	0.249	0.159	0.380	0.052	–	–	–	–

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. While the first four columns present Difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.37 – Institution interactions, from the Financial Reform database Abiad, Detragiache et Tressel (2010).

Dataset Period	GMM-DIFFERENCE				OLS-FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Old 1960-2010	Old 1980-2010	New 1960-2015	New 1980-2015	Old 1960-2010	Old 1980-2010	New 1980-2015	New 1990-2015
Log(initial GDP/capita)	-2.839* (1.506)	-0.886 (2.967)	-4.058*** (1.350)	-3.593 (2.626)	-4.030*** (0.774)	-4.907*** (0.799)	-5.439*** (0.801)	-6.418*** (1.467)
Private Credit	-1.910 (1.758)	-9.405** (3.872)	-3.308 (3.535)	-5.115 (4.461)	-0.897 (1.017)	-1.884 (1.145)	0.469 (1.200)	-2.934 (1.859)
Financial Reform Index	3.708*** (1.274)	2.767 (3.024)	1.985 (1.641)	0.780 (2.468)	3.067*** (0.970)	2.221** (1.075)	2.483*** (0.890)	3.158*** (0.992)
Private Credit × Financial Reform Index	-1.567 (1.988)	2.080 (4.061)	-3.473 (3.177)	-0.556 (4.074)	-0.853 (1.085)	-0.488 (1.252)	-2.735* (1.400)	0.174 (1.926)
Log(School)	-2.673** (1.067)	-5.583* (3.349)	-3.928** (1.601)	-0.380 (2.872)	-1.695* (0.931)	-1.944** (0.950)	-0.307 (0.859)	0.277 (1.112)
Log(Government consumption)	-2.085** (0.913)	-2.910*** (1.107)	-1.608 (1.260)	-1.805* (1.074)	-2.187*** (0.701)	-2.740*** (0.681)	-1.565*** (0.465)	-1.221* (0.641)
Log(Trade openness)	2.746*** (0.934)	1.202 (1.471)	0.759 (1.402)	2.127 (1.529)	1.937*** (0.594)	2.418*** (0.477)	1.442*** (0.534)	1.604** (0.755)
Log(Inflation rate)	-0.101 (0.215)	-0.462 (0.463)	-0.023 (0.211)	-0.192 (0.262)	-0.138 (0.145)	-0.429*** (0.149)	-0.256* (0.153)	-0.192 (0.146)
Observations	319	263	410	350	400	344	431	308
N. Countries	79	79	81	81	81	81	81	81
R ²	–	–	–	–	0.32	0.40	0.35	0.33
N. Instruments	79	44	96	61	–	–	–	–
AR(2) test <i>p</i> -value	0.261	0.093	0.489	0.114	–	–	–	–
Hansen test <i>p</i> -value	0.347	0.372	0.780	0.068	–	–	–	–

Notes : This table reports the results of a set of dynamic panel estimations in which the dependent variable is the average real GDP per capita growth rate. All regressions contain time and country fixed effects. While the first four columns present Difference-GMM estimations with the instruments set restricted to one lag, the subsequent columns report OLS fixed effect regressions. The null hypothesis of the AR(2) serial correlation test is that the errors in the first difference regression exhibit no second-order serial correlation. The null hypothesis of the Hansen test is that the instruments fail to identify the same vector of parameters (see Parentes et Silva, 2012). Robust Windmeijer standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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1 Data Appendix

Table B.1 – Data description and sources .

CORE VARIABLES	
Credit to the non-financial sector	Bank for International Settlement (BIS), F1
Credit to the private non-financial sector	Bank for International Settlement (BIS), F2
Credit to households sector	Bank for International Settlement (BIS), F3
Credit to non-financial corporation	Bank for International Settlement (BIS), F4
Credit to the government sector	Bank for International Settlement (BIS), F5 & Historical Public Debt Database (IMF)
CONTROL VARIABLES	
GDP per capita, constant 2010 USD	World Bank, (WDI)
Inflation of consumer prices, annual %	World Bank, (WDI)
Human capital index	Penn World Table (PENN 9.0)
Trade openness $\frac{X+M}{Y}$	World Bank, (WDI)
General government expenditure	World Bank, (WDI)
Dependency $\frac{(<15 \text{ \& } 65+)}{(15-64)}$	United Nations World Population Prospects
Gross national savings	World Economic Outlook Database (IMF)
Banking Crisis	Systemic Banking Crisis Database (IMF)

Notes : all samples were extracted in June 2019.

Table B.2 – List of countries by sub-groups.

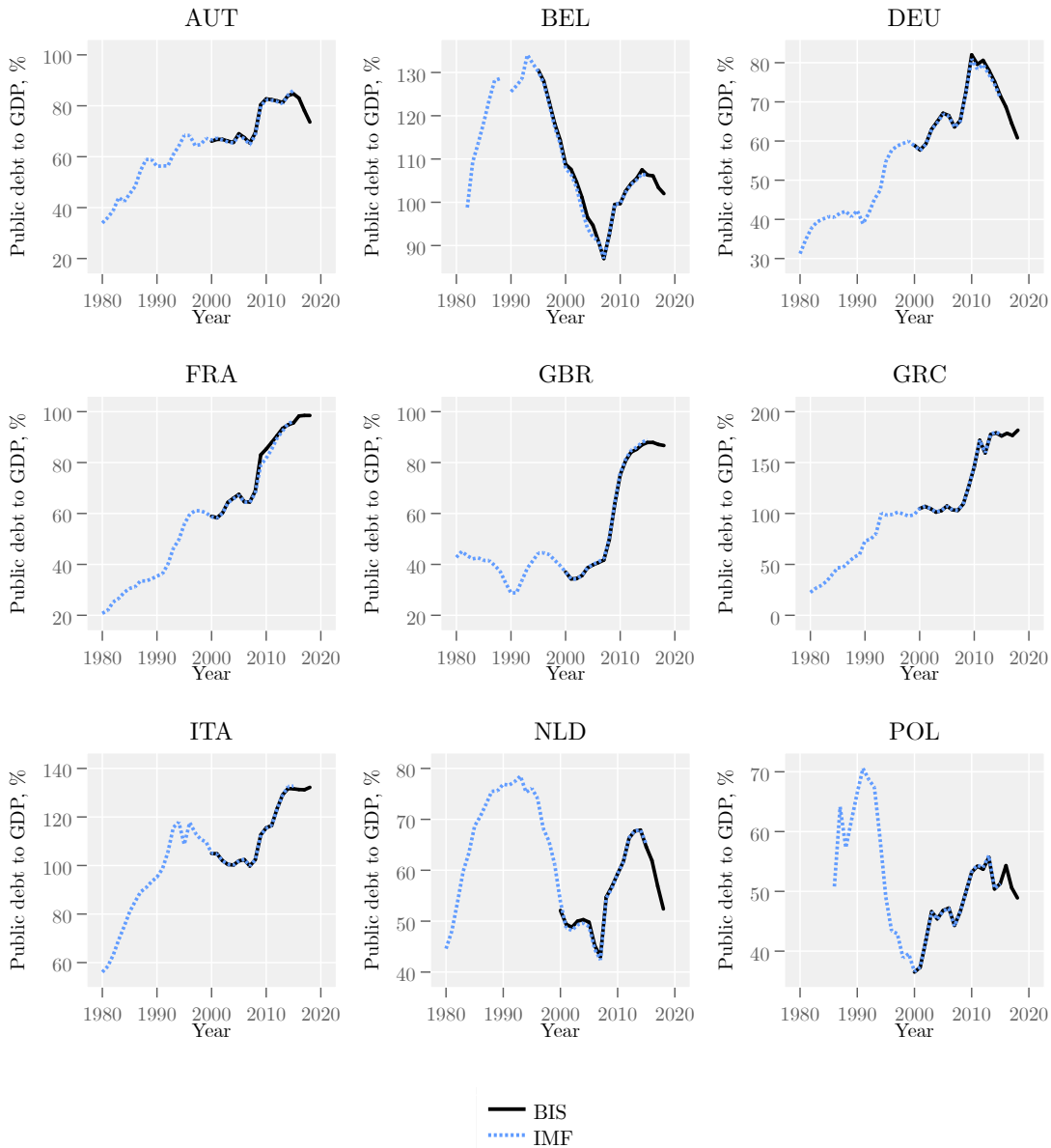
advanced economies : Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States.

non-advanced economies : Brazil, Chile, Colombia, Czech Republic, Hungary, India, Indonesia, Israel, Korea, Malaysia, Mexico, Poland, South Africa, Thailand, Turkey.

european union : Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Italy, Netherlands, Poland, Portugal, Spain, Sweden, United Kingdom.

eurozone : Austria, Belgium, Finland, France, Germany, Greece, Italy, Netherlands, Portugal, Spain.

Figure B.1 – Expanding the BIS public debt variable with the Historical Public Debt Database (IMF).



Note : Selected sample of countries for which the public debt series from the BIS credit database have been expanded with IMF's Historical Public Debt Database : Austria (AUT) Belgium (BEL), Germany (DEU), Denmark (DNK), Spain (ESP), Finland (FIN), France (FRA), United-Kingdom (GBR), Greece (GRC), Italy (ITA), Netherlands (NLD) and Poland (POL).

Table B.3 – Summary statistics for the credit and debt variables over various sub-groups of countries.

	Obs.	Mean	Std. Dev.	Min	Max
All sample					
Total debt/GDP	1,026	1.69	0.64	0.35	3.99
Public debt/GDP	1,026	0.56	0.31	0.04	2.38
Private credit/GDP	1,026	1.13	0.53	0.16	2.80
Firm credit/GDP	798	0.75	0.31	0.13	1.61
Household credit/GDP	798	0.47	0.27	0.01	1.39
Advanced economies					
Total debt/GDP	594	1.97	0.57	0.68	3.99
Public debt/GDP	594	0.63	0.33	0.08	2.38
Private credit/GDP	594	1.34	0.46	0.35	2.80
Firm credit/GDP	541	0.83	0.28	0.28	1.61
Household credit/GDP	541	0.55	0.24	0.05	1.39
Non-advanced economies					
Total debt/GDP	432	1.30	0.51	0.35	2.72
Public debt/GDP	432	0.46	0.25	0.04	1.66
Private credit/GDP	432	0.84	0.48	0.16	2.25
Firm credit/GDP	257	0.56	0.27	0.13	1.32
Household credit/GDP	257	0.30	0.26	0.01	1.17
European Union members					
Total debt/GDP	490	1.81	0.57	0.68	3.58
Public debt/GDP	490	0.61	0.27	0.09	1.72
Private credit/GDP	490	1.19	0.51	0.22	2.80
Firm credit/GDP	434	0.77	0.30	0.20	1.62
Household credit/GDP	434	0.46	0.27	0.02	1.39
Eurozone members					
Total debt/GDP	330	1.87	0.55	0.68	3.58
Public debt/GDP	330	0.67	0.30	0.09	1.72
Private credit/GDP	330	1.21	0.48	0.35	2.80
Firm credit/GDP	291	0.82	0.29	0.30	1.62
Household credit/GDP	291	0.45	0.23	0.05	1.19

Table B.4 – Correlations matrix.

	OVERALL	GROWTH5	PUBLIC	PRIVATE	TOTAL	FIRM	HOUSE	GDPpc	GOVCONS	INFL	TRADE	SCHOOL	DEPEN	SAVING	CRISIS
PUBLIC	-0.244***	1													
PRIVATE	-0.292***	0.0373	1												
TOTAL	-0.348***	0.465***	0.877***	1											
FIRM	-0.255***	0.0814*	0.927***	0.836***	1										
HOUSE	-0.339***	0.0662	0.881***	0.769***	0.683***	1									
GDPpc	-0.513***	0.237***	0.694***	0.685***	0.591***	0.621***	1								
GOVCONS	-0.339***	0.359***	0.396***	0.449***	0.449***	0.362***	0.609***	1							
INFL	0.0394	-0.118***	-0.638***	-0.633***	-0.533***	-0.533***	-0.407***	-0.240***	1						
TRADE	-0.0463	0.0611*	0.348***	0.291***	0.291***	0.407***	0.255***	0.235***	-0.298***	1					
SCHOOL	-0.264***	0.137***	0.638***	0.598***	0.407***	0.575***	0.779***	0.570***	-0.495***	0.372***	1				
DEPEN	0.0182	0.0406	-0.488***	-0.423***	-0.240***	-0.362***	-0.497***	-0.263***	0.504***	-0.279***	-0.618***	1			
SAVING	0.265***	-0.294***	0.255***	0.0904**	0.296***	0.0979**	0.0209	-0.222***	-0.264***	0.275***	0.0855**	-0.256***	1		
CRISIS	-0.214***	-0.0465	0.124***	0.0929**	0.143***	0.138***	-0.00815	-0.0344	0.118***	-0.0790**	-0.0658*	0.0727*	0.0506	1	
	BETWEEN	GROWTH5	PUBLIC	PRIVATE	TOTAL	FIRM	HOUSE	GDPpc	GOVCONS	INFL	TRADE	SCHOOL	DEPEN	SAVING	CRISIS
PUBLIC	-0.549***	1													
PRIVATE	-0.205	0.0580	1												
TOTAL	-0.365*	0.452**	0.905***	1											
FIRM	0.00675	0.0749	0.888***	0.842***	1										
HOUSE	-0.299	0.0809	0.894***	0.808***	0.684***	1									
GDPpc	-0.668***	0.292	0.717***	0.740***	0.531**	0.617***	1								
GOVCONS	-0.470**	0.388*	0.461**	0.528**	0.448**	0.373*	0.652***	1							
INFL	0.0550	-0.0537	-0.803***	-0.727***	-0.729***	-0.757***	-0.494**	-0.263	1						
TRADE	-0.0208	0.0100	0.327	0.260	0.280	0.279	0.212	0.283	-0.323	1					
SCHOOL	-0.314	0.0895	0.643***	0.588***	0.505**	0.550***	0.781***	0.634***	-0.520**	0.330	1				
DEPEN	-0.0889	0.144	-0.522**	-0.421*	-0.542***	-0.354*	-0.468**	-0.300	0.565***	-0.244	-0.586***	1			
SAVING	0.494**	-0.340*	0.391*	0.239	0.459**	0.257	-0.0591	-0.203	-0.539**	0.278	0.0251	-0.277	1		
CRISIS	0.0865	0.0715	-0.00459	0.0332	-0.00449	-0.0346	-0.0508	-0.174	0.0590	-0.307	-0.309	0.148	0.179	1	
	WITHIN	GROWTH5	PUBLIC	PRIVATE	TOTAL	FIRM	HOUSE	GDPpc	GOVCONS	INFL	TRADE	SCHOOL	DEPEN	SAVING	CRISIS
PUBLIC	0.143***	1													
PRIVATE	-0.433***	0.0666	1												
TOTAL	-0.285***	0.545***	0.814***	1											
FIRM	-0.374***	0.0244	0.862***	0.721***	1										
HOUSE	-0.404***	0.164***	0.747***	0.666***	0.552***	1									
GDPpc	-0.382***	0.134	0.688***	0.616**	0.429**	0.563**	1								
GOVCONS	-0.0734*	0.277***	0.150***	0.224***	0.118***	0.397***	0.142***	1							
INFL	0.0174	-0.170**	-0.443***	-0.461***	-0.321***	-0.434***	-0.486**	-0.241***	1						
TRADE	-0.152***	0.212***	0.578***	0.526***	0.347**	0.429***	0.698***	0.0769*	-0.308***	1					
SCHOOL	-0.176***	0.228**	0.583***	0.580**	0.386***	0.533**	0.793***	0.322**	-0.506***	0.689***	1				
DEPEN	0.113***	-0.0855**	-0.437***	-0.399***	-0.129**	-0.423***	-0.746***	-0.217***	0.426**	-0.500***	-0.756***	1			
SAVING	0.0275	-0.280***	0.0211	-0.160**	-0.0705*	-0.199**	0.315**	-0.346**	-0.00552	0.244***	0.187***	-0.224***	1		
CRISIS	-0.327***	-0.102***	0.180***	0.0999**	0.169***	0.132***	0.0168	-0.0138	0.145***	-0.0644*	-0.0273	0.0897**	0.0367	1	

2 Preliminary Investigation of the Instrument's Strength

Blundell et Bond (2000) suggest this simple test to assess the weakness of the potential internal instruments. For the difference GMM instruments, it consists of a regression of the variable in first-difference on their lagged levels. The R^2 statistics provide simple information about the overall fit of such instruments. In the same manner, for the Level-GMM instruments, the variable in level is regressed on its lagged difference. These regressions mimic the underlying instrumenting procedure of the System-GMM estimator. Here, the extremely low R^2 statistics for most sets of instruments suggest at best a weak instruments problem.

Table B.5 – Minimalist R^2 test of instrument's strength.

Difference-GMM Instruments						Level-GMM Instruments	
	L(2/2)	L(2/3)	L(2/4)	L(2/5)	L(2/6)		Δ .L(2)
$\Delta \ln(\text{GDP})$	0.12	0.17	0.19	0.20	0.20	$\ln(\text{GDP})$	0.10
$\Delta \ln(\text{Public Debt})$	0.04	0.07	0.07	0.07	0.06	$\ln(\text{Pub. Debt})$	0.01
$\Delta \ln(\text{Private Debt})$	0.01	0.03	0.04	0.04	0.04	$\ln(\text{Priv. Debt})$	<0.01
$\Delta \ln(\text{Firm Debt})$	0.01	0.02	0.03	0.02	0.02	$\ln(\text{Firm Debt})$	0.01
$\Delta \ln(\text{House Debt})$	0.04	0.11	0.13	0.16	0.25	$\ln(\text{House Debt})$	<0.01
$\Delta \ln(\text{Gov. cons.})$	0.01	0.01	0.01	<0.01	0.01	$\ln(\text{Gov. cons.})$	<0.01
$\Delta \ln(\text{Trade})$	<0.01	<0.01	<0.01	<0.01	<0.01	$\ln(\text{Trade})$	<0.01
$\Delta \ln(\text{Inflation})$	0.02	0.03	0.03	0.03	0.04	$\ln(\text{Inflation})$	<0.01
$\Delta \ln(\text{School})$	0.03	0.06	0.10	0.15	0.52	$\ln(\text{School})$	<0.01
$\Delta \ln(\text{Dependency})$	0.02	0.03	0.03	0.04	0.54	$\ln(\text{Depend.})$	<0.01
$\Delta \ln(\text{Saving})$	0.05	0.02	0.02	<0.01	0.03	$\ln(\text{Saving})$	0.02

3 Financial Interplay and Medium-Run Output Growth

3.1 Financial Structure

Table B.6 – Financial structure changes and medium-run future growth, OLS-FE estimates.

OLS-FE	(1)	(2)	(3)	(4)
Δ Total debt/GDP	-3.875*** (1.305)			
Δ Public debt/GDP		-0.352 (0.491)		-0.470 (0.462)
Δ Private credit/GDP			-2.909*** (0.858)	-2.967*** (0.863)
Log(initial GDP/capita)	-5.732*** (1.885)	-5.957*** (1.863)	-5.852*** (1.890)	-5.842*** (1.876)
Log(Government consumption)	-0.294 (1.003)	-0.480 (0.959)	-0.285 (1.131)	-0.222 (1.081)
Log(Inflation rate)	-0.454*** (0.115)	-0.430*** (0.116)	-0.500*** (0.126)	-0.489*** (0.121)
Log(Trade openness)	2.062** (0.814)	2.165** (0.805)	2.075** (0.833)	2.076** (0.830)
Log(School)	1.231 (1.263)	1.277 (1.261)	1.437 (1.243)	1.367 (1.260)
Log(Dependency ratio)	-4.938** (1.959)	-5.191*** (1.825)	-4.902** (1.975)	-4.965** (1.939)
Log(Saving)	1.363 (0.836)	1.559* (0.818)	1.738** (0.771)	1.655* (0.825)
Observations	1,020	1,020	1,020	1,020
R^2	0.456	0.441	0.454	0.455
Number of countries	34	34	34	34
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.7 – Financial structure and medium-run future growth, controlling for banking crisis, OLS FE estimates.

OLS-FE	(1)	(2)	(3)	(4)
Log(Total debt/GDP)	-0.204 (0.703)			
Log(Public debt/GDP)		0.758** (0.359)		0.610* (0.350)
Log(Private credit/GDP)			-1.295** (0.561)	-0.952* (0.536)
Banking crisis	-6.210*** (1.192)	-5.566*** (1.019)	-5.158*** (1.097)	-4.891*** (0.995)
Log(initial GDP/capita)	-5.718*** (1.787)	-5.798*** (1.412)	-5.236*** (1.767)	-5.408*** (1.421)
Log(Government consumption)	-0.563 (1.051)	-0.730 (1.216)	-0.785 (1.197)	-0.871 (1.292)
Log(Inflation rate)	-0.319*** (0.098)	-0.288*** (0.090)	-0.374*** (0.100)	-0.337*** (0.088)
Log(Trade openness)	1.817** (0.784)	1.312 (0.866)	2.161*** (0.752)	1.695* (0.846)
Log(School)	1.355 (1.084)	1.150 (1.214)	1.024 (1.111)	0.928 (1.195)
Log(Dependency ratio)	-4.335** (2.001)	-4.943** (1.838)	-4.275** (1.914)	-4.767** (1.814)
Log(Saving)	1.470* (0.774)	2.138*** (0.732)	1.197* (0.706)	1.760** (0.748)
Observations	1,026	1,026	1,026	1,026
R^2	0.441	0.477	0.481	0.499
Number of countries	34	34	34	34
Country FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.8 – Financial structure and medium-run future growth, various sub-groups of countries, OLS Fixed Effects estimates.

Sub-group :	Non-Advanced			Advanced			EU			Eurozone		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)				
OLS-FE												
Log(Public debt/GDP)	1.033** (0.432)	0.218 (0.527)	-0.155 (0.460)	-0.278 (0.507)	0.393 (0.352)	0.862** (0.366)	0.764 (0.694)	0.893 (0.501)				
Log(Private credit/GDP)	-0.266 (0.957)		-1.246*** (0.412)		-1.394*** (0.381)		-1.655*** (0.483)					
Log(Firm credit/GDP)		-0.274 (1.159)		-0.189 (0.563)		0.054 (0.702)		-0.566 (0.787)				
Log(Household credit/GDP)		-0.128 (0.264)		-1.233** (0.474)		-1.202** (0.473)		-1.178** (0.505)				
Log(initial GDP/capita)	-4.549*** (1.460)	-10.481*** (1.389)	-11.751*** (2.251)	-10.896*** (2.682)	-9.605*** (1.989)	-7.427** (3.110)	-12.270*** (3.222)	-11.988** (4.040)				
Log(Government consumption)	-0.523 (1.523)	1.889 (2.193)	0.092 (1.648)	0.453 (1.633)	-0.668 (1.560)	-0.124 (1.673)	3.504*** (1.101)	5.426*** (1.366)				
Log(Inflation rate)	-0.319** (0.113)	-0.215 (0.125)	-0.127 (0.114)	-0.208* (0.118)	-0.274* (0.134)	-0.399*** (0.123)	0.081 (0.268)	0.021 (0.174)				
Log(Trade openness)	0.077 (1.074)	-0.219 (1.206)	2.092*** (0.650)	2.257*** (0.656)	2.825** (0.994)	2.630*** (0.803)	2.470** (0.908)	2.446** (0.917)				
Log(School)	5.019*** (1.145)	1.100 (2.922)	-2.583* (1.345)	-1.524 (1.780)	-4.568** (1.561)	-4.453** (2.055)	-3.661* (1.783)	-2.138 (1.951)				
Log(Dependency ratio)	1.177 (3.055)	-7.256 (5.202)	-5.449*** (1.759)	-6.118*** (1.696)	-6.886*** (1.969)	-7.986** (3.036)	-5.051** (2.025)	-4.520* (2.390)				
Log(Saving)	1.455* (0.697)	1.921* (1.020)	1.157 (0.887)	0.709 (1.038)	1.185 (0.864)	1.016 (0.870)	2.487*** (0.585)	2.364** (0.840)				
Observations	432	257	594	541	490	434	330	291				
R ²	0.517	0.667	0.756	0.769	0.776	0.798	0.840	0.856				
Number of countries	16	16	18	18	16	16	10	10				
Country FE	YES	YES	YES	YES	YES	YES	YES	YES				
Time FE	YES	YES	YES	YES	YES	YES	YES	YES				

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

3.2 Quadratic Form

This section delves into potential nonlinearities and further analyze the complexity of the financial structure through a quadratic model allowing for a non-monotonic relation between public debt and medium-run future growth :

$$\begin{aligned} GROWTH_{i,t,t+h} = & \beta_1 PUBLI C_{i,t} + \beta_2 (PUBLI C_{i,t})^2 + \beta_3 \ln(PRIVATE_{i,t}) \\ & + \lambda y_{i,t} + \theta \mathbf{X}_{i,t} + \mu_i + \gamma_t + \varepsilon_{i,t,t+h} \end{aligned} \quad (\text{B.1})$$

with time- and country-specific fixed effects (μ_i and γ_t respectively). The two variables $PUBLI C_{i,t}$ and $(PUBLI C_{i,t})^2$ are the ratio of public debt to GDP and its square term, respectively. $\mathbf{X}_{i,t}$ includes the various control variables presented in section 2.1.

