UNIVERSITÀ DEGLI STUDI DI NAPOLI FEDERICO II

DIPARTIMENTO DI SCIENZE ECONOMICHE E STATISTICHE

Dottorato di Ricerca

in **Economia**

Ciclo **XXIX**

Public Policies and Incentives in Banking and Education

Tutor	**Coordinatore**	**Candidato**
Ch.mo Prof.	Ch.ma Prof.ssa	Dott.
Marco Pagano	Maria Gabriella Graziano	Lorenzo Pandolfi

Public Policies and Incentives in Banking and Education

Lorenzo Pandolfi

April 10, 2017

Contents

“L’economista è il fiduciario di una civiltà possibile
e se gli interessi costituiti prevalgono sulle idee,
tuttavia l’economista deve stare attento alle idee.”
(Federico Caffè, Scritti quotidiani, 2007)

“*ο δὲ ανεξέταστος βίος ου βίωτὸς ανθρὼπω.*”
(Platone, Apologia di Socrate, 399/388 a.c.)

Acknowledgements

In the last three years I have accumulated a sizeable debt of gratitude to a number of people without whose help and support writing this thesis would have never been possible. These few lines are just an attempt to partially repay my debt.

First and foremost, I wish to thank my supervisor, Marco Pagano. Not only I owe him my gratitude for his patient guidance, for the invaluable assistance he provided and for the tremendous encouragement I received from him during the thesis process and throughout my entire PhD, but also I sincerely thank him for his inspiring example and his contagious passion towards research. I can safely say that I would not be a researcher today if I had not met him in the last year of my master, and for this I owe him my largest debt of gratitude.

My profound gratitude goes also to those faculty members at the Department of Economics of the University of Naples Federico II who provided support at different stages of my research. In particular, I sincerely thank Maria Gabriella Graziano, Tommaso Oliviero, Annalisa Scognamiglio, Saverio Simonelli and Alberto Zazzaro for all of their precious suggestions and encouragements. I also thank Vittoria Cerasi and Anna Maria Menichini for their comments upon this work. Finally, among those who contributed to this research, a special mention goes to Marco Nieddu, who is an amazing coauthor and an even better friend.

Of course, I thank my parents, Aldo and Luciana - my first teachers and role models - as well as my brother Francesco and my sister Caterina that allowed me to get to this point by supporting me during my entire life with their unconditional love.

No words can express how grateful I am to Angelo, Betta, Bobo, Daria, Enzo, Laura, Marcello, Maria, Roberto, Ronnald and Valentina, for their friendship, for being part of my life since years and for all of the unforgettable moments we have shared, and we will share in the future.

Last but obviously not least, I wish to thank Ginevra, my safe harbor, for bringing me joy every single day. As a small recompense for this priceless gift, I dedicate this thesis to her.

Chapter 1

1 Public Policies and Private Incentives

Since its dawn, modern economic literature has devoted a considerable attention to study the effects of government intervention on agents' behavior. Public policies indeed provide a number of explicit and implicit incentives to firms and consumers, thus affecting their choices and their actions in a multitude of ways that are not always easy to forecast and to analyze. Whether government intervention - in goods, financial and labor markets - mitigates market inefficiencies or, on the contrary, exacerbates them, remains an open - ideological more than purely empirical - question, providing an answer to which goes beyond the ambition of this work, whose aim is instead to analyze the impact of public policies and government interventions in two different contexts.

In particular, the empirical analysis of the effects of government policies on the incentives of economic agents is the *leitmotif* of the present thesis, with two distinct fields of application. While the first essay mostly contributes to the empirical banking literature, with a focus on the link between implicit guarantees for bank debt and political connections in Europe, the second one contributes to the field of education economics and is devoted to an analysis of the effects of bibliometric-based hiring and promotion schemes in Italian public universities on scholars' productivity. The two essays also share some methodological affinities. First, the two projects exploit two different identification strategies that have the common ambition of isolating and estimating a causal effect of public policies on the outcomes of interest. Second, the two works are characterized by the use of two original datasets, that have been obtained merging multiple sources of data, some of them pre-existing and others that have been hand-collected. Finally, the two essays share the novelty of the research questions they aim to answer, which are relatively unexplored by the existing literature.