The method proposed by Sasabuchi (1980) and developed by Lind et Mehlum (2011), henceforth SLM test, is suited to ascertain the location and relevance of the extremum point. It involves determining whether the marginal effect of public debt on growth is significantly different from zero and positive at a low level but negative at a high level, within-sample :

$$\begin{aligned} H_0 : & (\beta_1 + 2\beta_2 PUBLI C_{\min} \leq 0) \cup (\beta_1 + 2\beta_2 PUBLI C_{\max} \geq 0) \\ H_1 : & (\beta_1 + 2\beta_2 PUBLI C_{\min} > 0) \cup (\beta_1 + 2\beta_2 PUBLI C_{\max} < 0) \end{aligned}$$

Table B.9 – Nonlinearities in public debt and medium-run future growth, various sub-groups of countries, OLS Fixed Effects estimates.

OLS-FE Sub-group :	(1) All	(2) Advanced	(3) ∅Advanced	(4) EU	(5) Eurozone
Public debt/GDP	2.784** (1.315)	-0.053 (1.208)	5.770** (2.158)	3.208** (1.333)	2.795 (3.197)
(Public debt/GDP) ²	-1.216** (0.503)	-0.479 (0.491)	-2.688** (1.132)	-2.484** (0.844)	-1.535 (1.594)
Log(Private credit/GDP)	-1.655*** (0.557)	-1.706*** (0.333)	-0.310 (0.968)	-1.347*** (0.308)	-1.666** (0.519)
Log(initial GDP/capita)	-5.069*** (1.573)	-12.485*** (2.185)	-4.227** (1.624)	-11.056*** (1.987)	-12.674*** (3.489)
Log(Government consumption)	-0.707 (1.247)	0.660 (1.505)	-0.469 (1.613)	-1.467 (1.439)	2.780* (1.444)
Log(Inflation rate)	-0.465*** (0.108)	-0.171 (0.124)	-0.324*** (0.094)	-0.320** (0.135)	-0.013 (0.259)
Log(Trade openness)	2.306*** (0.780)	2.106*** (0.618)	0.135 (1.055)	3.058*** (0.824)	2.843** (0.893)
Log(School)	0.763 (1.251)	-2.846** (1.265)	5.235*** (1.181)	-4.433** (1.571)	-3.676* (1.762)
Log(Dependency ratio)	-4.625** (1.831)	-4.719** (1.910)	1.544 (3.213)	-7.669*** (1.941)	-5.054** (1.973)
Log(Saving)	1.510* (0.748)	0.781 (0.832)	1.341* (0.676)	0.380 (0.728)	1.919** (0.687)
$\partial Growth/\partial Public = 0$	115%**	–	107%**	65%**	91%
90% Fieller CI	[67%-136%]	–	[91%-159%]	[36%-83%]	–
SLM test (<i>p</i> -value)	0.03	–	0.02	0.02	0.21
Observations	1,026	594	432	490	330
<i>R</i> ²	0.498	0.767	0.518	0.791	0.840
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The SLM test provides *p*-value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** *p* < 0.01, ** *p* < 0.05, * *p* < 0.10.

Table B.10 – Nonlinearities in private credit and medium-run future growth, various sub-groups of countries, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Private credit/GDP	-3.394** (1.304)	-3.332** (1.182)	0.948 (2.769)	-3.726*** (0.841)	-4.694*** (1.334)
(Private credit/GDP) ²	0.666 (0.403)	0.931** (0.393)	-1.153 (0.928)	1.083*** (0.316)	1.444*** (0.383)
Log(Public debt/GDP)	0.626* (0.367)	0.071 (0.536)	1.021** (0.426)	0.762* (0.422)	1.087 (0.622)
Log(initial GDP/capita)	-5.114*** (1.377)	-11.746*** (2.097)	-3.927*** (1.331)	-9.083*** (1.972)	-12.372*** (2.331)
Log(Government consumption)	-1.125 (1.296)	-0.442 (1.542)	-0.567 (1.536)	-1.662 (1.623)	2.394 (1.369)
Log(Inflation rate)	-0.423*** (0.096)	-0.159 (0.108)	-0.284** (0.108)	-0.294** (0.111)	-0.006 (0.229)
Log(Trade openness)	1.985** (0.790)	2.303*** (0.789)	0.064 (1.059)	3.056*** (0.898)	3.210*** (0.716)
Log(School)	0.685 (1.169)	-2.619* (1.367)	3.917*** (1.241)	-4.137*** (1.291)	-2.950* (1.415)
Log(Dependency ratio)	-4.968*** (1.623)	-5.235*** (1.771)	1.977 (2.822)	-6.474*** (1.909)	-4.453*** (1.869)
Log(Saving)	1.504** (0.678)	0.928 (0.811)	1.292* (0.699)	0.922 (0.815)	2.185*** (0.503)
$\partial Growth/\partial Private = 0$	255%	180%	41%	172%	162%**
90% Fieller CI	–	–	–	–	[132%-192%]
SLM test (<i>p</i> -value)	0.35	0.24	–	0.19	0.03
Observations	1,026	594	432	490	330
<i>R</i> ²	0.509	0.760	0.537	0.789	0.861
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The SLM test provides *p*-value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

3.3 Hansen Panel Threshold Regressions

This section provides additional estimates based on a fixed-effect panel threshold method (Hansen, 1999; Wang, 2015) to empirically investigate non-monotonicity in the financial structure. This technique has the advantage of not imposing a priori restriction on the relationship, as would a quadratic form. It is suited to estimate a structural break between financial depth and growth, as follow :

$$\Delta y_{i,t+k} = \lambda y_{i,t} + \beta_1 (\psi_{i,t} < \tau) PC_{i,t} + \beta_2 (\psi_{i,t} \geq \tau) PC_{i,t} + \gamma \mathbf{x}_{i,t} + \nu_{it+k}$$

with $\nu_{it+k} = \mu_i + \lambda_{t+k} + \varepsilon_{i,t+k}$

where the threshold variable $\psi_{i,t}$ represents successively the ratio of private credit to GDP, the ratio of public debt to GDP, and the GDP per capita (in logs). τ is the threshold parameter that divides the equation into two regimes with β_1 and β_2 as respective coefficients (for more details, see Hansen, 1999; Wang, 2015).

The LR statistic from the various estimates, reported each time in a figure, is constructed as :

$$LR = \frac{(\text{SSR}(\tau) - \text{SSR}(\hat{\tau}))}{\hat{\sigma}^2} \quad \text{with} \quad \hat{\tau} = \arg \min \text{SSR}(\tau)$$

SSR is the sum of squared residuals obtained by estimating the model for different values of the threshold variable.

Table B.11 – Nonlinearities in private credit and medium-run future growth, various sub-groups of countries, Hansen Panel Threshold Fixed Effects estimates.

HPT-FE Sub-group :	(1) All	(2) Advanced	(3) ∅Advanced	(4) EU	(5) Eurozone
Private credit/GDP < τ	0.976** (0.393)	-0.993*** (0.221)	-2.755*** (1.007)	-1.156*** (0.287)	-1.553*** (0.318)
Private credit/GDP $\geq \tau$	-1.192*** (0.194)	-0.377* (0.194)	-0.816 (0.667)	-0.472* (0.248)	-0.658** (0.267)
Log(Public debt/GDP)	0.367*** (0.142)	0.500*** (0.148)	1.088*** (0.258)	1.140*** (0.177)	1.533*** (0.217)
Log(initial GDP/capita)	-7.362*** (0.427)	-10.074*** (0.659)	-5.410*** (0.709)	-10.129*** (0.776)	-11.025*** (0.881)
Log(Government consumption)	-1.575*** (0.506)	-0.586 (0.565)	-1.890** (0.750)	-0.015 (0.654)	2.660*** (0.822)
Log(Inflation rate)	-0.403*** (0.075)	-0.347*** (0.070)	-0.652*** (0.165)	-0.276*** (0.083)	-0.258** (0.111)
Log(Trade openness)	1.998*** (0.306)	2.824*** (0.362)	-0.310 (0.561)	2.913*** (0.550)	3.336*** (0.607)
Log(School)	1.437*** (0.493)	-3.227*** (0.549)	6.537*** (0.929)	-4.991*** (0.676)	-4.379*** (0.700)
Log(Dependency ratio)	-5.628*** (0.671)	-4.905*** (0.788)	1.527 (1.462)	-4.957*** (0.946)	-4.677*** (0.981)
Log(Saving)	1.452*** (0.297)	1.431*** (0.263)	1.708*** (0.643)	1.817*** (0.301)	2.934*** (0.336)
Threshold τ	68***	155***	0.55	150***	155***
90% CI	[65%-72%]	[155%-156%]	[53%-0.55%]	[147%-151%]	[153%-156%]
Threshold-effect F -test (p -value)	0.01	0.00	0.71	0.01	0.01
Observations R^2	0.569	0.737	0.659	0.780	0.821
Number of countries	25	18	7	13	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of HPT fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The threshold-effect F -test provides p -value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

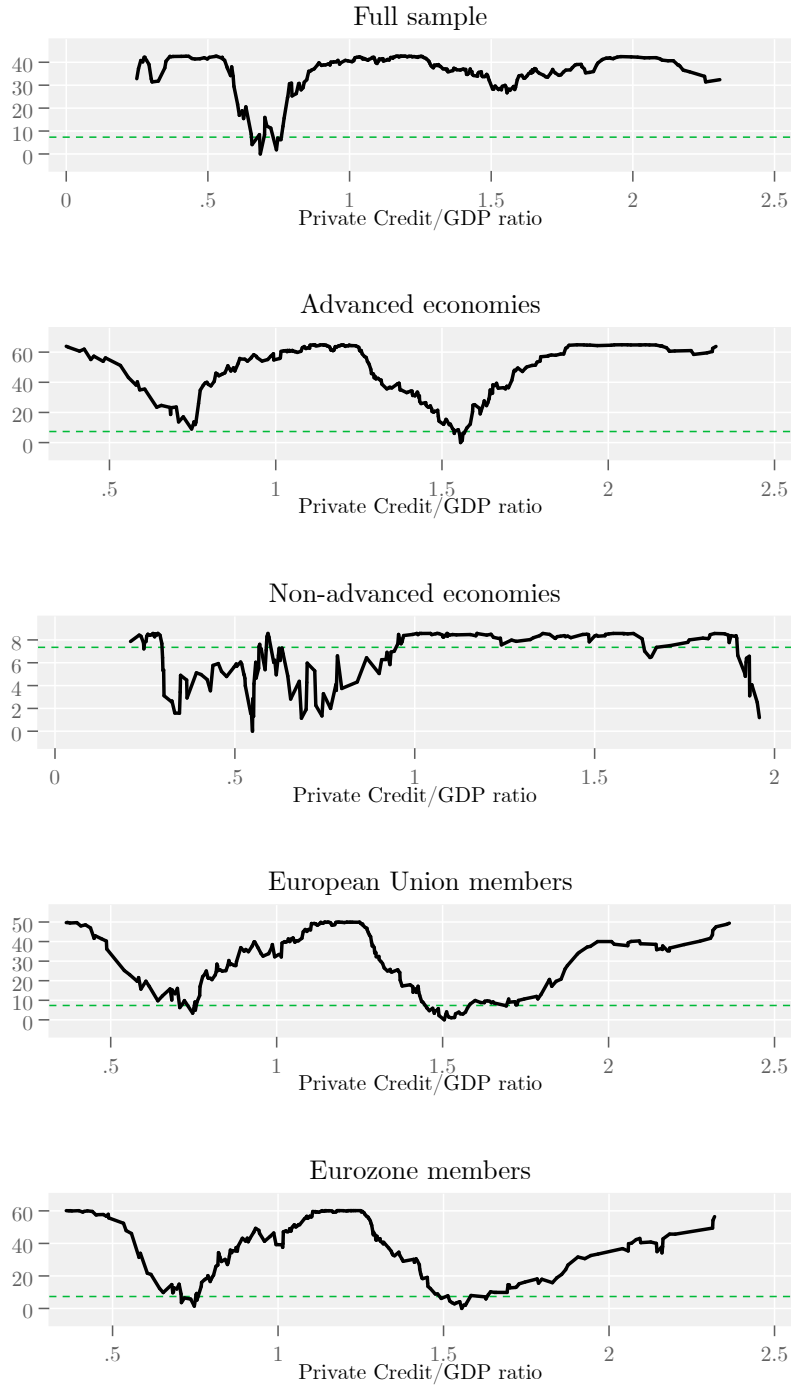


Figure B.2 – LR statistic from Table B.11 columns (1) through (5), respectively from top to bottom. The green dashed line denotes the critical value (7.35) at the 95% confidence level for the LR statistic.

Table B.12 – Nonlinearities in public debt and medium-run future growth, various sub-groups of countries, Hansen Panel Threshold Fixed Effects estimates.

HPT-FE Sub-group :	(1) All	(2) Advanced	(3) ∅Advanced	(4) EU	(5) Eurozone
Public debt/GDP < τ	1.230*** (0.313)	0.953*** (0.297)	5.762*** (0.863)	1.524*** (0.376)	2.615*** (0.511)
Public debt/GDP $\geq \tau$	0.195 (0.254)	-0.119 (0.227)	3.683*** (0.634)	0.441 (0.337)	1.581*** (0.459)
Log(Private credit/GDP)	-1.523*** (0.228)	-1.032*** (0.257)	0.711 (0.493)	-1.312*** (0.301)	-1.384*** (0.335)
Log(initial GDP/capita)	-6.973*** (0.443)	-10.076*** (0.661)	-6.142*** (0.636)	-10.133*** (0.800)	-10.543*** (0.946)
Log(Government consumption)	-1.064** (0.508)	0.070 (0.566)	-1.923*** (0.735)	0.033 (0.676)	3.001*** (0.948)
Log(Inflation rate)	-0.459*** (0.077)	-0.392*** (0.070)	-0.567*** (0.161)	-0.368*** (0.084)	-0.305*** (0.115)
Log(Trade openness)	2.438*** (0.312)	2.151*** (0.347)	-0.452 (0.554)	2.516*** (0.532)	2.780*** (0.599)
Log(School)	1.357*** (0.495)	-3.124*** (0.533)	7.607*** (0.838)	-4.661*** (0.679)	-4.212*** (0.734)
Log(Dependency ratio)	-4.903*** (0.682)	-5.166*** (0.781)	1.132 (1.346)	-5.951*** (0.969)	-4.930*** (1.068)
Log(Saving)	1.545*** (0.306)	1.223*** (0.288)	1.656*** (0.606)	1.348*** (0.346)	2.647*** (0.418)
Threshold τ	101%*	99%**	68%	99%**	99%*
90% CI	[100%–102%]	[98%–100%]	[67%–69%]	[98%–100%]	[99%–100%]
Threshold-effect F -test (p -value)	0.06	0.02	0.29	0.03	0.09
Observations	825	594	231	429	330
R^2	0.558	0.740	0.685	0.777	0.801
Number of countries	25	18	7	13	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of HPT fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The threshold-effect F -test provides p -value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

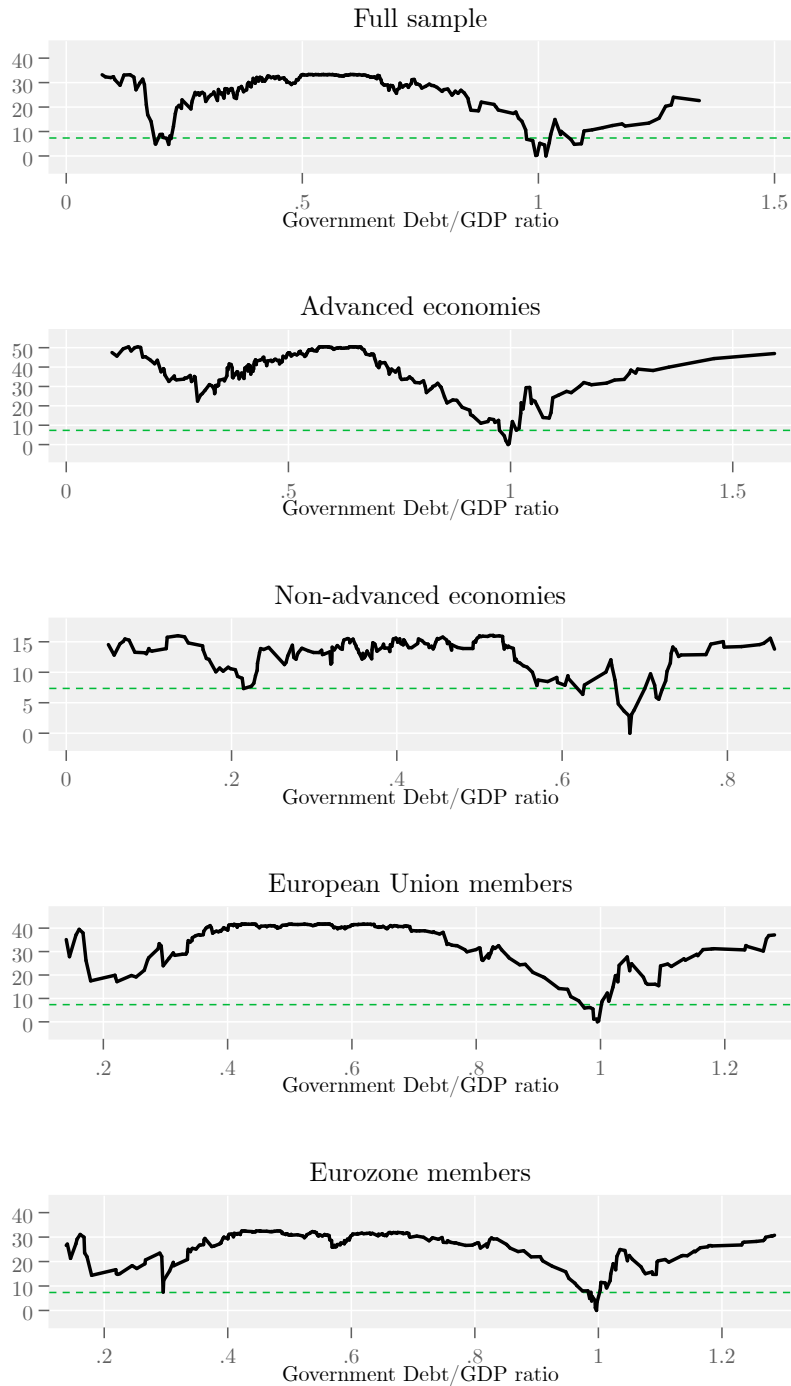


Figure B.3 – LR statistic from Table B.12 columns (1) through (5), respectively from top to bottom. The green dashed line denotes the critical value (7.35) at the 95% confidence level for the LR statistic.

Table B.13 – Nonlinearities in private credit and medium-run future growth, various sub-groups of countries, Hansen Panel Threshold Fixed Effects estimates for various levels of Public debt/GDP.

HPT-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Private credit/GDP	-1.321***	-0.190	-0.428	0.006	0.003
IF Public debt/GDP < τ	(0.195)	(0.191)	(0.634)	(0.244)	(0.275)
Private credit/GDP	-2.130***	-1.054***	0.660	-0.835***	-0.609**
IF Public debt/GDP $\geq \tau$	(0.247)	(0.223)	(0.678)	(0.279)	(0.309)
Log(Public debt/GDP)	0.567***	0.638***	0.423	1.132***	1.637***
	(0.144)	(0.150)	(0.272)	(0.180)	(0.233)
Log(initial GDP/capita)	-7.150***	-10.583***	-5.344***	-10.809***	-10.927***
	(0.430)	(0.660)	(0.670)	(0.792)	(0.948)
Log(Government consumption)	-0.810	0.468	-2.626***	0.129	2.417***
	(0.500)	(0.562)	(0.728)	(0.665)	(0.905)
Log(Inflation rate)	-0.398***	-0.327***	-0.653***	-0.259***	-0.117
	(0.076)	(0.070)	(0.157)	(0.084)	(0.118)
Log(Trade openness)	1.805***	1.812***	0.372	2.208***	2.207***
	(0.308)	(0.356)	(0.523)	(0.549)	(0.634)
Log(School)	1.231**	-3.394***	7.624***	-4.321***	-3.523***
	(0.495)	(0.549)	(0.921)	(0.681)	(0.743)
Log(Dependency ratio)	-4.613***	-4.784***	0.694	-5.519***	-5.189***
	(0.677)	(0.788)	(1.332)	(0.962)	(1.053)
Log(Saving)	1.803***	1.652***	0.692	1.955***	3.036***
	(0.290)	(0.259)	(0.635)	(0.304)	(0.370)
Threshold τ	100%*	100%***	46%	100%*	100%
90% CI	[98%–101%]	[98%–101%]	[45%–47%]	[99%–101%]	[99%–101%]
Threshold-effect F -test (p -value)	0.08	0.00	0.22	0.06	0.40
Observations	825	594	231	429	330
R^2	0.563	0.738	0.689	0.772	0.793
Number of countries	25	18	7	13	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of HPT fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The threshold-effect F -test provides p -value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

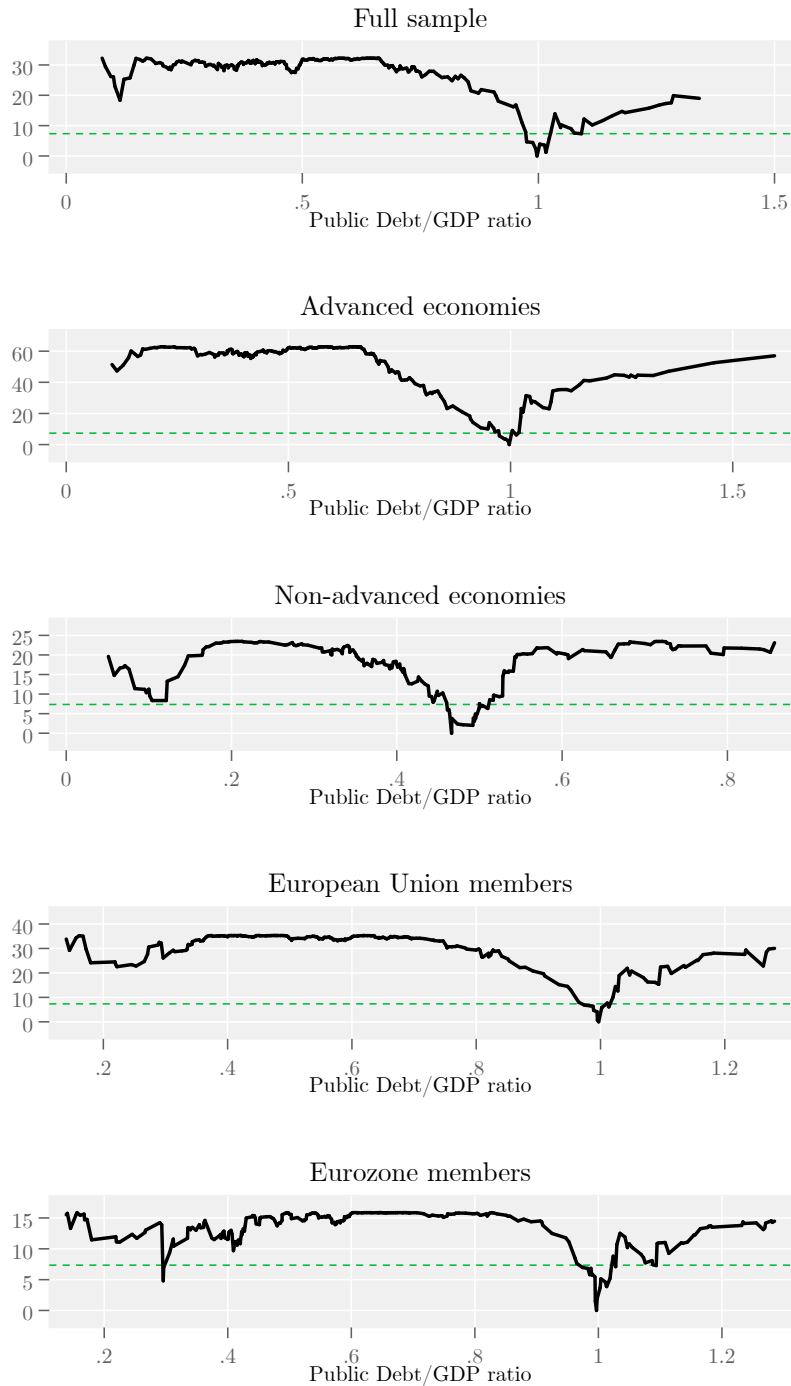


Figure B.4 – LR statistic from Table B.13 columns (1) through (5), respectively from top to bottom. The green dashed line denotes the critical value (7.35) at the 95% confidence level for the LR statistic.

Table B.14 – Nonlinearities in private credit and medium-run future growth, various sub-groups of countries, Hansen Panel Threshold Fixed Effects estimates for various levels of GDP per capita.

HPT-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Private credit/GDP < τ	4.120***	-0.050	4.294***	-0.082	0.084
IF GDP/capita < τ	(0.564)	(0.199)	(0.800)	(0.244)	(0.254)
Private credit/GDP $\geq \tau$	-1.255***	0.299	-0.805	0.532**	1.054***
IF GDP/capita $\geq \tau$	(0.187)	(0.224)	(0.572)	(0.259)	(0.284)
Log(Public debt/GDP)	0.427***	0.560***	1.083***	0.901***	1.517***
	(0.137)	(0.158)	(0.222)	(0.181)	(0.216)
Log(initial GDP/capita)	-7.462***	-10.705***	-6.489***	-11.570***	-13.259***
	(0.412)	(0.699)	(0.605)	(0.810)	(0.921)
Log(Government consumption)	-1.259***	-0.165	-2.055***	0.852	3.508***
	(0.480)	(0.585)	(0.647)	(0.660)	(0.812)
Log(Inflation rate)	-0.280***	-0.287***	-0.547***	-0.293***	-0.213*
	(0.073)	(0.073)	(0.143)	(0.085)	(0.109)
Log(Trade openness)	1.812***	1.980***	-0.088	2.545***	3.198***
	(0.295)	(0.371)	(0.469)	(0.552)	(0.598)
Log(School)	1.242***	-2.165***	5.819***	-2.976***	-0.919
	(0.474)	(0.620)	(0.798)	(0.711)	(0.746)
Log(Dependency ratio)	-6.861***	-5.311***	-4.278***	-6.201***	-6.445***
	(0.666)	(0.819)	(1.307)	(0.968)	(0.981)
Log(Saving)	1.886***	1.606***	2.064***	1.757***	2.696***
	(0.276)	(0.274)	(0.556)	(0.312)	(0.339)
Threshold τ	2067***	43768	2067***	38236*	39915***
90% CI	[1857–2084]	[43239–43956]	[1968–2071]	[37952–38372]	[39297–40052]
Threshold-effect F -test (p -value)	0.00	0.68	0.00	0.09	0.00
Observations	825	594	231	429	330
R^2	0.599	0.715	0.747	0.773	0.824
Number of countries	25	18	7	13	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of HPT fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The threshold-effect F -test provides p -value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

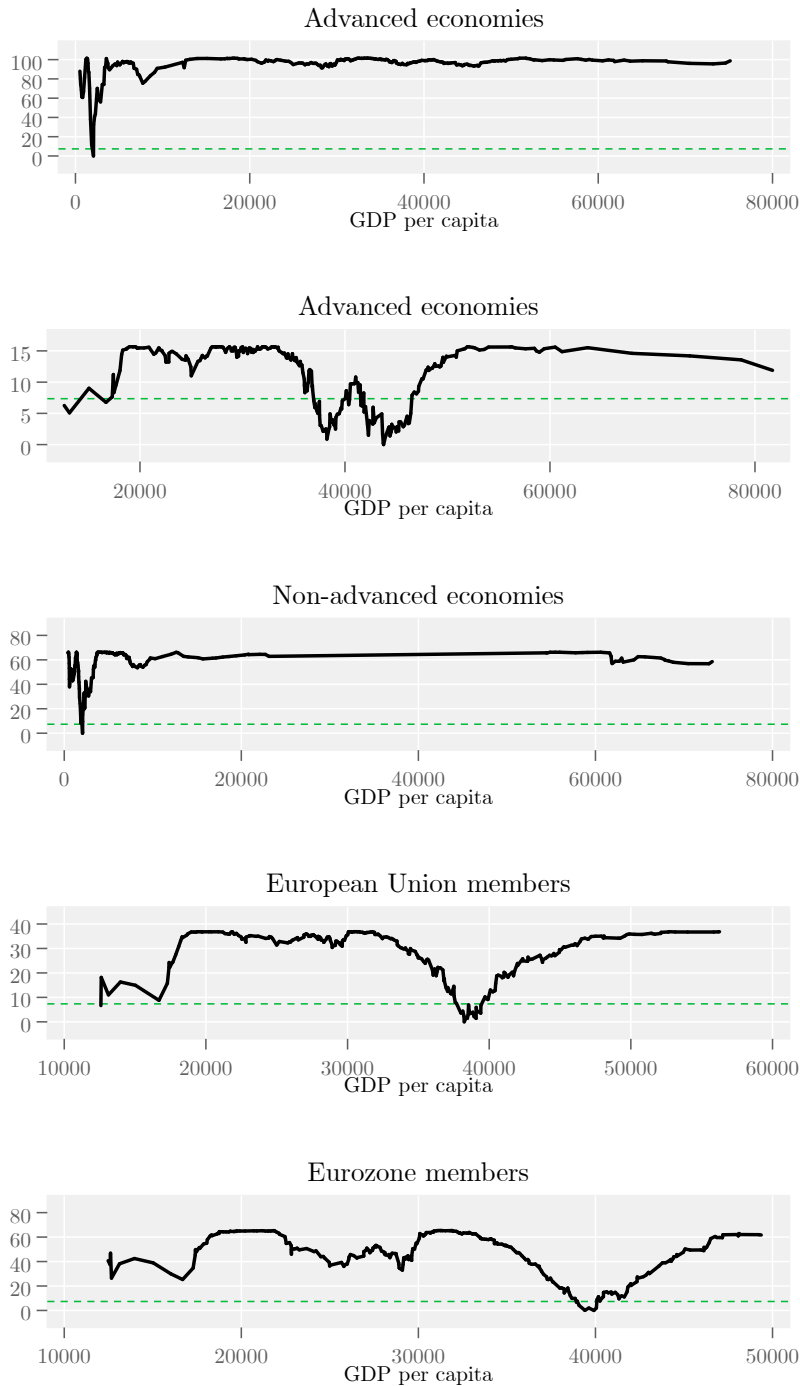


Figure B.5 – LR statistic from Table B.14 columns (1) through (5), respectively from top to bottom. The green dashed line denotes the critical value (7.35) at the 95% confidence level for the LR statistic.

3.4 Interplay Estimates

This section provides additional regressions to investigate the interplay between private credit and public debt, expression (II.2) is rewritten in the following form :

$$\begin{aligned} GROWTH_{i,t,t+h} &= \beta_1 PUBLI C_{i,t} + \beta_2 PRIVATE_{i,t} \\ &+ \beta_3 (PUBLI C_{i,t} \times PRIVATE_{i,t}) \\ &+ \lambda y_{i,t} + \theta \mathbf{X}_{i,t} + \mu_i + \gamma_t + \varepsilon_{i,t,t+h} \end{aligned} \quad (\text{B.2})$$

where $PUBLI C_{i,t}$ and $PRIVATE_{i,t}$ are ratios of public debt and private credit to GDP, respectively. Interacting public debt and private credit is expected to shed light on potential interplay.

The marginal effect of public debt or private credit can be evaluated by examining the partial derivatives of economic growth for each variable :

$$\frac{\partial GROWTH_{i,t,t+h}}{\partial PUBLI C_{i,t}} = \beta_1 + \beta_3 PRIVATE_{i,t}$$

Table B.15 – Interplay between private credit and public debt in level, medium-run future growth, various sub-groups of countries, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Private credit/GDP	-0.885* (0.522)	0.033 (0.606)	-2.493* (1.203)	-0.785 (0.702)	-0.297 (0.989)
Public debt/GDP	2.168** (0.993)	0.953 (1.224)	0.995 (1.728)	-0.026 (1.145)	0.943 (1.605)
Private credit/GDP × Public debt/GDP	-1.543** (0.698)	-1.233* (0.707)	1.226 (1.929)	-0.436 (0.686)	-0.195 (1.055)
Log(initial GDP/capita)	-5.171*** (1.645)	-12.612*** (2.304)	-3.367* (1.786)	-11.269*** (2.312)	-13.138*** (3.940)
Log(Government consumption)	-0.616 (1.150)	1.226 (1.276)	-0.870 (1.590)	0.025 (1.520)	4.339** (1.349)
Log(Inflation rate)	-0.428*** (0.114)	-0.129 (0.116)	-0.398*** (0.096)	-0.239 (0.153)	0.107 (0.274)
Log(Trade openness)	1.879*** (0.680)	1.850** (0.665)	0.587 (0.859)	2.966** (1.099)	3.107** (1.174)
Log(School)	0.844 (1.055)	-2.516** (1.184)	3.963*** (1.285)	-4.075** (1.507)	-2.855 (1.604)
Log(Dependency ratio)	-3.571** (1.551)	-3.751* (1.816)	1.748 (2.822)	-7.018** (2.577)	-3.741 (3.134)
Log(Saving)	1.409** (0.682)	1.370 (0.948)	0.949 (0.644)	1.107 (0.956)	3.017*** (0.744)
Observations	1,026	594	432	490	330
R^2	0.497	0.758	0.518	0.762	0.812
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The SLM test provides p -value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.16 – Interplay between private credit and public debt in logs, medium-run future growth, various sub-groups of countries, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Log(Private credit/GDP)	-2.036*** (0.540)	-1.672*** (0.423)	-0.553 (1.074)	-1.525*** (0.388)	-1.566** (0.533)
Log(Public debt/GDP)	0.617* (0.351)	0.146 (0.393)	0.960* (0.478)	0.411 (0.338)	0.820 (0.712)
Log(Private credit/GDP) × Log(Public debt/GDP)	-0.771** (0.327)	-1.038** (0.444)	-0.290 (0.563)	-0.377 (0.408)	0.318 (0.540)
Log(initial GDP/capita)	-5.423*** (1.321)	-11.232*** (1.941)	-4.564*** (1.461)	-9.528*** (1.933)	-12.341*** (3.353)
Log(Government consumption)	-0.686 (1.349)	0.263 (1.492)	-0.424 (1.587)	-0.616 (1.501)	3.528** (1.127)
Log(Inflation rate)	-0.480*** (0.104)	-0.154 (0.122)	-0.327** (0.115)	-0.285* (0.137)	0.105 (0.272)
Log(Trade openness)	2.163** (0.807)	1.972*** (0.613)	0.173 (1.091)	2.929*** (0.956)	2.350** (0.911)
Log(School)	1.054 (1.172)	-2.215* (1.264)	5.032*** (1.156)	-4.364** (1.516)	-3.806* (1.873)
Log(Dependency ratio)	-4.464*** (1.455)	-3.798** (1.427)	1.526 (2.932)	-6.388*** (1.704)	-5.540** (2.345)
Log(Saving)	1.672** (0.711)	1.192 (0.884)	1.455* (0.701)	1.163 (0.869)	2.517*** (0.557)
Observations	1,026	594	432	490	330
R^2	0.509	0.768	0.518	0.778	0.841
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. The SLM test provides p -value for the relevance of the estimated threshold. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.17 – Private credit changes in a financially deepened environment, OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Δ Private credit/GDP	1.432 (2.620)	-6.144 (7.124)	1.242 (2.682)	-4.457 (2.991)	3.707 (6.266)
Private credit/GDP	-1.319*** (0.407)	-0.251 (0.281)	-2.358** (0.842)	-0.556 (0.451)	-0.277 (0.630)
Δ Private credit/GDP × Private credit/GDP	-5.068** (2.439)	1.157 (5.274)	-2.625 (3.296)	-2.161 (2.666)	-6.100 (5.117)
Log(initial GDP/capita)	-5.161*** (1.818)	-11.511*** (2.312)	-3.358 (2.090)	-9.879*** (2.208)	-13.324*** (3.393)
Log(Government consumption)	-0.775 (1.186)	0.634 (1.518)	-1.017 (1.502)	0.274 (1.446)	3.877*** (1.021)
Log(Inflation rate)	-0.426*** (0.122)	-0.124 (0.115)	-0.403*** (0.096)	-0.263* (0.137)	0.073 (0.230)
Log(Trade openness)	1.970** (0.726)	2.088*** (0.704)	1.298 (0.824)	2.926** (1.065)	3.100** (1.072)
Log(School)	0.977 (1.063)	-2.551** (1.205)	3.702*** (1.123)	-3.982** (1.718)	-2.427 (1.703)
Log(Dependency ratio)	-3.980** (1.901)	-5.724*** (1.835)	1.871 (2.676)	-7.654** (2.759)	-4.521 (2.903)
Log(Saving)	1.503** (0.711)	1.625* (0.821)	0.454 (0.923)	1.468* (0.827)	2.662** (0.821)
Observations	1,014	594	420	488	330
R^2	0.501	0.757	0.489	0.784	0.817
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.18 – Public debt changes in time of low fiscal capacity, OLS Fixed Effects estimates.

OLS-FE Sub-group :	(1) All	(2) Advanced	(3) ∅Advanced	(4) EU	(5) Eurozone
Δ Public debt/GDP	0.863 (1.046)	1.292 (1.156)	-0.132 (0.894)	3.965* (2.044)	0.202 (2.118)
Public debt/GDP	1.150* (0.618)	-0.367 (0.308)	2.511** (1.026)	0.462 (0.614)	1.005 (0.916)
Δ Public debt/GDP × Public debt/GDP	-1.629 (2.003)	0.728 (3.079)	-2.970 (1.790)	-0.797 (3.911)	3.037 (3.888)
Log(initial GDP/capita)	-5.767*** (1.759)	-12.264*** (2.534)	-4.034** (1.689)	-9.579*** (2.327)	-11.943** (3.865)
Log(Government consumption)	-0.763 (1.166)	0.737 (1.287)	-0.798 (1.398)	-0.011 (1.342)	3.879** (1.269)
Log(Inflation rate)	-0.438*** (0.113)	-0.150 (0.115)	-0.296** (0.120)	-0.209 (0.137)	0.072 (0.237)
Log(Trade openness)	1.819** (0.847)	2.419*** (0.757)	-0.022 (0.795)	3.116*** (0.942)	3.200** (1.079)
Log(School)	1.161 (1.288)	-2.491* (1.299)	4.866*** (0.854)	-3.695** (1.727)	-2.818 (1.894)
Log(Dependency ratio)	-5.485*** (1.781)	-4.667* (2.297)	2.311 (3.068)	-5.892* (3.274)	-3.330 (3.608)
Log(Saving)	2.245** (1.002)	1.875* (0.911)	1.099 (0.884)	2.109* (1.011)	3.463** (1.109)
Observations	1,014	594	420	488	330
R^2	0.458	0.749	0.505	0.769	0.814
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.19 – Private credit changes in time of low fiscal capacity, OLS Fixed Effects estimates.

OLS-FE Sub-group :	(1) All	(2) Advanced	(3) ∅Advanced	(4) EU	(5) Eurozone
Δ Private credit/GDP	-6.234*** (2.167)	-12.036*** (2.356)	-3.254 (2.144)	-9.235*** (2.131)	-9.124** (3.179)
Public debt/GDP	0.924 (0.607)	-0.487 (0.314)	2.381** (0.913)	0.168 (0.600)	0.782 (0.896)
Δ Private credit/GDP × Public debt/GDP	5.343 (3.317)	10.913** (4.206)	5.663* (3.195)	3.758 (3.179)	8.936** (2.920)
Log(initial GDP/capita)	-5.766*** (1.819)	-12.052*** (2.594)	-4.254** (1.784)	-9.609*** (2.246)	-12.410*** (3.773)
Log(Government consumption)	-0.472 (1.260)	1.335 (1.304)	-0.952 (1.582)	0.302 (1.415)	4.359*** (1.242)
Log(Inflation rate)	-0.467*** (0.114)	-0.106 (0.107)	-0.358*** (0.099)	-0.274* (0.130)	0.111 (0.215)
Log(Trade openness)	1.812** (0.870)	2.124*** (0.684)	-0.030 (0.822)	3.275*** (0.984)	2.899** (1.029)
Log(School)	1.206 (1.222)	-2.390* (1.238)	5.081*** (0.767)	-3.840* (1.851)	-2.493 (1.748)
Log(Dependency ratio)	-5.259*** (1.921)	-5.061** (2.367)	2.059 (3.187)	-7.458** (3.157)	-4.208 (3.300)
Log(Saving)	2.337** (0.884)	1.715* (0.915)	1.551* (0.803)	1.622 (0.963)	3.183** (1.023)
Observations	1,014	594	420	488	330
R^2	0.471	0.765	0.498	0.780	0.817
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.20 – Private credit changes in time of low fiscal capacity (log), OLS Fixed Effects estimates.

OLS-FE	(1)	(2)	(3)	(4)	(5)
Sub-group :	All	Advanced	∅Advanced	EU	Eurozone
Δ Private credit/GDP	-1.451 (2.373)	-1.431 (3.748)	0.751 (2.179)	-5.247*** (1.725)	1.190 (1.690)
Log(Public debt/GDP)	0.809** (0.371)	-0.150 (0.430)	1.045** (0.406)	0.605* (0.333)	0.972 (0.664)
Δ Private credit/GDP × Log(Public debt/GDP)	1.938 (2.051)	6.388 (4.045)	1.468 (1.836)	2.146 (1.462)	7.089*** (2.170)
Log(initial GDP/capita)	-5.947*** (1.457)	-11.771*** (2.652)	-4.747*** (1.393)	-9.237*** (2.155)	-11.503** (3.718)
Log(Government consumption)	-0.542 (1.351)	0.983 (1.369)	-0.747 (1.625)	-0.042 (1.504)	3.546** (1.131)
Log(Inflation rate)	-0.438*** (0.104)	-0.087 (0.100)	-0.320** (0.110)	-0.252* (0.123)	0.199 (0.195)
Log(Trade openness)	1.575* (0.908)	2.068*** (0.676)	-0.090 (0.857)	2.802** (0.990)	2.148* (1.063)
Log(School)	1.117 (1.289)	-2.396 (1.430)	5.142*** (0.829)	-4.130** (1.889)	-2.707 (1.951)
Log(Dependency ratio)	-5.432*** (1.817)	-5.236** (2.189)	1.171 (3.028)	-7.407** (2.653)	-4.770 (2.707)
Log(Saving)	2.543*** (0.802)	1.725* (0.916)	1.816** (0.745)	1.861* (0.898)	3.155*** (0.876)
Observations	1,014	594	420	488	330
R^2	0.490	0.762	0.516	0.787	0.829
Number of countries	34	18	16	16	10
Country FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES

Notes : This table reports the results of a set of fixed effects regressions in which the dependent variable is the average real GDP per capita growth rate. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

3.5 Autoregressive Distributed Lag (ARDL) Estimates

The first part of the paper focuses on medium-run evidence, whereas the second part emphasizes the business cycle aspect of financial expansion. Working as a transition, this last subsection implements a model that incorporates both short-run and long-run effects. Indeed, the averaging in the growth regression conceals the dynamic relationship between financial depth and growth at different time frequencies. This first glance at multiple horizons sets the basis for the business cycle analysis.

Several estimation techniques, both consistent and efficient can be conducted using standard tests (Pesaran et Smith, 1995 ; Pesaran, 1997 ; Pesaran, Shin et Smith, 1999). The method consists in a panel error-correcting model, where short-run and long-run effects are estimated jointly from a general autoregressive distributed lag (ARDL) model.

Following Loayza et Rancière (2006), the dynamic heterogeneous panel regression can be incorporated into the error-correction model using the $ARDL(p, q)$ technique, where p is the lag of the dependent variable, and q is the lag of the independent variables as follows :

$$\Delta y_{it} = \sum_{j=1}^{p-1} \gamma_j^i \Delta y_{it-j} + \sum_{j=0}^{q-1} \delta_j^i \Delta X_{it-j} + \psi^i \left[y_{it-1} - (\beta_0^i + \beta_1^i X_{it-1}) \right] + \varepsilon_{it} \quad (\text{B.3})$$

$$y_{it} = \beta_0^i + \beta_1^i X_{it} + \mu_{it} \quad \text{where } \mu_{it} \sim I(0) \quad (\text{B.4})$$

where y is the GDP per capita growth rate, X is a set of independent variables including financial development, γ and δ represent the short-run coefficients of lagged dependent and independent variables respectively, β are the long-run coefficients, and ψ is the coefficient of speed of adjustment to the long-run equilibrium. The subscripts i and t represent country and time indexes, respectively. The term in the square brackets of equation (B.3) contains the long-run growth regression, which is derived from the equation (B.4).