1.1 Government Bailouts and Banks' Incentives

Chapter 2 in this thesis explores the relationship between the implicit guarantees of state aid provided to domestic banks by national governments and banks' funding costs. The contribution of this research to the existing literature is manifold: first, this is the first paper that investigates the effects of political connections on implicit guarantees for bank debt; second, it is based on a large panel dataset resulting from a merger of four different sources of data relative to a representative sample of large European banks from 2007 to 2012; finally, it proposes a novel measure of political connectedness - the share of a banks' board members previously employed by the domestic government either as directors or as senior managers - that might find other empirical applications.

The relationship between the public sector and the banking sector has been largely analyzed by different streams of the financial economics literature. One set of studies focuses on the desirability of government intervention in support of banks facing difficulties. Indeed, since Bagehot (1873) economists have being discussing about the rationale for public intervention to support banks in distress, reaching mixed conclusions. On the one hand helping banks in difficulties can distort bankers' incentives to undertake risk (Repullo (2005)) and reduce the incentives for uninsured creditors to monitor the behavior of the bank (Kaufman (1991)), thus leading to excessive risk taking. Bank recovery policies can also have the undesirable effect of providing incentives for banks to correlate their risks in order to maximize their rents from bailouts in case of a systemic failure of the banking system, as shown by Farhi and Tirole (2011). Also, according to Calomiris (2017), governments' implicit guarantee on bank debt is one of the "two 800 pounds gorillas" threatening financial stability since "bank protection creates rents outside the normal budgetary process that funds and encourages risk taking" (p. 1). On the other hand, public interventions to rescue banks in distress can avoid the inefficient closure of solvent banks facing a liquidity shock (Rochet and Vives (2004)) and, by reducing fire sales, can avert the risk of contagion, as highlighted by Acharya and Yorulmazer (2008). In presence of systemic risk, central bank interventions can even mitigate moral hazard, as shown by Dell'Ariccia and Ratnovski (2012), Diamond and Rajan (2005) and Cordella and Yeyati (2003).

De facto, despite the controversy on the desirability of public intervention in the banking sector, governments often use public funds to avoid bank failures, and justify this behavior with the necessity of averting the risk of large negative shocks that would hamper the whole economy. As a result, several financial institutions benefit from implicit guarantees for their debt provided by national governments and this expectation of public support has a direct and substantial impact on banks' funding cost. Rationally anticipating that governments do not easily let banks fail, and therefore that the cost of insolvency will be borne by taxpayers, reduces investors' required return, thus providing an implicit subsidy to banks that have access to cheap debt, given their risk profile. Not surprisingly, the financial companies that benefit the most from such implicit guarantees are those banks that are large and systemically important and whose failures would put at risk the stability of the whole banking sector. This *too-big-to-fail* (TBTF) mechanism has become increasingly popular, especially after the 2008 financial crisis, and a vast literature has been dealing with it. An exhaustive review of these studies is provided in Section 2.1.

Another source of connection among banks and the public sector is provided by the so-called *bank-sovereign nexus* - *i.e.*, an adverse feedback loop between sovereign debt and the stability of the banking sector - that has been modeled and documented by several authors, including Acharya, Drechsler and Schnabl (2013) and Gennaioli, Martin and Rossi (2014), among others. One of the drivers of this loop is the so-called *home bias*, that is, the large exposure of domestic banks to public debt, especially in fiscally stressed countries. Two main hypothesis have been provided to explain this evidence by scholars: a *carry trade* hypothesis, according to which undercapitalized banks increased their exposure to sovereign debt when returns from the latter became particularly high, thus adopting a "gamble for resurrection strategy", and a *moral suasion* one, with governments that put pressure on domestic banks to purchase sovereign debt in periods of high financing needs of the government. Evidence in support of the carry trade hypothesis is provided by Buch, Koetter and Ohls (2016), in a sample of German banks, as well as by Acharya and Steffen (2015) that document how banks located into those countries mostly affected by the sovereign debt crisis increased their exposure to high-yield domestic sovereign debt, actually betting on their own survival (coherently with Diamond and Rajan (2011) risk-shifting hypothesis according to which banks located into countries at risk of default have incentives to allocate their risk in the state of nature in which they would probably suffer from a bank run in any case).