These methods avoid the need for pre-testing and order-of-integration conformability given that they are valid whether the variables of interest are $I(0)$ or $I(1)$ (Engle et Granger, 1987 ; Hassler et Wolters, 2006). The main requirement are : that the coefficient

on the error-correction term ψ be negative and not lower than -2 (within the unit circle). In all the subsequent estimations, this coefficient falls within the dynamically stable range. Other requirements are specific to the precise estimation technique, and are further presented next.

Mean Group estimator. The Mean Group estimator (MG), presented by Pesaran et Smith (1995), relies on estimating N time-series regressions and averaging the coefficient. The MG estimates are thereby the unweighted mean of the N individual regression coefficients. This first estimator does not impose any restrictions. It allows for full heterogeneity as the coefficients can vary in both the long run and short run. However, to be consistent and valid, this method requires a large time-series dimension. Table B.22 reports the estimates for this estimator.

Dynamic Fixed Effect estimator. The Dynamic Fixed Effect (DFE) estimator restricts the coefficients of the cointegrating vector to be equal across all panels. This model further restricts the speed of adjustment coefficient and the short-run coefficients to be equal too. The DFE method is subject to simultaneous equation bias (Baltagi, Griffin et Xiong, 2000). Table B.23 reports the estimates for this estimator.

Pooled Mean Group estimator. This estimator is a balance of the previous two. Its main characteristic is to allow the short run adjustments to be country-specific (including the speed of adjustment), while the long-run coefficients are homogenous across countries. The PMG estimator offers more efficient estimates compared to the MG estimators under the assumption of long-run homogeneity (Samargandi, Fidrmuc et Ghosh, 2015). For more details on the PMG estimator and a comparison with the MG estimator, see Pesaran, Shin and Smith (1999). Following previous empirics, this estimator is the preferred method here :

“It can be argued that country heterogeneity is particularly relevant in short-run relationships, given that countries are affected by overlending and financial crises to widely different degrees. On the other hand, we can expect that long-run relationships would be more homogeneous across countries.”
(Loayza et Rancière, 2006)

The results obtained using the PMG estimator preferred given the gains in consistency and efficiency over other panel error-correction estimators. Nevertheless, for comparison purposes, the tables also report the results from the MG and the DFE estimators. To choose among these three methods, the Hausman test provides a test of difference between these estimators. The null of the Hausman test is that the difference between PMG and MG or PMG and DFE estimations is not significant. If the null is not rejected, the PMG estimator is recommended since it is efficient. Finally, due to the relatively limited time dimension, the estimation is based on a lag structure of order one to limit the parameter burden, following Samargandi, Fidrmuc et Ghosh (2015). Table B.21 reports the estimates for this estimator.

Table B.21 – Short-run and long-run growth effects of financial depth, Pooled Mean Group ARDL estimates.

Sample :	FULL			ADVANCED	NON-ADV.
Estimator : PMG	(1)	(2)	(3)	(4)	(5)
LONG-RUN COEFFICIENTS					
Private credit/GDP	-1.900*** (0.466)		-2.792*** (0.469)	-3.334*** (0.553)	-2.589*** (0.973)
Public debt/GDP		-0.012 (0.181)	-0.142 (0.150)	-0.243 (0.195)	1.808*** (0.521)
Initial GDP/capita	-5.458*** (0.675)	-6.754*** (0.577)	-5.207*** (0.672)	-5.061*** (0.855)	-1.536 (1.098)
Government consumption	-1.349* (0.814)	-1.382* (0.785)	-1.717** (0.858)	-2.332** (1.055)	3.698** (1.521)
Inflation rate	-1.544*** (0.128)	-1.298*** (0.120)	-1.635*** (0.128)	-1.784*** (0.151)	-0.342 (0.230)
Trade openness	0.750 (0.529)	1.157** (0.510)	1.104** (0.534)	1.224** (0.622)	3.738*** (0.989)
ERROR CORRECTION COEFFICIENTS					
ψ	-0.714*** (0.051)	-0.692*** (0.052)	-0.722*** (0.056)	-0.817*** (0.070)	-0.637*** (0.074)
SHORT-RUN COEFFICIENTS					
Δ Private credit/GDP	-4.520*** (1.251)		-4.647*** (1.313)	-4.995*** (1.938)	-4.329** (2.316)
Δ Public debt/GDP		-0.103 (1.105)	2.471 (2.067)	1.377 (1.567)	5.249 (3.676)
Δ Initial GDP/capita	-15.19*** (3.074)	-11.48*** (2.883)	-8.383*** (2.965)	-18.23*** (3.695)	3.692 (8.951)
Δ Government consumption	-1.899 (2.918)	-2.955 (2.728)	-2.863 (3.173)	-4.390 (4.550)	1.686 (4.369)
Δ Inflation rate	-1.264*** (0.172)	-1.209*** (0.189)	-1.337*** (0.199)	-1.470*** (0.168)	-0.878** (0.361)
Δ Trade openness	-2.565** (1.115)	-2.602** (1.157)	-3.115*** (1.112)	-4.350*** (0.996)	0.059 (1.77)
Intercept	43.27*** (3.210)	49.33*** (3.944)	41.65*** (3.460)	49.63*** (4.317)	-4.716*** (0.849)
Hausman test (p -value)	0.65	0.25	0.32	0.13	0.59
Number of countries	34	34	34	18	16
Observations	1341	1434	1341	810	531

Notes : Estimations are based on the (xtpmg) routine by Blackburne et Frank (2007) for STATA. This table report Pooled mean group estimates. For mean group and dynamic fixed effects, see Tables B.22 and B.23. All estimates are controlling for country and time effects. All control variables are specified in natural logs. Under the null hypothesis of the Hausman test, PMG is more efficient estimation than MG. In the present table, the Hausman test is indicating that PMG is consistent and efficient. It should be preferred to MG and DFE estimations. The lag structure is ARDL(1, 1, 1, 1, 1). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.22 – Short-run and long-run growth effects of financial depth, Mean Group ARDL estimates.

Sample :	FULL		ADVANCED	NON-ADV.	
Estimator : MG	(1)	(2)	(3)	(4)	(5)
LONG-RUN COEFFICIENTS					
Private credit/GDP	-5.282** (2.099)		-6.252*** (2.311)	-6.114*** (2.291)	-6.407 (4.711)
Public debt/GDP		1.517* (0.876)	2.916* (1.745)	0.932* (0.557)	5.147 (3.634)
Initial GDP/capita)	-5.781*** (1.858)	-10.30*** (1.560)	-4.062 (2.971)	-7.863** (3.247)	0.212 (5.054)
Government consumption	8.436 (5.277)	5.286 (4.222)	6.311 (6.181)	4.297 (3.308)	8.578 (12.80)
Inflation rate	-1.087** (0.444)	-1.113*** (0.386)	-1.186** (0.467)	-1.688*** (0.215)	-0.622 (0.961)
Trade openness	5.162** (2.404)	2.952 (2.504)	2.856 (2.614)	0.498 (1.077)	5.510 (5.438)
ERROR CORRECTION COEFFICIENTS					
ψ	-0.961*** (0.050)	-0.904*** (0.053)	-1.049*** (0.067)	-0.967*** (0.078)	-1.142*** (0.110)
SHORT-RUN COEFFICIENTS					
Δ Private credit/GDP	-5.050*** (1.786)		-4.918** (2.183)	-3.016*** (2.929)	-7.058** (3.282)
Δ Public debt/GDP		-0.510 (1.371)	2.654 (2.895)	-1.314 (2.096)	7.120 (5.575)
Δ Initial GDP/capita)	-28.42*** (3.473)	-21.85*** (3.790)	-24.57*** (6.163)	-26.75*** (3.815)	-22.12* (12.58)
Δ Government consumption	-2.370 (3.560)	-1.330 (3.124)	-3.977 (4.128)	-4.316 (4.456)	-3.596 (7.371)
Δ Inflation rate	-1.021*** (0.233)	-1.192*** (0.226)	-1.301*** (0.320)	-1.403*** (0.223)	-1.186* (0.645)
Δ Trade openness	-0.921 (1.445)	-3.044** (1.320)	-2.450* (1.449)	-4.656*** (1.166)	0.031 (2.702)
Intercept	46.50*** (17.25)	79.45*** (14.26)	55.26 (36.24)	66.59** (26.55)	42.52 (72.26)
Hausman test (p -value)	0.65	0.25	0.32	0.13	0.59
Number of countries	34	34	34	18	16
Observations	1341	1434	1341	810	531

Notes : Estimations are based on the (xtpmg) routine by Blackburne et Frank (2007) for STATA. This table report Mean group estimates. For pooled mean group and dynamic fixed effects, see Tables B.21 and B.23. All estimates are controlling for country and time effects. All control variables are specified in natural logs. Under the null hypothesis of the Hausman test, PMG is more efficient estimation than MG. In the present table, the Hausman test is indicating that PMG is consistent and efficient. It should be preferred to MG and DFE estimations. The lag structure is ARDL(1, 1, 1, 1, 1). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.23 – Short-run and long-run growth effects of financial depth, dynamic fixed effect ARDL estimates.

Sample :	FULL			ADVANCED	NON-ADV.
Estimator : DFE	(1)	(2)	(3)	(4)	(5)
LONG-RUN COEFFICIENTS					
Private credit/GDP	-1.876*** (0.718)		-1.890*** (0.698)	-2.751** (1.176)	-0.538 (0.848)
Public debt/GDP		0.361 (0.229)	0.186 (0.227)	-0.103 (0.287)	1.249*** (0.408)
Initial GDP/capita)	-2.709*** (0.593)	-3.663*** (0.477)	-2.644*** (0.521)	-4.797*** (1.003)	-2.625*** (0.642)
Government consumption	-1.251 (0.944)	-1.476** (0.726)	-1.638 (1.069)	-2.516** (1.066)	0.495 (1.724)
Inflation rate	-0.864*** (0.217)	-0.566*** (0.157)	-0.858*** (0.227)	-1.630*** (0.228)	-0.615** (0.269)
Trade openness	1.897* (1.073)	1.957** (0.871)	1.818* (1.064)	1.151* (0.701)	1.767 (1.378)
ERROR CORRECTION COEFFICIENTS					
ψ	-0.728*** (0.039)	-0.732*** (0.042)	-0.725*** (0.039)	-0.667*** (0.058)	-0.795*** (0.058)
SHORT-RUN COEFFICIENTS					
Δ Private credit/GDP	-3.526** (1.752)		-3.345* (1.771)	-5.880*** (2.089)	-2.136 (2.230)
Δ Public debt/GDP		0.727 (0.489)	1.208*** (0.454)	1.342* (0.779)	0.861 (0.593)
Δ Initial GDP/capita)	-6.917** (3.051)	-4.649** (2.086)	-4.809 (3.120)	-13.60*** (4.154)	-1.874 (3.246)
Δ Government consumption	1.530 (2.430)	-0.684 (3.454)	1.938 (2.344)	1.900 (5.117)	4.552 (2.999)
Δ Inflation rate	-0.803*** (0.185)	-0.754*** (0.172)	-0.798*** (0.194)	-1.350*** (0.133)	-0.442* (0.226)
Δ Trade openness	-1.676 (1.076)	-1.205 (1.238)	-1.696 (1.073)	-4.577*** (0.904)	0.834 (1.381)
Intercept	19.32*** (6.431)	26.09*** (4.250)	19.83*** (5.982)	38.88*** (9.891)	15.99* (9.318)
Hausman test (p -value)	0.00	0.00	0.00	0.00	0.00
Number of countries	34	34	34	18	16
Observations	1341	1434	1341	810	531

Notes : Estimations are based on the (xtpmg) routine by Blackburne et Frank (2007) for STATA. This table report dynamic fixed effects estimates. For pooled mean group and mean group, see Tables B.21 and B.22. All estimates are controlling for country and time effects. All control variables are specified in natural logs. Under the null hypothesis of the Hausman test, PMG is more efficient estimation than MG. In the present table, the Hausman test is indicating that PMG is consistent and efficient. It should be preferred to MG and DFE estimations. The lag structure is ARDL(1, 1, 1, 1, 1, 1). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4 Private Credit and Public Debt Over The Business Cycles

Table B.24 – Dates of normal and financial crisis recession peaks.

AUS	Norm.	1961	1977	1982	2008		ISR	1965	1975	1979	1988	2000	2008		
	Fin.	1990						1983							
AUT	Norm.	1974	1977	1980	1992		ITA	1974	1992	2002					
	Fin.	2008						2008							
BEL	Norm.	1974	1980	1992	2012		JPN	1969	1973	1992	2001	2007			
	Fin.	2008						1997							
BRA	Norm.	1962	1998	2000	2002	2008	KOR	1979							
	Fin.	1980	1989					1997							
CAN	Norm.	1974	1981	1989	2008		MEX	1985	2000	2007	2012				
	Fin.							1981	1994						
CHE	Norm.	1974	1981	1994	2001	2011	MYS	1974	1984	2000	2008				
	Fin.	1990	2008					1997							
CHL	Norm.	1964	1998	2008			NLD	1960	1974	1980	2001	2011			
	Fin.	1974	1981					2008							
COL	Norm.						NOR	1981	2007	2012					
	Fin.	1981	1997					1987							
CZE	Norm.	1996	2008	2011			POL								
	Fin.							1990							
DEU	Norm.	1974	1981	1992	2002		PRT	1974	1983	1992	2002				
	Fin.	2008						2008							
DNK	Norm.	1962	1973	1979	1987	1992	2011	SWE	1976	2011					
	Fin.	2007						1990	2007						
ESP	Norm.	1974	1980	1992			THA	2008							
	Fin.	1978	2008					1996							
FIN	Norm.	1976	2008				TUR	1960	1988	1990	1993	2008			
	Fin.	1990						1998							
FRA	Norm.	1974	1992	2011			USA	1969	1973	1979	1981	1990	2000		
	Fin.	2008						2007							
GBR	Norm.	1979					ZAF	1971	1974	1981	1984	1989	1997	2008	2014
	Fin.	1973	1990	2007											
GRC	Norm.	1973	1979	1986	1992										
	Fin.	2008													
HUN	Norm.	2011													
	Fin.	1991	2008												
IND	Norm.	1973	1978												
	Fin.	1990													
IDN	Norm.	1961	1966	1981											
	Fin.	1997													

Table B.25 – Financial crisis predictive ability, Logit models. Dependent variable : Crisis dummy.

Classifier logit model	(1)	(2)	(3)	(4)	(5)
Log(Total credit/GDP) (five-year m.a.)	0.74*** (0.21)				
Log(Public debt/GDP) (five-year m.a.)		0.72 (0.39)			
Log(Private credit/GDP) (five-year m.a.)			1.26*** (0.32)		
Log(Firm credit/GDP) (five-year m.a.)				0.27 (0.80)	
Log(House credit/GDP) (five-year m.a.)					2.28** (0.81)
AUROC	0.67*** (0.03)	0.62 (0.04)	0.71*** (0.03)	0.61 (0.06)	0.67*** (0.03)
σ					
AUROC equality χ_2 test (p -value)					
$H_0 : (*) = \text{GOLD}$	0.06	0.90	0.00	0.13	0.00
$H_0 : (*) = (3)$	0.02	0.00	–	0.90	0.23
Observations	1,166	1,485	1,168	739	749
Country FE	YES	YES	YES	YES	YES

Notes : The table shows logit model classifiers where the dependent variable is the financial crisis event dummy, and the regressors are specified as a five-year moving average. The AUROC significance stars are based on the equality test with the GOLD standard model. The unrestricted null GOLD standard model with only fixed effects has AUROC = 0.568 (0.04). AUROC equality χ_2 tests are based on sample specific GOLD standard models. Country fixed effects in all models, not reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.26 – Local projections showing response of Public debt per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom and the public debt level at the start of the recession. Sample : 1970-2018.

Log public debt per capita (relative to year 0, ×100)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
NORMAL RECESSION					
Average path	7.93*** (1.58)	12.44*** (2.37)	17.96*** (2.54)	21.09*** (3.17)	21.33*** (3.29)
Private credit	-0.87* (0.50)	-0.96* (0.54)	-0.89 (0.62)	-0.62 (0.73)	-0.63 (0.80)
Public debt	-7.09 (6.10)	-12.68 (11.78)	-21.43 (12.93)	-25.65 (15.55)	-27.16 (16.43)
Public debt × private credit	-0.59 (1.15)	-0.81 (2.26)	-1.98 (2.66)	-3.42 (3.12)	-4.00 (3.20)
FINANCIAL CRISIS					
Average path	13.15*** (3.62)	22.82*** (5.01)	29.74*** (5.87)	34.03*** (5.86)	35.83*** (6.01)
Private credit	-0.01 (1.06)	-0.84 (1.72)	-0.70 (2.13)	1.38 (1.97)	2.32 (2.15)
Public debt	-25.32** (11.39)	-50.43** (20.45)	-56.26** (26.01)	-63.71** (27.67)	-69.41** (29.23)
Public debt × private credit	-2.13 (3.48)	-3.13 (5.07)	-2.80 (6.38)	-8.47 (5.87)	-12.68** (5.59)
<i>F</i> -test equality of coefficients (<i>p</i> -value) :					
Normal = Financial (average effect)	0.21	0.07	0.07	0.05	0.03
Normal = Financial (credit)	0.29	0.95	0.93	0.38	0.25
Normal = Financial (debt)	0.19	0.09	0.18	0.18	0.15
Normal = Financial (interactions)	0.64	0.64	0.90	0.43	0.18
Normal, debt effect = 0	0.38	0.16	0.06	0.13	0.19
Financial, debt effect = 0	0.06	0.02	0.04	0.00	0.00
Observations	87	87	87	87	87
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
<i>R</i> ²	0.59	0.61	0.68	0.70	0.70

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

5 Robustness Checks on the Local Projections

5.1 Evaluation Without the Great Financial Crisis

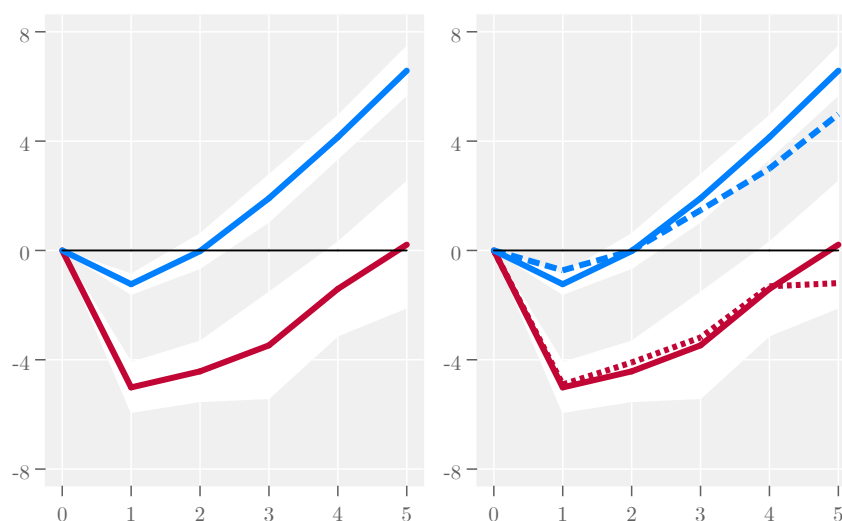


Figure B.6 – Local projection, private credit experiment. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the size of the preceding private credit surge and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The dashed and dotted lines refer to a private credit experiment when private credit during the expansion grew at the mean plus one standard deviation. The solid line refers to the average path, and hence replicates the average response reported in Table B.27, and the shaded region is a 90% confidence interval. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

Table B.27 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom. Sample : 1970-2006, without the subprime crisis.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.24*** (0.22)	-0.02 (0.38)	1.91*** (0.52)	4.14*** (0.48)	6.58*** (0.54)
Financial crisis (average effect)	-5.01*** (0.55)	-4.42*** (0.67)	-3.47*** (1.18)	-1.41 (1.04)	0.21 (1.41)
Private credit, Normal	0.12** (0.05)	0.00 (0.09)	-0.10 (0.14)	-0.26* (0.15)	-0.37** (0.17)
Private credit, Financial	0.04 (0.18)	0.11 (0.34)	0.09 (0.53)	0.03 (0.40)	-0.46 (0.65)
<i>F</i> -test equality of coefficients :					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (private credit) <i>p</i> -val.	0.65	0.79	0.74	0.52	0.88
Observations	56	56	56	56	56
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.86	0.67	0.55	0.71	0.78

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Robust standard deviation is in parentheses.

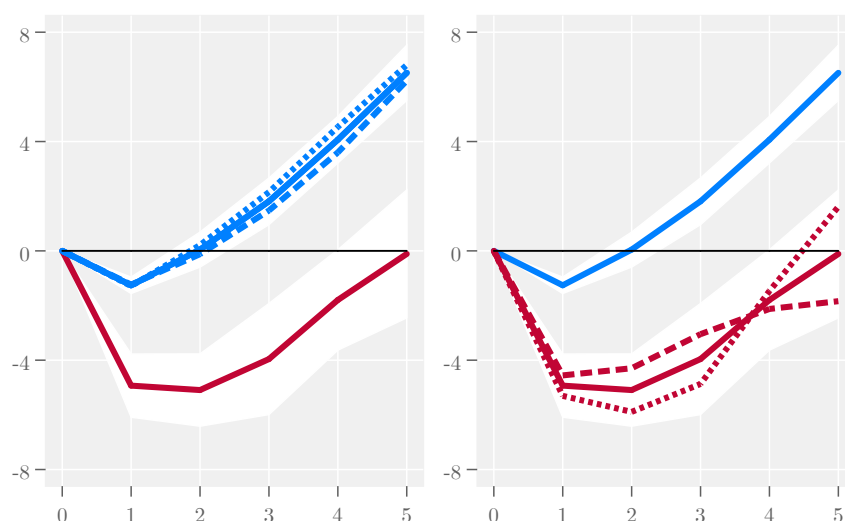


Figure B.7 – Local projections, public debt experiment. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the public debt level at the start of the recession and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The solid line refers to to public debt at the historical mean, and hence replicates the average response reported in Table B.28. The shaded region is a 90% confidence interval. The dotted lines correspond to public debt one standard deviation below mean, and the dashed lines to public debt one standard deviation above the mean. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

Table B.28 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the public debt level at the start of the recession. Sample : 1970-2006, without the subprime crisis.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.26*** (0.18)	0.05 (0.39)	1.81*** (0.52)	4.07*** (0.52)	6.51*** (0.61)
Financial crisis (average effect)	-4.93*** (0.70)	-5.09*** (0.81)	-3.96*** (1.24)	-1.80 (1.12)	-0.11 (1.43)
Public debt, Normal	0.10 (0.74)	-0.62 (1.38)	-1.25 (2.09)	-1.74 (2.07)	-1.07 (2.25)
Public debt, Financial	1.45 (1.95)	3.09 (2.53)	3.53 (3.99)	-1.27 (3.81)	-6.70 (6.50)
<i>F</i> -test equality of coefficients :					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (public debt) <i>p</i> -val.	0.55	0.20	0.24	0.90	0.39
Observations	60	60	60	60	60
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.82	0.67	0.49	0.64	0.74

Notes : Financial crisis and normal recession indicators are binary 0-1. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

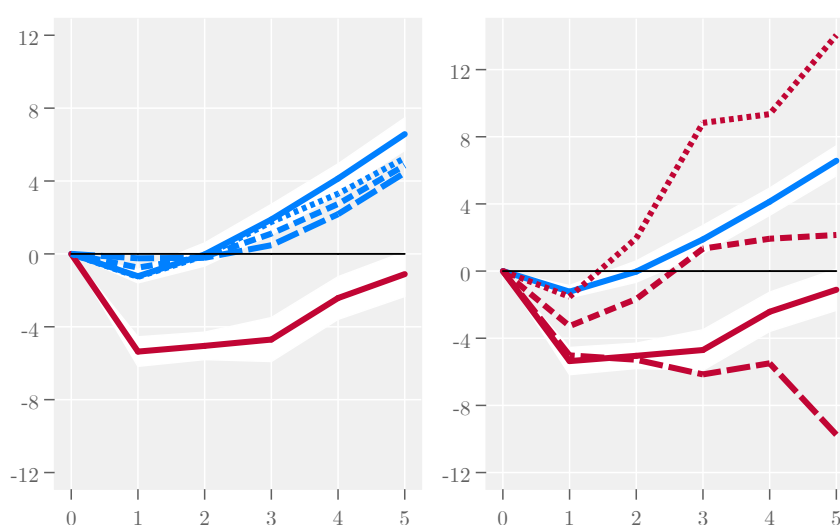


Figure B.8 – Local projections, private credit booms in time of low fiscal capacity. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the interaction of the size of the preceding private credit surge and the initial public debt level at the start of the recession and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The solid lines depict the average response in each type of recession with all variables at their mean, and hence replicates the average response reported in Table B.29. The shaded region is a 90% confidence interval. The various nonsolid lines indicate how the path of the economy differs with two simultaneous perturbations : when private credit grows at the average level plus one standard deviation in the previous expansion ; and, in addition, when public debt is set one standard deviation below mean (dotted line), at the mean (dashed line), or at one standard deviation above the mean (long-dashed line).

Table B.29 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom and the public debt level at the start of the recession. Sample : 1970-2006, without the subprime crisis.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
NORMAL RECESSION					
Average path	-1.23*** (0.22)	-0.03 (0.39)	1.89*** (0.51)	4.13*** (0.49)	6.57*** (0.54)
Private credit	0.11** (0.04)	-0.03 (0.10)	-0.18 (0.14)	-0.32** (0.13)	-0.39** (0.17)
Public debt	1.26 (1.15)	0.32 (1.92)	-0.67 (2.14)	-1.30 (2.09)	-1.09 (2.60)
Public debt \times private credit	0.14 (0.17)	-0.12 (0.30)	-0.38 (0.39)	-0.17 (0.41)	-0.09 (0.47)
FINANCIAL CRISIS					
Average path	-5.36*** (0.49)	-5.05*** (0.46)	-4.70*** (0.73)	-2.42*** (0.71)	-1.11 (0.75)
Private credit	0.68* (0.38)	1.11** (0.43)	1.98*** (0.62)	1.43** (0.55)	1.07* (0.54)
Public debt	-1.69 (2.49)	-4.98 (3.92)	-11.21*** (3.91)	-14.01*** (4.29)	-26.89*** (4.93)
Public debt \times private credit	-1.66* (0.84)	-2.95*** (0.82)	-5.82*** (1.10)	-4.82*** (1.26)	-6.27*** (1.32)
<i>F</i> -test equality of coefficients (<i>p</i> -value) :					
Normal = Financial (average effect)	0.00	0.00	0.00	0.00	0.00
Normal = Financial (private credit)	0.13	0.03	0.00	0.00	0.02
Normal = Financial (public debt)	0.36	0.18	0.01	0.01	0.00
Normal = Financial (interactions)	0.06	0.00	0.00	0.00	0.00
Observations	56	56	56	56	56
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.88	0.72	0.68	0.76	0.84

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

5.2 Evaluation Without Greece and Portugal

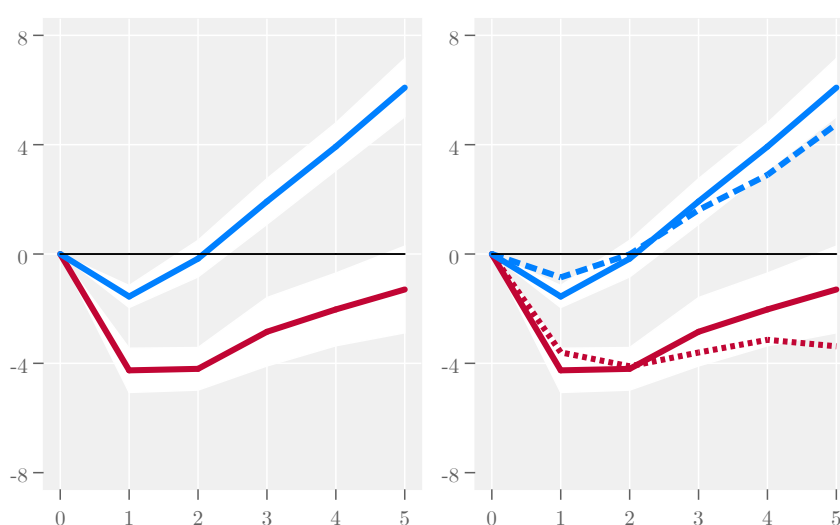


Figure B.9 – Local projection, private credit experiment without Greece and Portugal. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the size of the preceding private credit surge and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The dashed and dotted lines refer to a private credit experiment when private credit during the expansion grew at the mean plus one standard deviation. The solid line refers to the average path, and hence replicates the average response reported in Table B.30, and the shaded region is a 90% confidence interval. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

Table B.30 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom. Sample : 1970-2018, without Greece and Portugal.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.56*** (0.24)	-0.17 (0.41)	1.92*** (0.51)	3.93*** (0.53)	6.09*** (0.65)
Financial crisis (average effect)	-4.26*** (0.49)	-4.20*** (0.47)	-2.85*** (0.76)	-2.03** (0.81)	-1.30 (0.97)
Private credit, Normal	0.17** (0.06)	0.04 (0.12)	-0.08 (0.12)	-0.25 (0.18)	-0.33* (0.19)
Private credit, Financial	0.22 (0.14)	0.03 (0.18)	-0.25 (0.29)	-0.36 (0.33)	-0.68* (0.38)
<i>F</i> -test equality of coefficients :					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (private credit) <i>p</i> -val.	0.73	0.97	0.57	0.74	0.32
Observations	80	80	80	80	80
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.77	0.59	0.47	0.54	0.64

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Robust standard deviation is in parentheses.

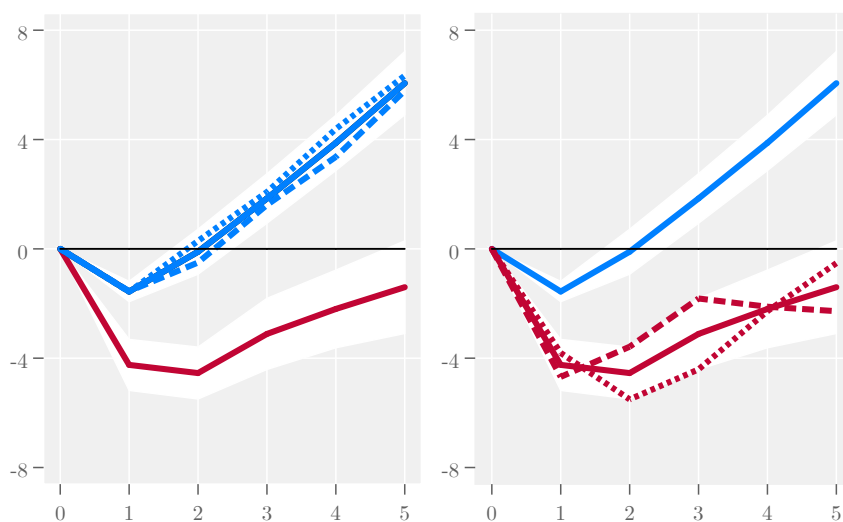


Figure B.10 – Local projections, public debt experiment, without Greece and Portugal. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the public debt level at the start of the recession and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The solid line refers to public debt at the historical mean, and hence replicates the average response reported in Table B.31. The shaded region is a 90% confidence interval. The dotted lines correspond to public debt one standard deviation below mean, and the dashed lines to public debt one standard deviation above the mean. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

Table B.31 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the public debt level at the start of the recession. Sample : 1970-2018, without Greece and Portugal.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.56*** (0.23)	-0.11 (0.50)	1.84*** (0.55)	3.88*** (0.62)	6.06*** (0.70)
Financial crisis (average effect)	-4.25*** (0.57)	-4.54*** (0.58)	-3.11*** (0.79)	-2.20** (0.87)	-1.40 (1.03)
Public debt, Normal	0.04 (0.71)	-1.22 (1.43)	-0.73 (1.02)	-1.58 (1.51)	-0.85 (1.84)
Public debt, Financial	-1.70 (2.07)	3.75 (2.40)	5.05 (3.50)	0.32 (3.73)	-3.42 (4.62)
<i>F</i> -test equality of coefficients :					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (public debt) <i>p</i> -val.	0.42	0.09	0.15	0.67	0.65
Observations	84	84	84	84	84
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.73	0.57	0.42	0.48	0.59

Notes : Financial crisis and normal recession indicators are binary 0-1. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

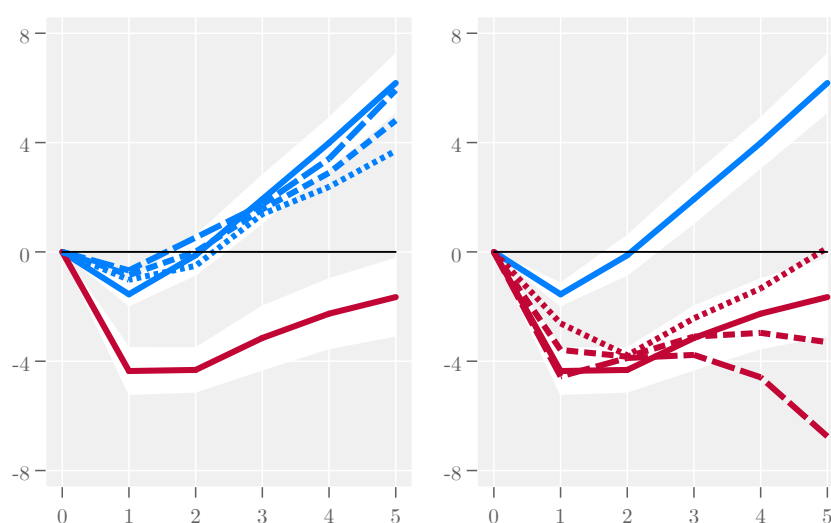


Figure B.11 – Local projections, private credit booms in time of low fiscal capacity, without Greece and Portugal. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the interaction of the size of the preceding private credit surge and the initial public debt level at the start of the recession and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The solid lines depict the average response in each type of recession with all variables at their mean, and hence replicates the average response reported in Table B.32. The shaded region is a 90% confidence interval. The various nonsolid lines indicate how the path of the economy differs with two simultaneous perturbations : when private credit grows at the average level plus one standard deviation in the previous expansion ; and, in addition, when public debt is set one standard deviation below mean (dotted line), at the mean (dashed line), or at one standard deviation above the mean (long-dashed line).

Table B.32 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom and the public debt level at the start of the recession. Sample : 1970-2018, without Greece and Portugal.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
NORMAL RECESSION					
Average path	-1.55*** (0.25)	-0.12 (0.44)	1.93*** (0.53)	3.99*** (0.56)	6.18*** (0.65)
Private credit	0.17** (0.07)	0.03 (0.12)	-0.09 (0.12)	-0.27 (0.16)	-0.34* (0.18)
Public debt	0.42 (0.93)	0.37 (1.88)	0.36 (1.77)	-0.02 (2.02)	1.01 (2.24)
Public debt \times private credit	0.02 (0.13)	0.29 (0.27)	0.04 (0.27)	0.39 (0.29)	0.58* (0.33)
FINANCIAL CRISIS					
Average path	-4.36*** (0.52)	-4.32*** (0.49)	-3.15*** (0.71)	-2.26*** (0.78)	-1.65* (0.86)
Private credit	0.25 (0.19)	0.16 (0.25)	0.02 (0.38)	-0.23 (0.48)	-0.54 (0.54)
Public debt	-2.05 (1.99)	1.88 (2.13)	2.71 (2.76)	-2.34 (3.02)	-7.40** (3.20)
Public debt \times private credit	-0.59 (0.60)	-0.68 (0.67)	-1.76 (1.06)	-1.35 (1.13)	-2.06 (1.30)
<i>F</i> -test equality of coefficients (<i>p</i> -value) :					
Normal = Financial (average effect)	0.00	0.00	0.00	0.00	0.00
Normal = Financial (private credit)	0.66	0.61	0.78	0.93	0.70
Normal = Financial (public debt)	0.27	0.55	0.44	0.51	0.04
Normal = Financial (interactions)	0.31	0.18	0.09	0.15	0.06
Observations	80	80	80	80	80
Macroeconomic controls	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES
R^2	0.78	0.61	0.50	0.56	0.67

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

5.3 Evaluation Without Right-Hand Side Variables

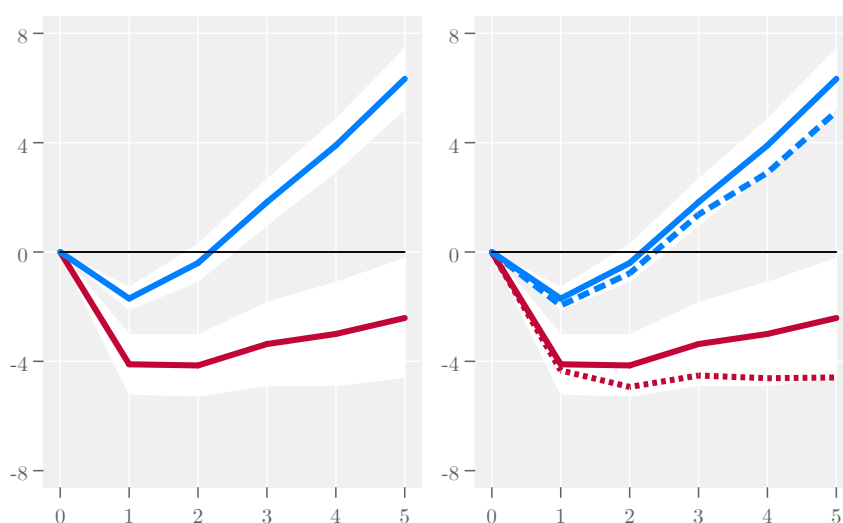


Figure B.12 – Local projection, private credit experiment. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the size of the preceding private credit surge and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The dashed and dotted lines refer to a private credit experiment when private credit during the expansion grew at the mean plus one standard deviation. The solid line refers to the average path, and hence replicates the average response reported in Table B.33, and the shaded region is a 90% confidence interval. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

Table B.33 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom. Sample : 1970-2018, without macroeconomic controls.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.70*** (0.24)	-0.40 (0.41)	1.83*** (0.51)	3.90*** (0.58)	6.34*** (0.67)
Financial crisis (average effect)	-4.11*** (0.66)	-4.15*** (0.68)	-3.37*** (0.92)	-3.00** (1.15)	-2.41* (1.32)
Private credit, Normal	-0.06 (0.06)	-0.09 (0.06)	-0.11 (0.07)	-0.24*** (0.09)	-0.29** (0.11)
Private credit, Financial	-0.07 (0.14)	-0.24* (0.14)	-0.36* (0.21)	-0.50** (0.23)	-0.68** (0.28)
<i>F</i> -test equality of coefficients :					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (private credit) <i>p</i> -val.	0.95	0.28	0.27	0.32	0.23
Observations	113	113	113	113	113
Macroeconomic controls	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES
R^2	0.56	0.34	0.28	0.41	0.54

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Robust standard deviation is in parentheses.

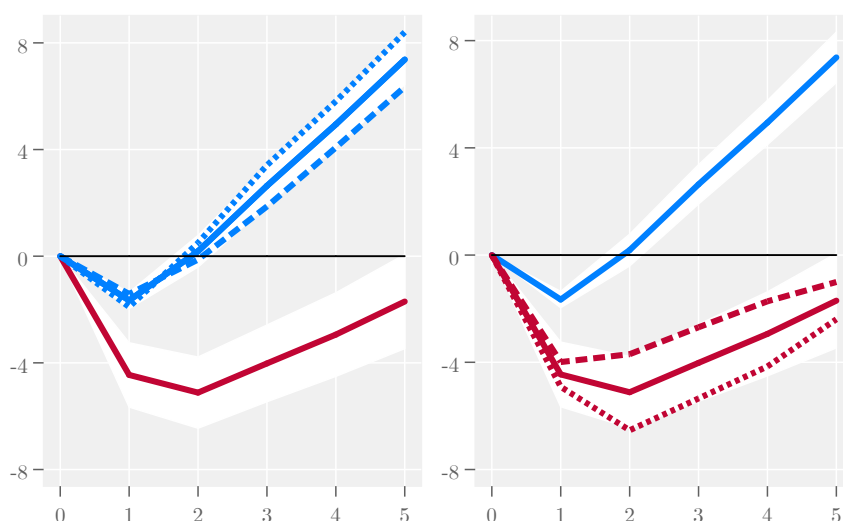


Figure B.13 – Local projections, public debt experiment, without macroeconomic controls. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the public debt level at the start of the recession and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The solid line refers to public debt at the historical mean, and hence replicates the average response reported in Table B.34. The shaded region is a 90% confidence interval. The dotted lines correspond to public debt one standard deviation below mean, and the dashed lines to public debt one standard deviation above the mean. The local projections are conditional on the full set of macroeconomic aggregates and their lags, with paths evaluated at the means.

Table B.34 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the public debt level at the start of the recession. Sample : 1970-2018, without macroeconomic controls.

Log real GDP per capita (relative to year 0, ×100)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
Normal recession (average effect)	-1.66*** (0.18)	0.18 (0.36)	2.64*** (0.45)	4.94*** (0.50)	7.37*** (0.57)
Financial crisis (average effect)	-4.45*** (0.73)	-5.12*** (0.81)	-4.02*** (0.88)	-2.94*** (0.95)	-1.70 (1.08)
Public debt, Normal	0.77** (0.33)	-0.99 (0.90)	-2.45*** (0.89)	-2.75** (1.10)	-3.26*** (1.10)
Public debt, Financial	1.08 (0.97)	3.30*** (1.14)	3.09* (1.81)	2.83 (2.98)	1.62 (3.68)
<i>F</i> -test equality of coefficients :					
Norm. = Fin. (average effect) <i>p</i> -val.	0.00	0.00	0.00	0.00	0.00
Norm. = Fin. (public debt) <i>p</i> -val.	0.77	0.01	0.01	0.08	0.23
Observations	153	153	153	153	153
Macroeconomic controls	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES
<i>R</i> ²	0.55	0.38	0.36	0.45	0.54

Notes : Financial crisis and normal recession indicators are binary 0-1. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

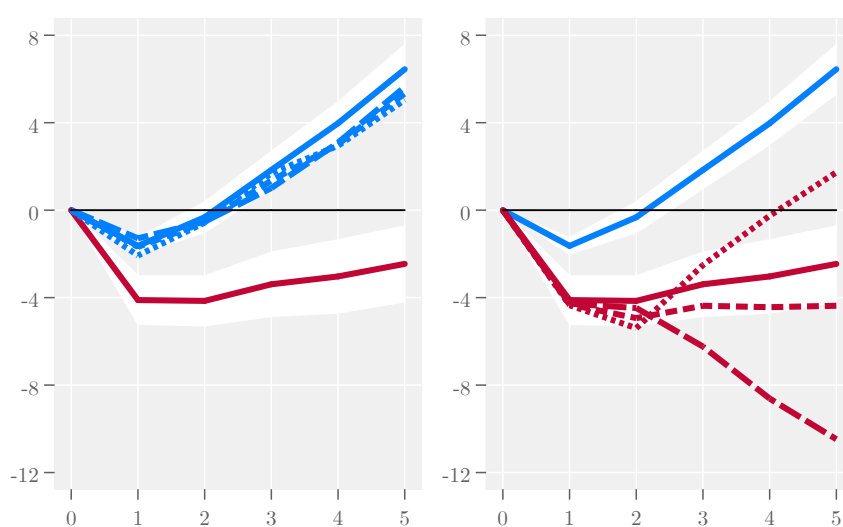


Figure B.14 – Local projections, private credit booms in time of low fiscal capacity, without macroeconomic controls. Conditional cumulative change from the start of the recession of real GDP per capita as a function of the interaction of the size of the preceding private credit surge and the initial public debt level at the start of the recession and the type of recession. The blue projections are associated with normal recessions, whereas the red ones are related to financial crises. The solid lines depict the average response in each type of recession with all variables at their mean, and hence replicates the average response reported in Table B.35. The shaded region is a 90% confidence interval. The various nonsolid lines indicate how the path of the economy differs with two simultaneous perturbations : when private credit grows at the average level plus one standard deviation in the previous expansion ; and, in addition, when public debt is set one standard deviation below mean (dotted line), at the mean (dashed line), or at one standard deviation above the mean (long-dashed line).

Table B.35 – Local projections showing response of real GDP per capita, for normal versus financial crisis recessions, allowing for the size of the preceding private credit boom and the public debt level at the start of the recession. Sample : 1970-2018, without macroeconomic controls.

Log real GDP per capita (relative to year 0, $\times 100$)	YEAR 1	YEAR 2	YEAR 3	YEAR 4	YEAR 5
NORMAL RECESSION					
Average path	-1.63*** (0.23)	-0.32 (0.43)	1.84*** (0.52)	3.97*** (0.59)	6.44*** (0.68)
Private credit	-0.01 (0.03)	-0.06 (0.06)	-0.12* (0.07)	-0.24*** (0.07)	-0.28*** (0.08)
Public debt	1.07* (0.55)	-0.63 (1.27)	-1.27 (1.54)	-1.22 (1.60)	-1.23 (1.98)
Public debt \times private credit	0.07 (0.09)	0.18 (0.18)	0.03 (0.22)	0.37 (0.23)	0.57* (0.33)
FINANCIAL CRISIS					
Average path	-4.11*** (0.67)	-4.15*** (0.69)	-3.39*** (0.89)	-3.03*** (1.02)	-2.46** (1.06)
Private credit	-0.06 (0.16)	-0.24 (0.15)	-0.31 (0.25)	-0.44 (0.30)	-0.60* (0.30)
Public debt	0.66 (2.26)	1.47 (3.24)	-2.16 (5.31)	-8.60 (6.33)	-13.76** (6.28)
Public debt \times private credit	-0.17 (0.57)	0.06 (0.59)	-1.44 (0.99)	-2.06* (1.13)	-2.63** (1.12)
<i>F</i> -test equality of coefficients (<i>p</i> -value) :					
Normal = Financial (average effect)	0.00	0.00	0.00	0.00	0.00
Normal = Financial (private credit)	0.75	0.22	0.48	0.52	0.31
Normal = Financial (public debt)	0.86	0.54	0.87	0.28	0.08
Normal = Financial (interactions)	0.70	0.85	0.15	0.04	0.01
Observations	112	112	112	112	112
Macroeconomic controls	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES
R^2	0.56	0.35	0.30	0.47	0.61

Notes : Financial crisis and normal recession indicators are binary 0-1. Changes in private credit percentage points change per year in the prior expansion. Public debt level is the ratio relative to GDP at the business cycle peak. Robust standard deviation is in parentheses.