The moral suasion channel is also documented by several scholars. Among those, Ongena, Popov and van Horen (2016) find evidence of an increase of domestic banks' exposure to sovereign debt in fiscally stressed countries, with the effect being stronger for state-owned banks. Further evidence of moral suasion is provided by De Marco and Macchiavelli (2016), that document how banks with stronger political connections - that is, state-owned banks and banks with former politicians sitting in the board of directors - were characterized by a larger *home bias* that unconnected banks, especially in GIIPS countries. Relatedly, Altavilla, Pagano and Simonelli (2016) show that both a moral suasion and a carry trade motive were contemporaneously at work during the European sovereign debt crisis, when recently bailed-out and undercapitalized banks increased their holdings of domestic public debt more than other banks.

What remains relatively unexplored by the literature are the determinants of the expected amount of state aid of banks, other than size and systemic importance. The aim of the analysis presented in Chapter 2 is exactly to shed light on another possible determinants of implicit guarantees: political connectedness, whose effect is estimated in a sample of European banks. Given the nature of the dataset used for this analysis - that combines bank's balance sheet and board-specific data with credit ratings and information about state aids approved by the European Union - the methodology adopted to this purpose is a relatively standard panel analysis. To capture the effect of political connections on credit rating uplifts, long-term ratings and the likelihood of state aid (the main variables of interests in the analysis)

I estimate several different specifications with panel data techniques. A possible concern for the causal interpretation of the results is that unobservables that correlate with both implicit guarantees and political connections might lead to endogeneity problems. I tackle this endogeneity concern in two ways: firstly, I exploit the variation of banks' baseline ratings over time and the differential effect of political connections on long-term ratings depending on banks' financial strength - this effect being larger the lower the baseline rating - to estimate the effect of the interaction between the former and the measure of political connections in a model with bank fixed effects, that account for any bank-specific and time-invariant unobservable that might affect the estimates. Second, I exploit the exogenous nature of the 2010-2011 European sovereign debt crisis to estimate the differential impact of political connectedness in those country mostly affected by the crisis. A more detailed discussion about the empirical methodology is provided in Section 2.2.

The main results in Chapter 2 - whose detailed description is contained in details Section 2.3 - are the following: i) political connections increase both the credit rating uplifts and the long-term ratings, controlling for the baseline ones; ii) the impact of political connections is larger for banks whose baseline rating is lower; iii) the effect of political connectedness on implicit government guarantees is lower during the sovereign debt crisis in the countries mostly affected by the latter; iv) political connections affect implicit bank guarantees both at the intensive and at the extensive margin; v) the higher expectation of state aid driven by political connectedness and embedded in credit rating uplifts translates into a higher likelihood of state aid for European banks.

1.2 Regulation of the University Labor Market and Academic Productivity

The focus of the second essay is on the effects of government policies on academics' incentives towards research. Given the importance of public education in most of the

European countries, where higher-education is mainly provided by the public sector, understanding the response of scholars to different hiring and promotion schemes become crucial, especially when these schemes are designed exactly with the goal of promoting research. This is for instance the case with the centralized evaluation systems introduced in recent years in Spain and Italy, among others, to limit the access to university professorship to candidates complying with minimum standards established at the national level. Whether such bibliometric-based mechanisms actually increase scholars' productivity or not - for instance by "crowding out" scholars' intrinsic motivation - is clearly a crucial question in education economics. It is also worth analyzing whether an eventual increase in the productivity of scholars - measured by the number of publications - occurs at the detriment of the average quality of their research. However, providing an answer to such questions is empirically challenging, since it is often impossible to disentangle all the factors that determine research productivity. As a result, the literature in this area is relatively scarce and belongs to two main strands: one focusing on the *post*-tenure productivity of scholars, mostly developed during the 90s, and a more recent one that deals with *post*-prize productivity in academia. Most of this literature however, tends to agree on the conclusion that, once scholars reach tenure or get promoted, their productivity declines.[1]

Chapter 3 in this thesis aims to provide an answer to the questions discussed above by exploiting the introduction of the National Scientific Habilitation in Italy in 2012 and proposing a triple regression discontinuity approach where the research production of barely successful and unsuccessful candidates is compared in order to estimate the causal effect of the habilitation on academics' productivity. This work not only contributes to the education economic literature but also to the applied econometrics' one, since the triple regression discontinuity setting proposed is novel in the literature and can find other empirical applications.

Since the early 2000s, regression discontinuity design became increasingly popular in several fields of applied economics. The main advantage offered by such method-

[1] An exhaustive review of this literature is provided in Section 3.1.

ology consists in the possibility of evaluating the causal effect of an intervention on a given outcome of interest by exploiting the discontinuity in the probability of receiving the treatment depending on the value of a specific running variable and its distance from a given threshold. As long as all the other covariates that might correlate with the variable of interest are continuous around the threshold, any discontinuity that emerges at the cutoff can be therefore attributed to the treatment itself. A few authors recently extended the application of regression discontinuities to the case of multiple cutoffs - where the treatment is received if a score is above a cutoff that can in principle vary across observations - and to the case of multiple running variables, where eligibility for the treatment depends on being above a given threshold in more than a single score.[2] Since the general rule introduced by the National Scientific Habilitation states that candidates get the habilitation depending on their score in three different bibliometric indicators, given some sector specific thresholds, this work proposes a combination of the two aforementioned extension of RD designs in a novel fuzzy regression discontinuity setting with several sector-specific cutoffs and multiple running variables, *i.e.* the three bibliometric indicators. As a result, this methodology allows to estimate the effect of obtaining the habilitation - thus getting one step closer to tenure - on different research outcomes measuring scholars' productivity, both in terms of quantity and quality of the articles produced after 2012.

To this purpose, a massive work of data collection, whose details will be discussed in Section 3.4, has been performed. Indeed, the dataset used for this analysis comes from three main sources. The website of the MIUR was searched to obtain data on all the candidates to the first call of the NSH, in 2012, including the candidate-specific indicators, sector medians, and the academic position of each applicant at the time of the application. This information has been merged with all available publication-level data relative to Italian scholars, obtained through SCOPUS, and including

[2] Among others Cattaneo, Keele, Titiunik and Vazquez-Bare (2016) discuss the methods and the intepretation of the estimates in RD designs with multiple cutoffs; Papay, Willett and Murname (2016), instead, propose an estimation methodology that can be applied in presence of more than a single running variable.

detailed information about each publication and the corresponding authors. Using the information about the journals, each publication has been then matched with the Scimago Journal Ranking (SJR) of the corresponding journal, that has been obtained from SCIMAGO, and publications have been ranked depending on the quartile of the sector-specific distribution of the SJR. Finally, a number of algorithms to solve most of the omonymy-related problems and to correctly match each candidate to the NSH with its publications period has been used, thus obtaining a final dataset that contains yearly data on over 40 thousands scholars over the 2009-2016 period.

Estimating the triple regression discontinuity design above described on this exhaustive set of data gives the following two main results: i) the professors that passed the habilitation and qualified as full professors in 2012 published fewer articles than rejected candidates, and this effect can be attributed to the increase in the productivity of barely rejected scholars more than from a decline in productivity of successful candidates; ii) given the relatively small time span of data, an equally strong and significant effect on the qualitative side is not found, but the average quality of the publications of barely successful candidates in 2016 is larger than the one of barely rejected scholars.