6 Panel Vector Autoregression Estimates

I use a panel-data vector autoregression methodology to assess the robustness of the evidence provided by the Local Projections. This technique combines the mainstream VAR with the panel data approach, allowing for unobserved individual heterogeneity. I estimate the following first-order panel VAR with fixed effect as follow :

$$y_{it} = \Gamma_0 + \Gamma_1 y_{it-1} + f_i + d_{it} + e_t$$

where y_{it} is the growth rate of GDP per capita of country i at time t , f_i are fixed effects, and d_{it} some country-specific time dummies to account for financial crises and recessions. I also interact with these distress dummies, the private credit and public debt expansion before a crisis, and the initial level of government debt to reproduce the investigation from section 4. The PVAR structure imposes an exponential number of parameters to estimate, which is one of the reasons to favor Local Projections as they are less parameter intensive. The relatively small sample imposes a narrow specification. Following Love et Zicchino (2006), I specify a first-order PVAR with the smallest set of variables.

The PVAR estimates satisfy the stability condition. All the eigenvalues lie inside the unit circle. The stability implies that the PVAR has an infinite-order vector moving-average representation, providing the baseline for impulse-response functions (Lütkepohl, 2005). The impulse-responses are presented in Figure B.15 and B.16. Note, they do not provide a causal association. The confidence intervals are estimated using Monte Carlo simulation based on robust standard errors clustered at the country level.

6.1 Sub-samples

The first robustness exercise consists of focusing on subgroups of countries to evaluate the response of GDP growth to a normal recession and a financial crisis. Figure B.15 presents this exercise through four panels of two IRFs. For each sub-group, the panel provides the IRF for a normal recession next to the IRF for a financial crisis. Beyond the precisely estimated response, there is a high level of symmetries in response to

adverse shocks within the different samples. Each time, the response to a financial crisis is larger, conforing this chapter's conclusions. Also, the downturn is more pronounced in non-advanced economies than the advanced ones (for details on the samples, see Table B.2).

6.2 Various Scenarios

The second robustness exercise, presented in Figure B.16, focuses on the whole sample but provides some insight into the different scenarios from sections 4.3-4.5. The top-left panel provides IRFs for the full sample. The financial crisis response is almost two time larger than the one for normal recessions, underlining a slower recovery. Note that the reading of the IRFs is different from the Local Projection. Here, we observe deviations from GDP growth. Therefore, the evidence suggests that there is not a catch-up effect after an adverse shock. GDP levels are durably lower after a normal recession, and much lower after a financial crisis. The second step consists in investigating the impact of government debt surge before an adverse shock. The evidence from the top right panel is in line with previous evidence from this chapter and suggests that public debt does not trigger nor amplify an adverse shock.

On the contrary, evidence from the bottom left panel suggests that private debt build-ups fo foster larger financial crises, with a much larger response. Finally, the IRFs from the bottom right panel consider the scenario of private credit surge before a crisis with fiscal space. The evidence points to fiscal space enabling counter-cyclical response, dampening the adverse effect of the shock.

Overall, this PVAR robustness exercise supports the evidence presented in this chapter based on Local Projections. The LP technique is also suited to estimates impulse responses. It provides an analog to vector autoregression techniques while being more flexible and less parameter intensive. Indeed, LP is straightforward as it builds on simple regression techniques robust to misspecification, and accommodate experimentation with compliant specifications. Given the available sample, Local Projections should be favored.

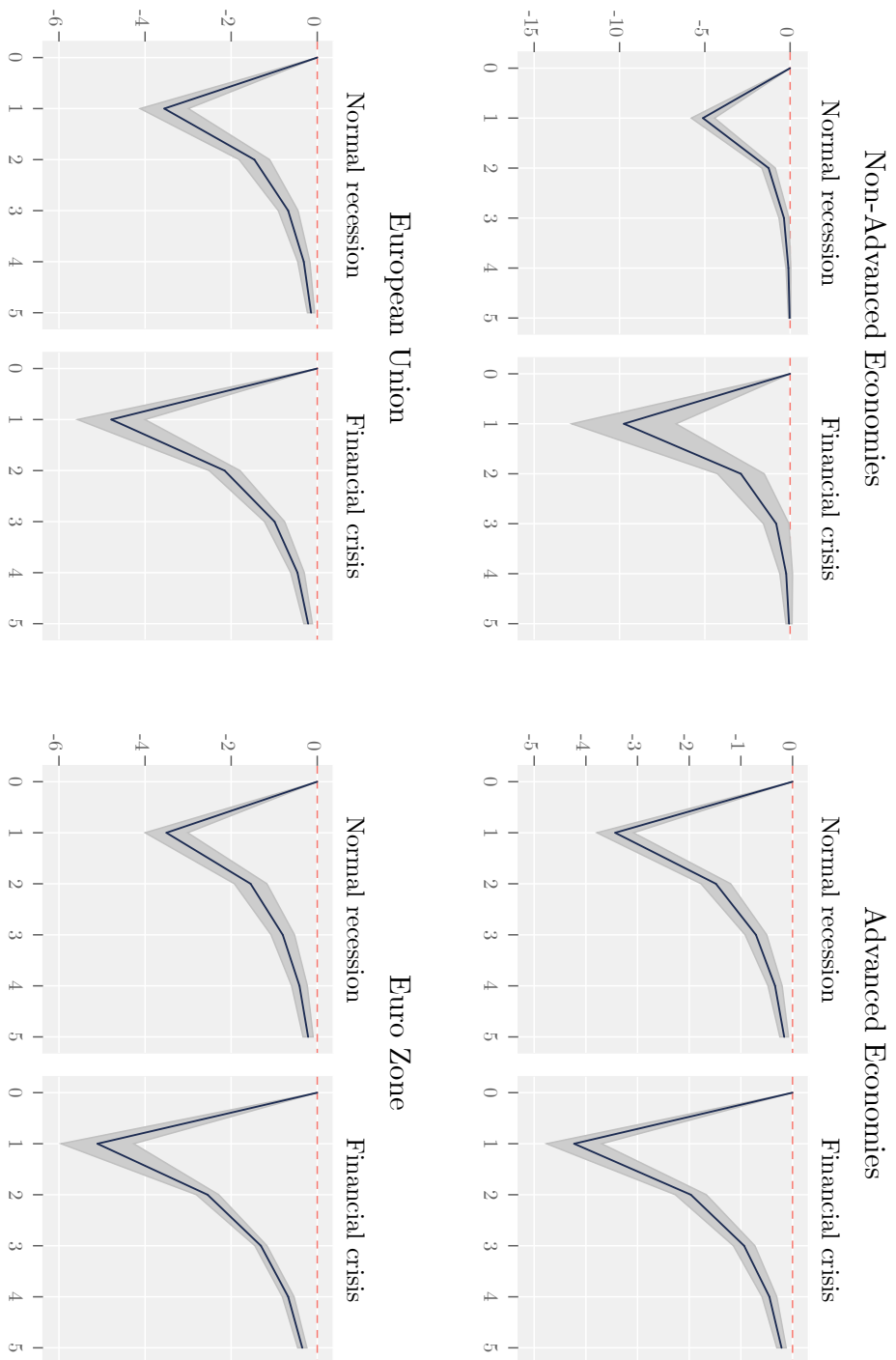


Figure B.15 – Panel Vector Autoregression Estimates : normal recessions vs. financial crises for different sub-samples. Impulse-Response Functions from the start of the recession of real GDP per capita as a function of the type of recession. The solid lines depict the IRF, the shaded region is a 90% confidence interval. All the eigenvalues lie inside the unit circle. The PVAR estimates satisfy the stability condition.

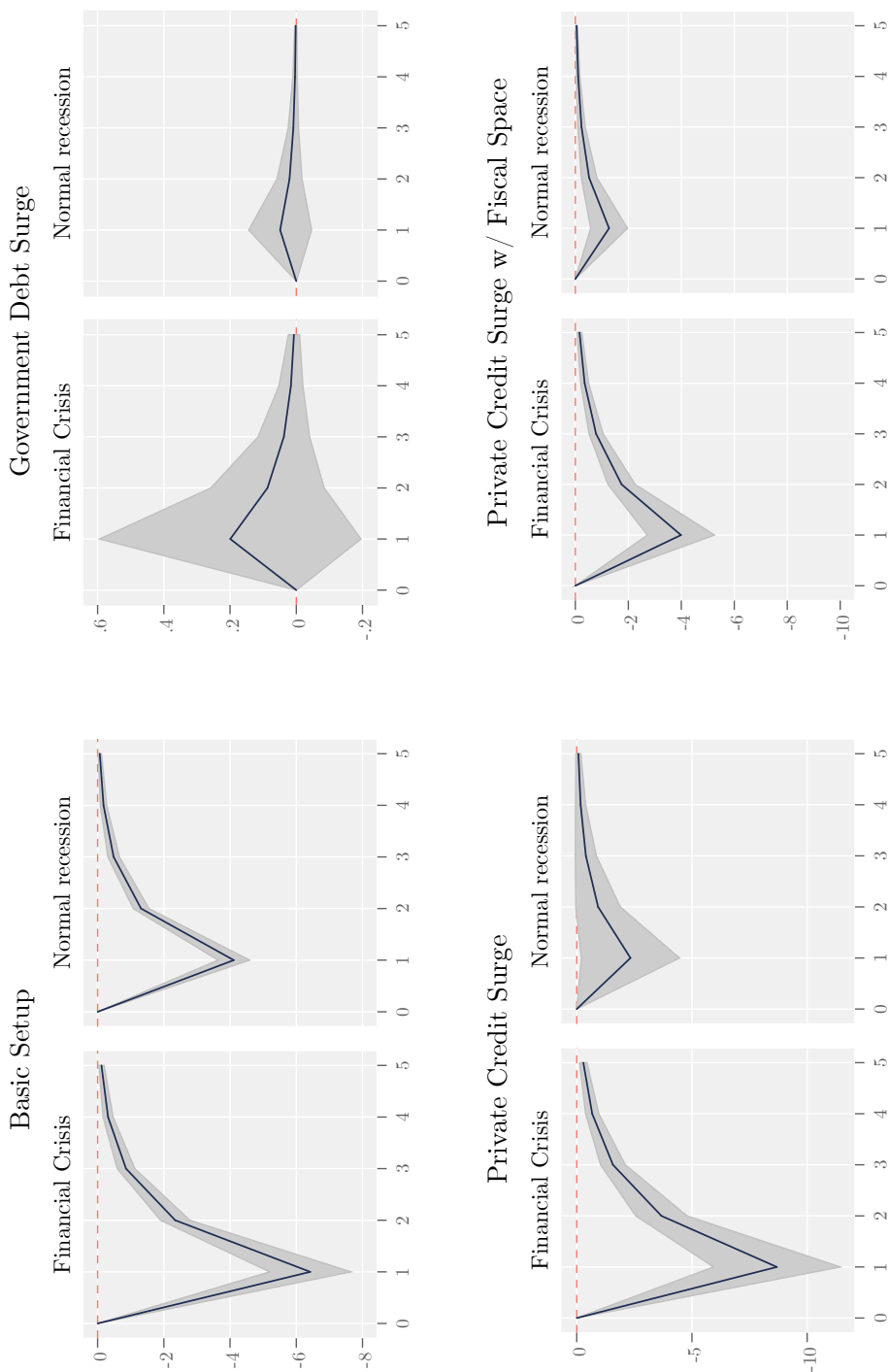


Figure B.16 – Panel Vector Autoregression Estimates : normal recessions vs. financial crises for different scenarios. Impulse-Response Functions from the start of the recession of real GDP per capita as a function of the interaction of the size of the preceding public/private credit surge and the initial public debt level at the start of the recession and the type of recession. The solid lines depict the IRF, the shaded region is a 90% confidence interval. All the eigenvalues lie inside the unit circle. The PVAR estimates satisfy the stability condition.

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1 Contrat sur emprunts structurés

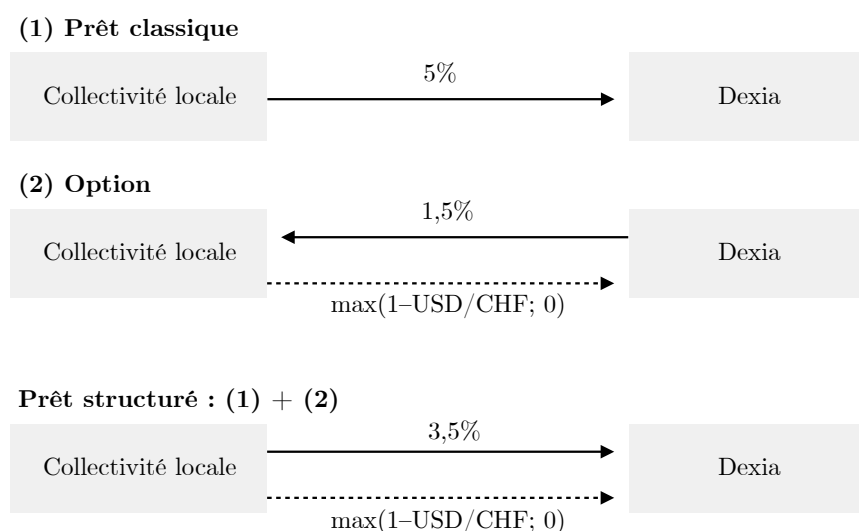


Figure C.1 – Décomposition d'un prêt structuré Dexia.

- Première phase (du 1^{er} septembre 2008 au 1^{er} octobre 2010) : le taux d'intérêt applicable est un taux fixe de 2,95 % l'an.

- Deuxième phase (du 1^{er} octobre 2010 au 1^{er} octobre 2023) : le taux d'intérêt est déterminé, de manière post fixée, successivement pour chaque période d'intérêts de douze mois précédant chaque date d'échéances d'intérêts, selon les modalités suivantes :

- Si le cours de change du dollar US en Yen est supérieur ou égal au cours pivot de 87 yens pour un dollar US, le taux d'intérêt appliqué au décompte des intérêts est égal au taux minimum constaté entre l'EURIBOR 12 mois et le taux fixe de 5,50 %, taux minoré d'une marge de 0,33 %.

- Si le cours de change du dollar US en Yen est strictement inférieur au cours pivot de 87 yen pour un dollar US, le taux d'intérêt appliqué au décompte des intérêts est égal à la somme :

d'une part du taux minimum constaté entre l'EURIBOR 12 mois et le taux fixe de 5,50 %, minoré d'une marge de 0,33 %,

d'autre part, de 26% du taux de variation du cours de change du dollar US en yen.

Figure C.2 – Contrat emprunt structuré Dexia.

2 Statistiques descriptives

Table C.1 – Statistiques descriptives pour l'ensemble des communes touchées par les emprunts toxiques Dexia.

COMMUNES TOUCHÉES PAR DEXIA	Moy.	σ	Obs.	Min	Max
Ratio de surcoût total	0.116	0.086	1542	-0.074	0.793
Dette par habitant (en milliers d'euros)	1.364	2.157	1542	0.011	43.60
Part Dexia dans l'endettement total	0.458	0.131	1542	0.010	1.000
Capacité de désendettement	9.887	38.73	1542	-271.8	1237.6
Coefficient de mobilisation du potentiel fiscal	1.123	0.303	1542	0.260	3.676
Taux d'équipement	0.256	0.182	1542	0.000	2.076
Taux d'endettement	0.945	0.464	1542	0.010	4.797
Taux d'autofinancement des nouveaux invest.	0.268	0.621	1542	-7.260	7.078
Taux de capacité d'autofinancement	0.145	0.083	1542	-0.326	0.694
Taux de rigidité des charges de structure	0.565	0.082	1542	0.159	0.844
Population	16050	35707	1542	110	859367
Niveau vie médian	20504	3339	1542	13061	37881
Taux de chômage	0.140	0.054	1542	0.000	0.383
Ratio de 15-64 ans	0.620	0.043	1542	0.433	0.810
Extrême gauche	0.043	0.204	1542	0.000	1.000
Gauche	0.318	0.466	1542	0.000	1.000
Sans étiquette et autres	0.049	0.215	1542	0.000	1.000
Droite	0.584	0.493	1542	0.000	1.000
Extrême droite	0.006	0.076	1542	0.000	1.000

Table C.2 – Statistiques descriptives pour l'ensemble des communes ayant reçu une aide du fonds de soutien des emprunts toxiques.

AIDE DU FONDS DE SOUTIEN : OUI	Moy.	σ	Obs.	Min	Max
Ratio de surcoût total	0.231	0.120	120	0.018	0.793
Dette par habitant (en milliers d'euros)	1.873	2.458	120	0.374	22.694
Part Dexia dans l'endettement total	0.570	0.294	120	0.041	1.226
Capacité de désendettement	12.12	28.37	120	-104.7	237.13
Coefficient de mobilisation du potentiel fiscal	1.110	0.246	120	0.521	1.713
Taux d'équipement	0.198	0.117	120	0.054	0.855
Taux d'endettement	1.106	0.398	120	0.273	2.442
Taux d'autofinancement des nouveaux invest.	0.160	0.435	120	-2.362	1.427
Taux de capacité d'autofinancement	0.108	0.072	120	-0.241	0.324
Taux de rigidité des charges de structure	0.599	0.097	120	0.230	0.844
Population	23846	30747	120	1225	173256
Niveau vie médian	20429	3447	120	14256	36194
Taux de chômage	0.148	0.057	120	0.026	0.332
Ratio de 15-64 ans	0.622	0.043	120	0.433	0.755
Extrême gauche	0.050	0.219	120	0.000	1.000
Gauche	0.283	0.453	120	0.000	1.000
Sans étiquette et autres	0.025	0.157	120	0.000	1.000
Droite	0.642	0.482	120	0.000	1.000
Extrême droite	0.000	0.000	120	0.000	0.000

Table C.3 – Statistiques descriptives pour l'ensemble des communes n'ayant pas reçu une aide du fonds de soutien des emprunts toxiques.

AIDE DU FONDS DE SOUTIEN : NON	Moy.	σ	Obs.	Min	Max
Ratio de surcoût total	0.106	0.075	1422	-0.074	0.700
Dette par habitant (en milliers d'euros)	1.321	2.125	1422	0.011	43.601
Part Dexia dans l'endettement total	0.448	0.137	1422	0.010	1.000
Capacité de désendettement	9.698	39.48	1422	-271.8	1237.6
Coefficient de mobilisation du potentiel fiscal	1.124	0.308	1422	0.260	3.676
Taux d'équipement	0.261	0.186	1422	0.000	2.076
Taux d'endettement	0.931	0.466	1422	0.010	4.797
Taux d'autofinancement des nouveaux invest.	0.277	0.633	1422	-7.260	7.078
Taux de capacité d'autofinancement	0.148	0.083	1422	-0.326	0.694
Taux de rigidité des charges de structure	0.562	0.080	1422	0.159	0.778
Population	15393	36027	1422	110	859367
Niveau vie médian	20511	3331	1422	13061	37881
Taux de chômage	0.139	0.053	1422	0.000	0.383
Ratio de 15-64 ans	0.620	0.043	1422	0.435	0.810
Extrême gauche	0.043	0.203	1422	0.000	1.000
Gauche	0.321	0.467	1422	0.000	1.000
Sans étiquette et autres	0.051	0.219	1422	0.000	1.000
Droite	0.579	0.494	1422	0.000	1.000
Extrême droite	0.006	0.079	1422	0.000	1.000

Table C.4 – Matrice de corrélation.

	Fonds	Ratio suretité	Detre/lab	Part Dexia	Cap. désend	Coef. Poffisc.	Taux equip.	Taux dette	CAF invest	Taux CAF	Rigidité	Ln(Pop)	Ln(Niv-vie)	clonage	ratio 15-64
Aide du fonds	1														
Ratio de suretité total	0.386***	1													
Detre par habitant	0.0815***	0.134***	1												
Part Dexia dans l'endettement total	-0.00312	-0.0000276	-0.0208***	1											
Capacité de désendettement	0.0140*	0.0165**	-0.00120	0.0151	1										
Coef. potentiel fiscal	-0.0159	0.0217*	0.267***	-0.00588	0.0113	1									
Taux d'équipement	-0.0639***	-0.0391***	0.0565***	0.0113	-0.0145*	-0.0734***	1								
Taux d'endettement	0.0974	0.144***	0.433***	-0.0521***	0.0234***	0.0558***	0.248***	1							
Taux autoch. hourv. invest.	-0.0460**	-0.0441***	-0.0836***	0.0297***	-0.0183**	0.0223*	-0.0069***	-0.224***	1						
Taux CAF	-0.0890**	-0.0948**	0.00829	0.0290*	-0.0263***	0.0105	0.271***	-0.0506***	0.484***	1					
Rigidité des charges de structure	0.113***	0.118***	-0.0601***	-0.0491***	0.0145*	0.00476	-0.189***	0.109***	-0.0837***	-0.204***	1				
Ln(Niveau de vie médian)	0.148***	0.0996***	-0.148***	-0.0567***	0.0108	-0.168***	-0.277***	-0.224***	-0.116***	-0.304***	0.289***	1			
Taux de clonage	-0.00872	-0.0247***	-0.00472	0.00306	-0.0119*	-0.217***	0.0692***	-0.0424***	0.0666***	0.0801***	-0.0955***	-0.131***	1		
Ratio de 15-64 ans	0.0445**	0.0498**	-0.121***	-0.0170**	0.0194***	0.0730***	-0.166***	-0.0819***	-0.0961***	-0.225***	0.175**	0.368***	0.232**	1	
	0.0165**	0.00107	0.0639***	0.0114	-0.000142	0.007222	-0.0299**	-0.0898***	0.00153	-0.0726***	-0.00827	0.214***	-0.235***	0.214***	1

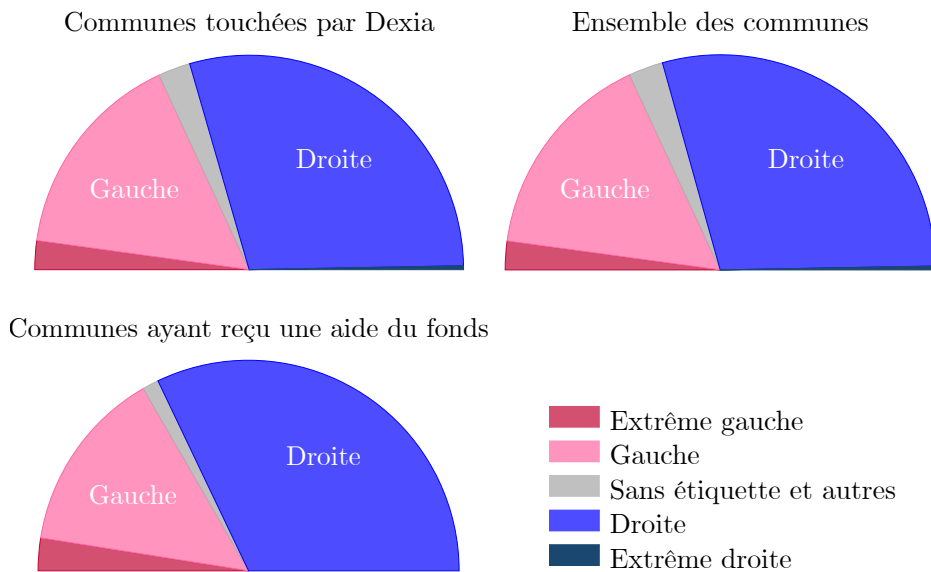
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3 Autres dimensions

3.1 L'absence de capture politique du fonds de soutien

Une littérature abondante a analysé l'influence de déterminants politiques dans l'octroi d'une aide par le gouvernement central (Sorribas-Navarro, 2011 ; Dietrichson et Ellegård, 2015 ; Farvaque, Héricourt et Joanis, 2019). Ainsi, le gouvernement aurait tendance à octroyer en priorité des aides aux communes politiquement alignées sur le parti au pouvoir. La capture politique de l'aide est d'autant plus probable que l'aide est discrétionnaire.