References

[1] Acharya, Viral, Drechsler, Itamar, and Schnabl, Philipp, (2014), "A Pyrrhic Victory? Bank Bailouts and Sovereign Credit Risk", *Journal of Finance*, 69, issue 6, p. 2689-2739.

[2] Acharya, Viral, and Steffen, Sascham, (2015), "The Greatest Carry Trade Ever? Understanding Eurozone Bank Risks", *Journal of Financial Economics*, 115, issue 2, p. 215-236.

[3] Acharya, Viral, and Yorulmazer, Tanju, (2008), "Cash-in-the-Market Pricing and Optimal Resolution of Bank Failures", *Review of Financial Studies*, 21, issue 6, p. 2705-42.

[4] Altavilla, Carlo, Pagano, Marco, and Simonelli, Saverio, (2016), "Bank Exposures and Sovereign Stress Transmission", CSEF Working Paper No. 410.

[5] Bagehot, Walter, (1873), "Lombard Street: A Description of the Money Market", McMaster University Archive for the History of Economic Thought.

[6] Buch, Claudia M., Koetter, Michael, and Ohls, Jana, (2016), "Banks and Sovereign Risk: a Granular View", *Journal of Financial Stability*, 25 issue C, p. 1-15.

[7] Cattaneo, Matias, Keele, Luke, Titiunik, Rocío, and Vazquez-Bare, Gonzalo, (2016), "Interpreting Regression Discontinuity Designs with Multiple Cutoffs", *The Journal of Politics*, 78, issue 4, p. 1229-1248.

[8] Calomiris, Charles, (2017), "Taming the Two 800 Pounds Gorillas in the Room", NBER Working Paper.

[9] Cordella, Tito, and Levy Yeyati, Eduardo, (2003), "Bank Bailouts: Moral Hazard vs. Value Effect", *Journal of Financial Intermediation*, 12, issue 4, p. 300-330.

[10] De Marco, Filippo, and Macchiavelli, Marco, (2016), “The Political Origin of Home Bias: the case of Europe”, Finance and Economics Discussion Series 2016-060. Board of Governors of the Federal Reserve System.

[11] Diamong, Douglas W., and Rajan, Raghuram G., (2005), “Liquidity Shortages and Banking Crises”, *Journal of Finance*, 60, issue 2, p. 615-47.

[12] Diamong, Douglas W., and Rajan, Raghuram G., (2011), “Fear of Fire Sales, Illiquidity Seeking, and Credit Freeze, *The Quarterly Journal of Economics*, 126, issue 2, p. 557-591.

[13] Farhi, Emmanuel, and Tirole, Jean, (2012), “Collective Moral Hazard, Maturity Mismatch, and Systemic Bailouts”, *American Economic Review*, 102, issue 1, p. 60-93.

[14] Freixas, Xavier, Parigi, Bruno, and Rochet, Jean-Charles, (2000), “Systemic Risk, Interbank Relations and Liquidity Provision by the Central Bank”, *Journal of Money, Credit & Banking*, 32, issue 3, p. 611-38.

[15] Freixas, Xavier, Parigi, Bruno, and Rochet, Jean-Charles, (2004), “The Lender of Last Resort: A Twenty-First Century Approach”, *Journal of the European Economic Association*, 2, issue 6, p. 1085-1115.

[16] Gennaioli, Nicola, Martin, Alberto, and Rossi, Nicola, (2014), “Sovereign Default, Domestic Banks and Financial Institutions”, *The Journal of Finance*, 69, issue 2, p. 819-866.

[17] Horvath, Balint, Huizinga, Harry, and Ioannidou, Vasso, (2015), “Determinants and Valuation Effects of the Home Bias in European Banks’ Sovereign Debt Portfolios”, CEPR Working Paper No. 10661.