Néanmoins, le fonds de soutien pour la sortie des emprunts toxiques se fonde sur des critères financiers objectifs. En principe, l'octroi d'une aide n'est donc pas discrétionnaire. Khemani (2003) a cependant montré que les décideurs politiques peuvent manipuler une aide qui serait en principe fondée sur des critères objectifs.



La figure C.3 présente la composition des communes par famille politique pour (i) l'ensemble des communes, (ii) celles touchées par les emprunts structurés Dexia, et enfin (iii) celles ayant perçu une aide du fonds. Lorsque l'on considère les différents échantillons, nous constatons *a priori* que la dimension politique n'a pas été un critère d'octroi de l'aide.

Table C.5 – La dimension politique n'est pas un déterminant de l'aide.

Régressions Logit	(1)	(2)	(3)	(4)	(5)	
Autres dimensions comme variables de contrôle :	NON	OUI	NON	NON	OUI	Effets margin.
(a) Extrême gauche	0.977 (0.727)	-0.154 (0.838)				
(b) Gauche	0.698 (0.614)	0.029 (0.630)				
(c) Droite	0.927 (0.600)	0.274 (0.624)				
(d) Nuance politique			0.071 (0.096)	0.145 (0.112)	0.111 (0.114)	0.006 (0.006)
Ratio de surcoût total		9.606*** (1.016)		10.521*** (1.202)	9.614*** (1.010)	0.540*** (0.061)
Observations	1542	1542	1542	1542	1542	–
AUROC	0.540 (0.023)	0.873 (0.017)	0.540 (0.023)	0.861 (0.018)	0.872 (0.017)	–

Notes : La dernière colonne présente les estimations en intégrant l'ensemble des dimensions considérées. La table C.8 en annexe présente l'ensemble des coefficients pour cet exercice. Les variables *Extrême gauche*, *Gauche* et *Droite* sont des variables binaires prenant la valeur 1 si le maire de la commune a été élu sur une liste identifiée comme étant de cette famille politique. La variable *Sans étiquette et autres* est retirée pour éviter la colinéarité. De plus, la variable *Extrême droite* n'est pas mobilisée car elle ne représente qu'une infime proportion de communes dont aucune ne reçoit d'aide du fonds, ce qui empêche l'estimation par variable binaire. Elle est cependant insérée dans la variable *Nuance politique* qui est un continuum allant de -2 pour l'extrême gauche à $+2$ pour l'extrême droite, avec -1 pour la gauche, $+1$ pour la droite et 0 pour les sans étiquette et autres. Écart-types robustes entre parenthèses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

La table C.5 confirme ce point. Elle évalue non seulement la capacité prédictive, mais aussi l'effet marginal de l'appartenance d'une commune à une famille politique. Que ce soit en incluant une variable indicatrice pour chaque famille politique (moins une pour éviter toute colinéarité), ou une variable continue de nuance politique, les estimations suggèrent qu'il n'y a pas de capture politique de l'attribution d'une aide.

Ainsi, appartenir à une famille politique, et notamment celle de la majorité, n'accroît pas la probabilité d'obtenir une aide du fonds de soutien.

3.2 L'absence de motifs socio-économiques

Pour compléter l'analyse, nous évaluons l'incidence de critères socio-économiques sur la probabilité de bénéficier d'une aide. Ces derniers sont absents du décret et ne devraient donc pas être pris en considération pour l'octroi de cette aide. Nous vérifions donc qu'aucune logique de redistribution n'a guidé l'allocation du fonds de soutien.

La table C.6 souligne que ces critères socio-économiques complémentaires ne sont pas déterminants, mis à part un effet taille. En effet, en procédant comme pour les précédentes dimensions, les colonnes (1) à (4) intègrent progressivement les différentes variables de cette dimension. Que ce soit en intégrant par la suite le ratio de surcoût total (colonne (5)) ou même l'ensemble des autres dimensions comme variables de contrôle (colonne (6)), il apparaît très nettement que la richesse de la population, le dynamisme du bassin d'emplois ou encore la part de la population en âge de travailler n'ont aucune incidence sur la probabilité d'octroi d'une aide par le fonds de soutien.

Seule la taille de la population augmente les chances de bénéficier d'une aide. Ce résultat corrobore le constat préalable que les communes de plus de 5000 habitants sont plus représentées parmi celles bénéficiant d'une aide. Ainsi, au regard des effets marginaux moyens, doubler la taille de la commune augmente de $2pp$ la probabilité de percevoir une aide du fonds. Cet effet peut révéler un différentiel dans l'accès à l'information sur les constitutions de dossiers pour une demande d'aide auprès du fonds de soutien. Nous pouvons donc écarter toute intention de redistribution dans la gestion allocative du fonds.

Une fois encore, nous observons que le ratio de surcoût total est une variable importante. En effet, nous constatons que cette variable est statistiquement significative et positivement associée à la probabilité d'obtenir une aide. L'AUC passe de 0,67 pour l'estimation avec l'ensemble de la dimension socio-économique de la colonne (4), à 0,86 pour l'estimation de la colonne (5) intégrant le ratio de surcoût total supporté par la commune.

Table C.6 – Dimension socio-économique.

Régressions Logit	(1)	(2)	(3)	(4)	(5)	(6)	
Autres dimensions comme variables de contrôle :	NON	NON	NON	NON	NON	OUI	Effets margin.
(a) Log(Population)	0.471*** (0.076)	0.477*** (0.077)	0.470*** (0.080)	0.525*** (0.097)	0.507*** (0.105)	0.477*** (0.117)	0.027*** (0.007)
(b) Log(Niveau vie médian)		0.278 (0.584)	0.463 (0.931)	0.368 (0.931)	0.221 (1.068)	0.097 (1.203)	0.005 (0.068)
(b) Taux de chômage			0.745 (3.298)	-0.307 (3.428)	-1.470 (3.600)	-1.749 (3.860)	-0.098 (0.217)
(c) Part de 15-64 ans dans la population totale				-3.201 (2.724)	-1.515 (2.868)	-1.342 (2.974)	-0.075 (0.167)
Ratio de surcoût total					10.164*** (1.137)	9.614*** (1.010)	0.540*** (0.061)
Observations	1542	1542	1542	1542	1542	1542	–
AUROC	0.666 (0.024)	0.664 (0.024)	0.665 (0.024)	0.670 (0.024)	0.861 (0.018)	0.872 (0.017)	–

Notes : La dernière colonne présente les estimations en intégrant l'ensemble des dimensions considérées. La table C.8 en annexe présente l'ensemble des coefficients pour cet exercice. Écart-types robustes entre parenthèses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4 La prégnance de la stratégie d'évitement du conflit

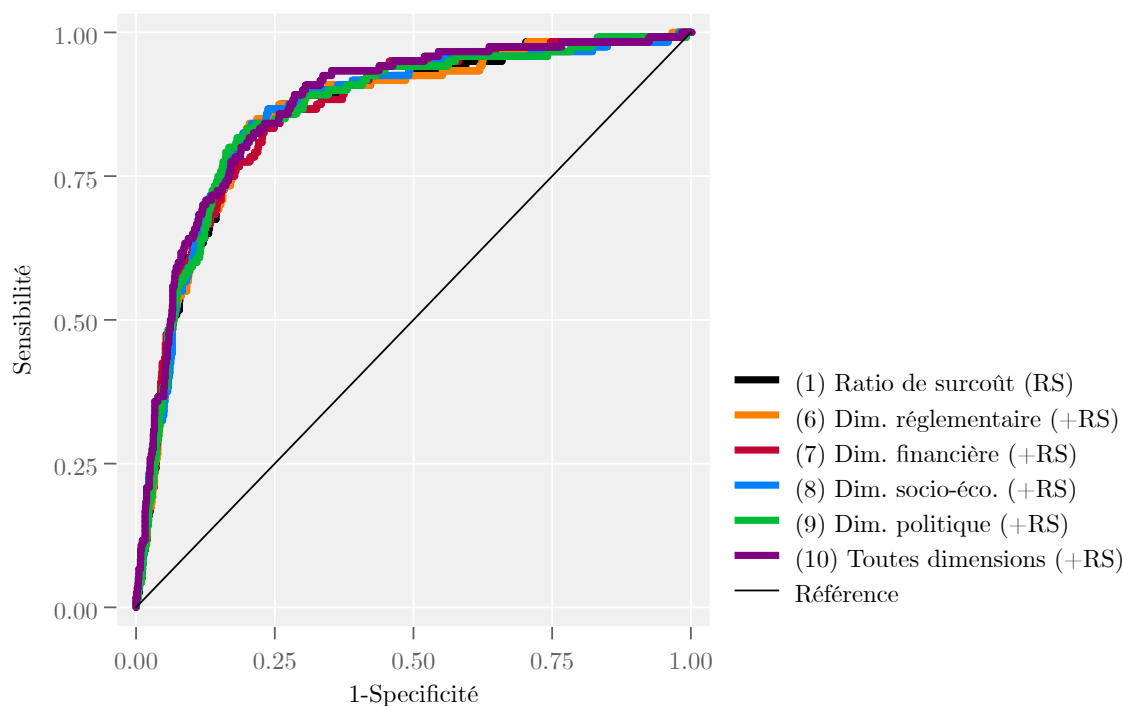


Figure C.4 – Receiving Operator Curves : les différentes dimensions n'améliorent pas la capacité prédictive du ratio de surcoût. Le modèle (1) correspond à une régression logit comportant une seule variable explicative pour le bénéfice du fonds de soutien : le niveau de ratio de surcoût des emprunts Dexia de la commune. Le modèle (6) est une régression logit avec pour variables explicatives l'ensemble des quatre variables réglementaires, augmenté du ratio de surcoût. Ce modèle reprend la régression présentée à la table III.2, colonne (5). Le modèle (7), présenté dans la colonne 6 de la table III.3, se concentre sur les autres dimensions financières pertinentes, augmentées du ratio de surcoût. Le modèle (8) évalue l'importance de la dimension socio-économique, augmentée du ratio de surcoût. Il est présenté dans la colonne (5) de la table C.6. Le modèle (9) présente la dimension politique augmentée du ratio de surcoût, visible dans la colonne (4) de la table C.5. Enfin, le modèle (10) intègre l'ensemble des dimensions, y compris le ratio de surcoût total. Les courbes ROC de ces différents modèles sont représentées dans la figure ci-dessus.

Table C.7 – AUROC pour différents modèles.

		AUROC	σ	IC (95%)	$H_0 : (1) = (*)$ p -value
(1)	Ratio de surcoût (RS)	0.859	0.018	[0.824 ; 0.894]	–
(6)	(RS) + Dimension réglementaire	0.858	0.018	[0.823 ; 0.893]	0.60
(7)	(RS) + Dimension financière	0.861	0.017	[0.827 ; 0.895]	0.84
(8)	(RS) + Dimension socio-éco.	0.861	0.018	[0.825 ; 0.897]	0.82
(9)	(RS) + Dimension politique	0.861	0.018	[0.826 ; 0.896]	0.61
(10)	(RS) + Ensemble des dimensions	0.872	0.017	[0.839 ; 0.905]	0.25

Notes : Le modèle (1) correspond à une régression logit comportant une seule variable explicative pour le bénéfice du fonds de soutien : le niveau de ratio de surcoût des emprunts Dexia de la commune. Le modèle (6) est une régression logit avec pour variables explicatives l'ensemble des quatre variables réglementaires, augmenté du ratio de surcoût. Ce modèle reprend la régression présentée à la table III.2, colonne (5). Le modèle (7), présenté dans la colonne 6 de la table III.3, se concentre sur les autres dimensions financières pertinentes, augmentées du ratio de surcoût. Le modèle (8) évalue l'importance de la dimension socio-économique, augmentée du ratio de surcoût. Il est présenté dans la colonne (5) de la table C.6. Le modèle (9) présente la dimension politique augmentée du ratio de surcoût, visible dans la colonne (4) de la table C.5. Enfin, le modèle (10) intègre l'ensemble des dimensions, y compris le ratio de surcoût total. Les courbes ROC de ces différents modèles sont représentées dans la figure C.7.

5 **Évaluations complémentaires sur la robustesse des estimations**

Nous reportons ici trois exercices complémentaires pour tester la robustesse des estimations conduites dans le chapitre 3. Tout d'abord, la section 5.1 présente les tables complètes avec l'ensemble des variables des différentes dimensions, intégrées progressivement. Nous contrôlons également la robustesse des estimations pour plusieurs années. Ensuite, la section 5.2 propose une discussion sur des méthodes d'estimations alternatives au logit proposé dans le papier. Par ailleurs, la section 5.3 étudie un potentiel effet d'interaction entre le taux d'endettement par habitant de la commune et le ratio de surcoût que celle-ci supporte. La section 5.4 propose enfin d'étudier les caractéristiques des communes qui bénéficient d'une aide du fonds par le Bayesian Model Averaging.

Ces différents tests de robustesse corroborent qualitativement les estimations présentées dans le chapitre 3, et soulignent la prégnance du ratio de surcoût dans le bénéfice d'une aide du fonds, sans que la santé financière de la commune n'apparaisse comme un élément déterminant.

5.1 Estimations sur l'ensemble des dimensions

Pour des raisons de lisibilité et d'interprétabilité, nous avons choisi de présenter plusieurs régressions avec des ensembles alternatifs de variables. Chaque dimension présentée dans la section 4 du dernier chapitre a fait l'objet d'une estimation pour tester nos hypothèses. Par souci de transparence et de clarté, la dernière colonne de chacune des tables de résultats présente les estimations de la dimension étudiée assortie des autres dimensions comme variables de contrôle. Les tables complètes pour cet exercice, avec inclusion progressive des variables, sont présentées ici.

Les variables explicatives sont mesurées en 2014, qui correspond à la date de dépôt des dossiers. Cependant, il est envisageable que les communes et donc les évaluateurs des dossiers de demande de fonds n'aient pas bénéficié de données stabilisées pour l'année 2014 sur la santé financière des communes. Si on se réfère aux données de la direction des collectivités locales, les données sont en général stabilisées avec un décalage de deux années. Or, on peut imaginer qu'étant face à une situation financière critique, les données ont pu fluctuer d'une année à l'autre sur cette période. Ainsi, nous avons estimé notre modèle sur les données de 2013 et 2012, constituant ainsi un test de robustesse complémentaire.

Les résultats sont qualitativement similaires, soulignant la relative inertie dans les finances publiques locales. Ces résultats sont présentés dans les Tables C.9 et C.10.

Table C.8 – Régressions Logit sur l'ensemble des dimensions étudiées, année de référence : 2014.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	
														Estim.	Margin	
Taux d'équipement	-3.228*** (0.906)	-3.289*** (0.868)	-3.350*** (0.887)	-2.227** (0.838)	-2.008* (0.856)	-1.906* (0.856)	-1.857* (0.849)	-1.881* (0.854)	-2.015* (0.903)	-2.380* (1.118)	-2.383* (1.115)	-2.378* (1.122)	-2.290* (1.125)	-2.287* (1.123)	-2.704* (1.279)	-0.152* (0.072)
Taux d'endettement		0.711*** (0.157)	0.673*** (0.165)	0.831*** (0.180)	0.752*** (0.193)	0.660** (0.214)	0.667** (0.215)	0.686** (0.216)	0.679** (0.226)	0.981*** (0.230)	0.988*** (0.234)	0.988*** (0.235)	0.957*** (0.233)	0.954*** (0.231)	0.819*** (0.237)	0.046*** (0.014)
Taux d'autofinancement des nouveaux investissements			-0.180 (0.101)	0.204 (0.114)	0.184 (0.125)	0.216 (0.132)	0.198 (0.131)	0.194 (0.133)	0.223 (0.136)	0.271* (0.136)	0.265* (0.135)	0.266* (0.135)	0.283* (0.136)	0.286* (0.136)	0.124 (0.151)	0.007 (0.008)
Taux de capacité d'autofinancement			-6.775*** (1.474)		-6.061*** (1.461)	-6.248*** (1.471)	-6.288*** (1.462)	-6.415*** (1.460)	-6.453*** (1.466)	-5.058*** (1.628)	-5.005*** (1.624)	-4.988*** (1.633)	-5.205*** (1.645)	-5.308*** (1.656)	-4.209* (1.894)	-0.236* (0.106)
Taux de rigidité des charges de structure					3.404* (1.662)	3.580* (1.630)	3.660* (1.633)	3.792* (1.670)	3.828* (1.717)	1.904 (1.843)	1.919 (1.828)	1.919 (1.835)	2.048 (1.909)	2.110 (1.891)	1.563 (1.923)	0.088 (0.107)
Dette par habitant					0.034 (0.050)	0.035 (0.052)	0.035 (0.052)	0.035 (0.053)	0.049 (0.069)	0.050 (0.054)	0.047 (0.053)	0.048 (0.053)	0.058 (0.049)	0.056 (0.049)	0.023 (0.035)	0.001 (0.002)
Part Dexia dans l'endettement total						0.076** (0.029)	0.077** (0.029)	0.077** (0.029)	0.073* (0.029)	0.140*** (0.033)	0.143*** (0.033)	0.143*** (0.033)	0.143*** (0.036)	0.142*** (0.036)	0.110*** (0.039)	0.006** (0.002)
Capacité de désendettement							-0.002 (0.002)	-0.002 (0.002)	-0.003 (0.002)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.002 (0.002)	-0.000 (0.000)
Coefficient de mobilisation du potentiel fiscal							-0.454 (0.344)	-0.454 (0.344)	-0.454 (0.344)	-0.263 (0.356)	-0.178 (0.359)	-0.175 (0.358)	-0.168 (0.363)	-0.154 (0.365)	-0.198 (0.395)	-0.011 (0.022)
ln(Population)										0.484*** (0.084)	0.497*** (0.086)	0.493*** (0.090)	0.550*** (0.101)	0.539*** (0.103)	0.477*** (0.117)	0.027*** (0.007)
ln(Niveau vie médian)											0.497 (0.614)	0.627 (1.008)	0.531 (1.011)	0.430 (1.035)	0.097 (1.203)	0.005 (0.068)
Taux de chômage												0.516 (3.485)	-0.658 (3.646)	-0.653 (3.643)	-1.749 (3.860)	-0.098 (0.217)
Part de 15-64 ans dans la population totale													-3.711 (2.808)	-3.410 (2.912)	-1.342 (2.974)	-0.075 (0.167)
Nuance politique															0.061 (0.103)	0.006 (0.006)
Ratio de surcoût total															9.614*** (1.010)	0.540*** (0.061)
Observations	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542
AUROC	0.621 (0.024)	0.678 (0.024)	0.687 (0.024)	0.715 (0.023)	0.716 (0.024)	0.719 (0.024)	0.722 (0.023)	0.723 (0.023)	0.728 (0.023)	0.759 (0.022)	0.758 (0.022)	0.758 (0.022)	0.761 (0.022)	0.760 (0.022)	0.872 (0.017)	0.872 (0.017)

Table C.9 – Régressions Logit sur l'ensemble des dimensions étudiées, année de référence : 2013.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	Estim.	Margin
Taux d'équipement	-2.168** (0.676)	-2.314*** (0.648)	-2.380*** (0.658)	-1.294* (0.570)	-1.153* (0.577)	-1.013 (0.579)	-1.010 (0.575)	-1.012 (0.576)	-1.130 (0.605)	-1.209 (0.745)	-1.227 (0.748)	-1.234 (0.750)	-1.140 (0.754)	-1.097 (0.750)	-1.320 (0.894)	-0.075 (0.051)
Taux d'endettement		0.673*** (0.152)	0.642*** (0.156)	0.812*** (0.154)	0.762*** (0.157)	0.551** (0.188)	0.553** (0.188)	0.558** (0.188)	0.540** (0.191)	0.924*** (0.280)	0.933*** (0.228)	0.934*** (0.230)	0.896** (0.230)	0.876** (0.231)	0.756** (0.258)	0.043** (0.015)
Taux d'autofinancement des nouveaux investissements		-0.239 (0.149)	0.601** (0.210)	0.604** (0.215)	0.666** (0.221)	0.648** (0.220)	0.648** (0.220)	0.648** (0.220)	0.665** (0.220)	0.840** (0.267)	0.821** (0.268)	0.818** (0.269)	0.844** (0.277)	0.852** (0.278)	0.571 (0.322)	0.032 (0.018)
Taux de capacité d'autofinancement		-8.048*** (1.749)	-7.232*** (1.773)	-7.442*** (1.752)	-7.442*** (1.751)	-7.423*** (1.751)	-7.444*** (1.748)	-7.511*** (1.749)	-6.530** (1.985)	-6.459** (1.979)	-6.398** (2.008)	-6.751** (2.057)	-7.011*** (2.074)	-5.501* (2.238)	-0.312* (0.127)	-0.114 (0.099)
Taux de rigidité des charges de structure		3.198* (1.400)	3.868** (1.374)	3.915** (1.380)	3.936** (1.387)	3.975** (1.393)	1.857 (1.518)	1.897 (1.505)	1.907 (1.520)	1.924 (1.555)	2.216 (1.574)	2.006 (1.754)	2.006 (1.754)	2.006 (1.754)	2.006 (1.754)	0.114 (0.099)
Dettes par habitant		0.079 (0.060)	0.079 (0.060)	0.079 (0.060)	0.079 (0.060)	0.079 (0.060)	0.079 (0.060)	0.079 (0.060)	0.097 (0.084)	0.078 (0.104)	0.075 (0.100)	0.077 (0.102)	0.087 (0.095)	0.085 (0.092)	0.057 (0.047)	0.003 (0.003)
Part Dexia dans l'endettement total		0.020 (0.013)	0.021 (0.013)	0.020 (0.013)	0.020 (0.013)	0.020 (0.013)	0.021 (0.013)	0.021 (0.013)	0.020 (0.013)	0.048** (0.013)	0.049** (0.013)	0.049** (0.013)	0.051** (0.013)	0.051** (0.013)	0.046** (0.014)	0.003** (0.001)
Capacité de désendettement		-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.000 (0.002)	-0.000 (0.000)
Coefficient de mobilisation du potentiel fiscal		-0.514 (0.332)	-0.249 (0.354)	-0.157 (0.356)	-0.154 (0.354)	-0.131 (0.356)	-0.112 (0.354)	-0.186 (0.368)	-0.112 (0.354)	-0.249 (0.354)	-0.157 (0.356)	-0.154 (0.354)	-0.131 (0.356)	-0.112 (0.354)	-0.186 (0.368)	-0.011 (0.021)
ln(Population)		0.482*** (0.083)	0.496*** (0.085)	0.489*** (0.088)	0.550*** (0.100)	0.535*** (0.102)	0.485*** (0.115)	0.485*** (0.115)	0.482*** (0.083)	0.496*** (0.085)	0.489*** (0.088)	0.550*** (0.100)	0.535*** (0.102)	0.485*** (0.115)	0.485*** (0.115)	0.028** (0.007)
ln(Niveau vie médian)		0.525 (0.609)	0.730 (0.972)	0.588 (0.985)	0.373 (1.011)	0.273 (1.157)	0.015 (0.066)	0.015 (0.066)	0.525 (0.609)	0.730 (0.972)	0.588 (0.985)	0.373 (1.011)	0.273 (1.157)	0.015 (0.066)	0.015 (0.066)	0.015 (0.066)
Taux de chômage		0.823 (3.414)	-0.559 (3.653)	-0.588 (3.653)	-1.169 (3.762)	-0.066 (3.214)	-0.066 (3.214)	-0.066 (3.214)	0.823 (3.414)	-0.559 (3.653)	-0.588 (3.653)	-1.169 (3.762)	-0.066 (3.214)	-0.066 (3.214)	-0.066 (3.214)	-0.066 (3.214)
Part de 15-64 ans dans la population totale		-3.862 (2.731)	-3.429 (2.741)	-1.668 (2.773)	-1.668 (2.773)	-1.668 (2.773)	-1.668 (2.773)	-1.668 (2.773)	-3.862 (2.731)	-3.429 (2.741)	-1.668 (2.773)	-1.668 (2.773)	-1.668 (2.773)	-1.668 (2.773)	-1.668 (2.773)	-0.095 (0.157)
Nuance politique		0.107 (0.097)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.107 (0.097)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.124 (0.112)	0.007 (0.006)
Ratio de surecôt total		9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	9.675*** (1.005)	0.549*** (0.062)
Observations	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539	1539
AUROC	0.590 (0.025)	0.650 (0.024)	0.655 (0.024)	0.686 (0.024)	0.692 (0.024)	0.701 (0.024)	0.701 (0.024)	0.701 (0.024)	0.707 (0.024)	0.742 (0.022)	0.742 (0.022)	0.742 (0.022)	0.745 (0.022)	0.747 (0.022)	0.868 (0.017)	0.868 (0.017)

Table C.10 – Régressions Logit sur l'ensemble des dimensions étudiées, année de référence : 2012.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	Margin	
Taux d'équipement	-2.807** (0.888)	-3.042*** (0.869)	-3.079*** (0.877)	-2.033* (0.832)	-1.946* (0.844)	-1.878* (0.849)	-1.876* (0.849)	-1.871* (0.850)	-2.047* (0.897)	-2.117* (1.068)	-2.110* (1.067)	-2.128* (1.062)	-2.083 (1.071)	-2.071 (1.071)	-2.077 (1.094)	-0.118 (0.063)	
Taux d'endettement		0.672*** (0.152)	0.660*** (0.153)	0.762*** (0.159)	0.718*** (0.165)	0.613** (0.188)	0.617** (0.188)	0.627*** (0.189)	0.613** (0.200)	1.025*** (0.242)	1.028*** (0.243)	1.030*** (0.246)	1.001*** (0.244)	0.991*** (0.246)	0.828** (0.285)	0.047** (0.016)	
Taux d'autofinancement des nouveaux investissements			-0.082 (0.130)	0.370* (0.172)	0.365* (0.174)	0.376* (0.176)	0.371* (0.175)	0.372* (0.176)	0.380* (0.179)	0.547** (0.211)	0.540* (0.213)	0.535* (0.213)	0.551* (0.218)	0.552* (0.217)	0.400 (0.219)	0.023 (0.012)	
Taux de capacité d'autofinancement				-5.594*** (1.655)	-5.225** (1.642)	-5.266** (1.635)	-5.259** (1.632)	-5.362** (1.672)	-5.510** (1.688)	-3.923* (1.939)	-3.933* (1.943)	-3.802 (1.989)	-4.122* (2.027)	-4.245* (2.027)	-2.781 (2.086)	-0.158 (0.118)	
Taux de rigidité des charges de structure					1.810 (1.597)	2.048 (1.581)	2.126 (1.598)	2.133 (1.600)	2.122 (1.667)	-0.263 (1.837)	-0.259 (1.816)	-0.278 (1.824)	-0.243 (1.870)	-0.091 (1.881)	-0.419 (1.953)	-0.024 (0.111)	
Dette par habitant					0.037 (0.068)	0.038 (0.069)	0.038 (0.069)	0.038 (0.069)	0.060 (0.103)	0.044 (0.094)	0.042 (0.092)	0.044 (0.096)	0.053 (0.089)	0.052 (0.090)	0.030 (0.042)	0.002 (0.002)	
Part Dexia dans l'endettement total					0.025 (0.015)	0.025 (0.015)	0.025 (0.015)	0.025 (0.015)	0.024 (0.015)	0.058*** (0.016)	0.058*** (0.016)	0.058*** (0.016)	0.060*** (0.016)	0.060*** (0.016)	0.051** (0.016)	0.003** (0.001)	
Capacité de désendettement								-0.001 (0.003)	-0.002 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.003)	0.001 (0.003)	0.000 (0.000)	
Coefficient de mobilisation du potentiel fiscal									-0.631 (0.338)	-0.332 (0.340)	-0.280 (0.340)	-0.272 (0.339)	-0.252 (0.342)	-0.237 (0.341)	-0.287 (0.365)	-0.016 (0.021)	
ln(Population)									0.541*** (0.088)	0.548*** (0.090)	0.548*** (0.090)	0.536*** (0.092)	0.592*** (0.102)	0.585*** (0.104)	0.539*** (0.117)	0.031*** (0.007)	
ln(Niveau vie médian)										0.293 (0.644)	0.293 (0.644)	0.663 (1.025)	0.558 (1.030)	0.441 (1.057)	0.293 (1.230)	0.017 (0.070)	
Taux de chômage												1.485 (3.467)	0.278 (3.665)	0.271 (3.655)	0.032 (3.885)	0.002 (0.221)	
Part de 15-64 ans dans la population totale													-3.634 (2.762)	-3.388 (2.790)	-1.263 (2.855)	-0.072 (0.162)	
Nuance politique														0.061 (0.096)	0.080 (0.110)	0.005 (0.006)	
Ratio de surcoût total															9.567*** (1.100)	0.544*** (0.067)	
Observations	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	1541	
AUROC	0.629 (0.025)	0.668 (0.025)	0.670 (0.025)	0.687 (0.025)	0.693 (0.025)	0.694 (0.025)	0.694 (0.025)	0.695 (0.025)	0.703 (0.025)	0.745 (0.022)	0.745 (0.022)	0.745 (0.022)	0.745 (0.022)	0.749 (0.022)	0.750 (0.022)	0.868 (0.017)	1541

5.2 Méthodes d'estimations alternatives

La répartition entre les communes qui bénéficient d'une aide et celles qui n'en bénéficient pas nous montre que ces dernières prédominent (90%). Il est donc légitime de se demander si notre modèle estimé en logit permet de tenir compte ici de ce déséquilibre. Notons que ce n'est pas tant la proportion mais plutôt le faible nombre d'observations attachées à une catégorie qui peut être problématique. Au regard du nombre d'observations pour les communes bénéficiant d'une aide (120 communes), l'estimation logit est en principe fiable.

Un modèle de Poisson n'aurait-il pas été plus adapté ici ? Un tel modèle permet l'estimation d'événements discrets, sans nécessairement être binaires. Ici, la distribution étudiée étant binaire, mobiliser un modèle de Poisson pour le réduire à une approche dichotomique ne fournirait qu'une approximation de loi binomiale. Il semble ici plus intéressant d'utiliser un modèle logit reposant directement sur une telle approche. De plus, le modèle de poisson requiert que la moyenne et la variance soient égales ($\mu = \sigma^2$), ce qui n'est pas le cas de notre échantillon ($\mu = 0.07, \sigma^2 = 0.27$). Néanmoins, la Table C.11 conduit une estimation par un modèle de Poisson pour tester la robustesse des résultats. Les nouvelles estimations confirment globalement nos conclusions. Notamment, la qualité prédictive du modèle (mesurée par AUROC) n'augmente pas substantiellement lorsqu'on inclut l'ensemble des variables explicatives dans la colonne (4).

Enfin, pour compléter l'analyse de la robustesse de l'estimation en logit, la Table C.11 produit les mêmes estimations en mobilisant cette fois-ci le Penalized Maximum Likelihood Logit de Firth (1993), devenue une approche standard pour l'analyse des variables dépendantes binaires avec de petits échantillons (en médecine notamment). Les résultats obtenus dans les colonnes (5) et (6) confirment bien les estimations obtenues par un modèle logit conventionnel dans les colonnes (1) et (2). Ainsi, la méthode ne révèle pas de biais particulier pouvant être lié à la taille de l'échantillon. Pour plus de détails sur cette méthode, voir Puhr et al. (2017).

Table C.11 – Robustesse du Logit : estimations alternatives.

	LOGIT		POISSON		FIRTH-LOGIT	
	(1)	(2)	(3)	(4)	(5)	(6)
Ratio de surcoût total	10.391*** (1.175)	9.614*** (1.010)	6.216*** (0.515)	6.543*** (0.433)	10.341*** (0.917)	9.372*** (0.909)
Taux d'équipement		-2.704* (1.279)		-2.334* (0.924)		-2.576* (1.080)
Taux d'endettement		0.819*** (0.237)		0.626*** (0.158)		0.801** (0.251)
Taux d'autofinancement des nouveaux investissements		0.124 (0.151)		0.125 (0.108)		0.137 (0.202)
Taux de capacité d'autofinancement		-4.209* (1.894)		-3.276* (1.455)		-4.178* (2.083)
Taux de rigidité des charges de structures		1.563 (1.923)		1.803 (1.416)		1.335 (1.503)
Dette par habitant		0.023 (0.035)		0.024 (0.028)		0.030 (0.039)
Part des emprunts Dexia dans l'endettement total		0.110** (0.039)		0.095*** (0.028)		0.124* (0.052)
Capacité de désendettement		-0.002 (0.002)		-0.001 (0.001)		-0.000 (0.002)
Coefficient de mobilisation du potentiel fiscal		-0.198 (0.395)		-0.260 (0.303)		-0.168 (0.394)
ln(Population)		0.477*** (0.117)		0.377*** (0.080)		0.461*** (0.123)
ln(Niveau vie median)		0.097 (1.203)		0.229 (0.858)		0.154 (1.172)
Taux de chômage		-1.749 (3.860)		-0.260 (2.722)		-1.600 (3.733)
Ratio de 15-64 ans		-1.342 (2.974)		-1.356 (2.177)		-1.319 (2.812)
Nuance politique		0.111 (0.114)		0.066 (0.089)		0.109 (0.109)
Observations	1542	1542	1542	1542	1542	1542
AUROC	0.859	0.872	0.859	0.865	0.859	0.873
σ	(0.018)	(0.017)	(0.018)	(0.017)	(0.018)	(0.017)

Notes : Les colonnes (1) et (2) reproduisent les estimations basées sur la méthode employée dans le chapitre 3. Les colonnes (3) et (4) présentent les estimations de l'octroi d'une aide par le fonds au travers d'un modèle Poisson. Enfin, les colonnes (5) et (6) reportent les estimations des deux mêmes modèles, cette fois-ci en mobilisant le Penalized Maximum Likelihood Logit proposé par Firth (1993).

5.3 Interaction entre endettement et surcoût

L'exercice présenté maintenant part du constat suivant : dans la table III.2, la variable d'endettement par habitant de la commune n'est statistiquement plus significative lorsque l'on intègre le ratio de surcoût supporté par la commune. Par ailleurs, cette même variable n'est pas significativement associée à la probabilité d'octroi d'une aide lorsque l'on contrôle pour la dimension financière dans les tables C.8, C.9 et C.10 présentées en annexe.

Ce point mérite d'être étudié de plus près car il se peut que l'endettement de la commune et le niveau du ratio de surcoût supporté par celle-ci interagissent pour l'octroi d'une aide du fonds. Nous vérifions ce point en ajoutant un terme d'interaction. La table C.12 présente cet exercice.

La colonne (1) reproduit l'estimation sur la base du seul ratio de surcoût, notamment pour avoir un étalon pour l'AUC pour les autres estimations de la table. Les colonnes (2) et (3) proposent une estimation pour l'endettement, normé dans un premier temps par la richesse de la commune, puis par le nombre d'habitants. Ainsi, le taux d'endettement figure dans la dimension financière, alors que la dette par habitant était un des critères réglementaires pour le fonds. Un premier constat, déjà soulevé dans le corps du chapitre, est que le taux d'endettement est plus précisément estimé.

Dans les colonnes (4) et (5), nous estimons le ratio de surcoût assorti d'une des variables d'endettement. Nous retrouvons ici le constat qui a motivé cet exercice. La dette par habitant n'est plus significativement associée à l'obtention d'une aide du fonds. On note toutefois que le taux d'endettement reste lui significativement associé à cette aide, mais le coefficient perd un peu en ampleur et en précision. Ces estimations confirment que l'endettement a pu être un élément dans la prise de décision, sans pour autant être basé sur l'indicateur réglementaire. Notons toutefois qu'aucune des variables d'endettement n'accroît l'AUC obtenu avec le seul ratio de surcoût dans la colonne (1).

Enfin, les colonnes (7) à (10) reportent les estimations avec un terme d'interaction entre la variable d'endettement et le ratio de surcoût, avec une estimation complémentaire en tenant compte des autres dimensions comme variables de contrôle. Le terme d'interaction n'est jamais statistiquement significatif, tout comme la variable d'endette-

ment. En revanche le ratio de surcoût demeure statistiquement significatif et l'AUROC ne varie pas substantiellement par rapport à l'estimation fondée sur ce seul ratio de surcoût.

La colonne (6) soulève un point intéressant, en intégrant les deux variables d'endettement, il ressort que le taux d'endettement a été un élément considéré par le fonds de soutien, plus encore que la dette par habitant qui devait être un critère réglementaire. Il semble donc que le fait que l'endettement par habitant perde en significativité traduit en réalité que cette variable, lorsqu'elle est estimée avec peu de variables de contrôle, souffre d'un biais lié à des variables omises, notamment le taux d'endettement. Cet exercice souligne donc non seulement la faible capacité prédictive des variables d'endettement, mais surtout la prégnance du ratio de surcoût.

Table C.12 – Régressions Logit avec termes d'interactions.

LOGIT	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ratio de surcoût total	10.391*** (1.175)			10.178*** (1.152)	10.318*** (1.171)		8.490** (2.927)	7.874*** (2.317)	10.204*** (1.271)	9.208*** (1.143)
Taux d'endettement		0.652*** (0.143)		0.505** (0.161)		0.613*** (0.162)	0.237 (0.368)	0.578 (0.362)		
Dette par habitant			0.063* (0.028)		0.032 (0.027)	0.013 (0.031)			0.016 (0.040)	0.011 (0.052)
Ratio de surcoût × Taux d'endettement							1.676 (2.636)	1.820 (2.271)		
Ratio de surcoût × Dette par habitant									0.075 (0.247)	0.387 (0.287)
Autres dimensions	NON	NON	NON	NON	NON	NON	NON	OUI	NON	OUI
Observations	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542
AUROC	0.859	0.643	0.687	0.856	0.859	0.645	0.857	0.873	0.859	0.863
σ	0.018	0.024	0.023	0.017	0.018	0.024	0.018	0.017	0.018	0.018

5.4 Variables pertinentes : Bayesian Model Averaging

Cette section conduit un exercice supplémentaire basé sur le Bayesian Model Averaging (BMA). L'objectif est ici d'identifier les caractéristiques des communes qui bénéficient d'une aide du fonds, en complément de l'approche centrale autour du logit.

Cette méthode¹, déjà été employée dans le chapitre 1 (voir la section 3.2 page 194), fournit un mécanisme de prise en compte de l'incertitude du modèle lors de l'estimation de paramètres (Leamer, 1978). L'idée sous-jacente est de calculer une moyenne pondérée des estimations conditionnelles pour toutes les combinaisons possibles de variables, car chacune d'elles fournit des informations sur les paramètres estimés. Le poids donné à chaque modèle et les estimations conditionnelles de ses paramètres sont déterminés suivant une inférence bayésienne, qui combine des probabilités *a priori* sur les éléments inconnus du modèle avec les informations supplémentaires issues des données².

La méthode BMA estime d'abord le paramètre d'intérêt conditionnel à chaque modèle parmi toutes les combinaisons possibles de variables (pour k variables, $I = 2^k$ modèles \mathcal{M}_i sont estimés). Ensuite, les estimations inconditionnelles sont obtenues par une moyenne pondérée des estimations conditionnelles.

Nous utilisons donc cette méthode pour identifier les caractéristiques des communes qui bénéficient d'une aide du fonds. La table C.13 fournit ainsi la moyenne et l'écart-type de la distribution *a posteriori*, la statistique t , la probabilité d'inclusion postérieure ainsi qu'un intervalle de confiance. Les tableaux ne rapportent pas les valeurs p des ratios t pour tester la significativité statistique du paramètre. La raison est que la contrepartie bayésienne d'une valeur p ne peut pas être facilement calculée parce que les distributions ne sont pas gaussiennes (De Luca et Magnus, 2011).

En suivant le travail de Magnus (2002), une variable est robuste si le rapport t sur son coefficient est supérieur à 1 en valeur absolue³. Alternativement, Raftery (1995) et Masanjala et Papageorgiou (2008) suggèrent qu'une probabilité d'inclusion postérieure

1. Pour une description détaillée de la littérature sur le *Model Averaging*, voir Hoeting et al. (1999).

2. BMA attribue une probabilité a priori égale $\mathbb{P}(\mathcal{M}_i) = 2^{-k}$ à chaque modèle. Pour plus de détails, voir De Luca et Magnus (2011).

3. "L'intuition derrière ce résultat est également liée à une propriété bien connue de \bar{R}^2 (le R^2 ajusté), qui augmente si et seulement si le rapport t associé à un régresseur ajouté est supérieur à un en valeur absolue" (De Luca et Magnus, 2011, p.531).

supérieure à 0.5 peut servir de seuil approximatif d'inclusion.

À la lecture des résultats, trois variables ressortent comme robustes : le ratio de surcoût, le taux d'endettement et la taille de la commune. Ces variables correspondent bien à celles identifiées au travers du logit présenté dans le corps du papier. Sans surprise, le ratio de surcoût est la variable qui a le rapport t sur coefficient le plus élevé, soulignant ici la grande robustesse de cette variable.

Une subtilité de lecture mérite toutefois d'être soulignée. La méthode BMA permet d'identifier les variables qui demeurent statistiquement significatives face aux différents agencements de variables explicatives. Une variable peut donc avoir une association stable et précisément estimée, tout en étant économiquement marginale. C'est précisément l'intérêt de la méthode de l'AUROC, permettant de dépasser la seule significativité statistique. Ainsi, comme nous l'avons mis en évidence tout au long du chapitre, le taux d'endettement répond à ce cas de figure. En effet, bien que précisément estimé, cette variable ne dispose que d'une faible capacité prédictive de l'octroi d'une aide par le fonds, bien inférieure à celle du ratio de surcoût.

Table C.13 – Bayesian Model Average : Variables pertinentes pour le fonds de soutien.

BMA : Aide du fonds de soutien	Observations : 1542					
	Coef. (μ)	σ	t	PIP	$\mu \pm \sigma$	
Ratio de surcoût total	1.131*	0.075	14.95	1.00	1.056	1.207
Taux d'équipement	-0.025	0.043	-0.58	0.29	-0.068	0.018
Taux d'endettement	0.031*	0.022	1.39	0.73	0.008	0.054
Taux d'autofinancement nouveaux invest.	-0.001	0.002	-0.05	0.03	-0.002	0.002
Taux de capacité d'autofinancement	-0.045	0.091	-0.50	0.24	-0.136	0.045
Taux de rigidité charges de struct.	0.008	0.038	0.21	0.06	-0.030	0.046
Dettes par habitant	0.001	0.001	0.14	0.04	-0.001	0.001
Part des emprunts Dexia	0.001	0.001	0.20	0.06	-0.001	0.002
Capacité de désendettement	0.000	0.201	-0.07	0.03	-0.001	0.001
Coefficient de potentiel fiscal	-0.001	0.003	-0.04	0.03	-0.003	0.003
ln(Population)	0.027*	0.007	3.80	0.99	0.020	0.034
ln(Niveau vie median)	0.001	0.009	0.12	0.03	-0.008	0.011
Taux de chômage	-0.003	0.027	-0.10	0.03	-0.030	0.024
Ratio de 15-64 ans	-0.001	0.026	-0.06	0.03	-0.028	0.025
Nuance politique	0.001	0.002	0.17	0.05	-0.001	0.002
Cons.	-0.329*	0.128	-2.56	1.00	-0.045	-0.020

Notes : Cette table présente les coefficients estimés et leur écart-type, qui sont obtenus par la distribution postérieure, ainsi que la statistique t , la probabilité d'inclusion postérieure (PIP) et les intervalles de confiance. Un régresseur est une variable robuste si le rapport t sur son coefficient est supérieur à 1 en valeur absolue, soit un ratio t supérieur au coefficient (Magnus, 2002). Alternativement, Raftery (1995) et Masanjala et Papageorgiou (2008) suggèrent une règle approximative selon laquelle une probabilité d'inclusion postérieure (PIP) supérieure à 0.5 peut servir de seuil (* PIP > 0.5).

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Essais sur les conséquences de l'expansion financière : croissance, cycles et crises

La présente thèse propose une étude des conséquences de l'expansion financière initiée à la fin des Trente Glorieuses comme nouveau ressort d'une croissance économique alors en perte de vitesse. De manière incidente, ce travail se penche sur la problématique de la mesure des régularités économiques par le biais des outils économétriques. Ainsi, le premier chapitre conduit une analyse approfondie de la méthodologie qui guide une grande partie de la littérature empirique sur la relation finance-croissance. Il lève le voile sur un certain nombre de problèmes remettant en question la fiabilité des résultats antérieurs, dont notamment la conclusion selon laquelle la relation entre expansion financière et croissance économique serait régie par un effet de seuil. Nos estimations documentent en revanche un effet globalement négatif de l'expansion du crédit au secteur privé sur la croissance. Cet effet, qui s'est intensifié dans le temps, apparaît comme plus prononcé dans les économies avancées. L'analyse jointe de l'endettement privé et public, conduite dans le deuxième chapitre, montre que la dette publique tend à freiner la croissance, non pas initialement, mais à la suite d'une crise financière, si le gouvernement est incapable de conduire une politique budgétaire contra-cyclique. La trajectoire de l'endettement public est ici l'élément déterminant, plus encore que le niveau d'endettement. Le dernier chapitre, en étudiant le sauvetage des communes exposées aux emprunts structurés toxiques, offre une illustration d'une dérive de la libéralisation financière engagée en France dans les années 1980. Ce travail souligne toute l'ambivalence des fonds de soutien.

Mots clés : Finance · Croissance · Cycles économiques · Crises · Dette publique · Crédit privé · Économétrie appliquée · Évaluation des politiques publiques

Essays on the Consequences of Financial Expansion: Growth, Cycles and Crises

The present thesis studies the consequences of the financial expansion initiated at the end of The Glorious Thirty to spur a declining economic growth. Incidentally, this work questions the issue of measuring economic regularities through econometric tools. Thus, the first chapter carries out an in-depth analysis of the methodology that guides a substantial part of the empirical literature on the finance-growth nexus. It highlights an array of statistical issues casting serious doubts on the reliability of previous results, including, in particular, the conclusion that the relationship between financial expansion and economic growth is non-linear. However, our estimates document an overall negative effect of expanding credit to the private sector on growth. This effect, which has intensified over time, appears to be more pronounced in advanced economies. The joint analysis of private and public debt, conducted in the second chapter, shows that public debt tends to be a drag on growth, not initially, but in the aftermath of a crisis if policymakers are unable to pursue a counter-cyclical fiscal policy. It is not the level of public debt but rather its trajectory that affects economic growth. Finally, the last chapter investigates the bailout of municipalities exposed to toxic structured loans. It illustrates a failure in the financial liberalization initiated in France in the 1980s to stimulate local governments' financing. This work highlights the ambivalence of the support funds.

Keywords : Finance · Growth · Business Cycles · Crises · Public Debt · Private Credit · Applied Econometrics · Policy Evaluation