[18] Kaufman, George G., (1990), “Lender of Last Resort: a Contemporary Perspective”, Working Paper No. 9008, Federal Reserve Bank of Dallas.

[19] Ongena, Steven, Popov, Alexander, and Van Horen, Neeltje, (2015) "The Invisible Hand of the Government: Moral Suasion during the European Sovereign Debt Crisis", CEPR Working Paper No. 11153.

[20] Papay, John P., Willett, John B. and Murnane, Richard, (2011), "Extending the Regression-Discontinuity Approach to Multiple Assignment Variables", *Journal of Econometrics*, 161, issue 2, p. 203-207.

[21] Ratnovski, Lev, and Dell'Ariccia, Giovanni, (2012), "Bailouts, Contagion, and Bank Risk-Taking", 2012 Meeting Papers No. 133, Society for Economic Dynamics.

[22] Repullo, Rafael, (2005), "Liquidity, Risk Taking, and the Lender of Last Resort", *International Journal of Central Banking*, 1, issue 2, p. 47-80.

[23] Rochet, Jean-Charles, and Vives, Xavier, (2004), "Coordination Failures and the Lender of Last Resort: Was Bagehot Right After All?", *Journal of the European Economic Association*, 2, issue 6, p. 1116-1147.

Chapter 2

2 Too Connected To Fail: Implicit Guarantees and Political Connections

Abstract

This paper investigates the effects of political connections on implicit guarantees of state aid for large European banks. By looking at the share of a bank's board members previously employed in the domestic government in a sample of large European banks over the period 2007-2012, I find evidence of a strong positive effect of political connections on implicit guarantees, proxied by credit rating uplifts, *i.e.* the differential between the long-term and the stand-alone rating of the financial institution. Coherently with this result, the effect of political connections on rating uplifts is lower in countries that were most exposed to the sovereign debt crisis in 2010-2011, when implicit guarantees were lowered by public budget constraints. To check whether the expectation of public support embedded into rating uplifts also translated into an actually higher *ex-post* probability of receiving state aid, I also estimate the effect of political connectedness on the likelihood of receiving public support over the sample period. As a result, I find that political connections actually increase the likelihood of receiving state aid. Thus, results suggest that political connections play a significant role as a determinant of implicit guarantees for bank debt in Europe. On top of being *too-big-to-fail*, some European banks appear to be also *too-connected-to-fail*.

2.1 Introduction

When financial institutions - especially if large and systemically important - encounter difficulties, governments often intervene to provide support to them. The failure of a large bank can indeed cause severe damages to the whole economy, because of fire-sales, contagion effects and credit disruption. Therefore, averting the risk of large negative shocks that would promptly propagate to the rest of the economy - as happened for instance after the failure of Lehman Brothers in 2008 - provides the main rationale for the use of public funds to avoid bank failures. As a result, many banks benefit from implicit guarantees for their liabilities provided by national governments.

This expectation of public support has, as a direct consequence, an impact on banks' funding cost. Indeed, when investors believe that the government will not allow large banks to fail, they will price banks' securities accordingly. Anticipating that they will not bear the cost of bank insolvency - at least not with certainty - lowers the return demanded by investors, if compared to the one they would ask in the absence of the subsidy and given the risk profile of the bank. The financial companies that benefit the most from this subsidy are banks that are large and systemically important: these banks are known as *too big to fail* (TBTF).

Empirical evidence of the existence of an implicit subsidy to TBTF banks has been provided by several authors in the literature. O'Hara and Shaw (1990) exploit the announcement made by the Comptroller of the Currency in 1984 that 11 banks were *'simply too big to fail'* and find evidence of a positive wealth effect for bank shareholders of such banks, while smaller banks were negatively affected. Similarly, Kane (2000) documents that megamergers in the US banking sector - in the period from 1991 to 1998 - positively affect the stock value of the acquirer with the effect being increasing in the size of the target bank.

Relatedly, Penas and Unal (2001) analyze 65 mergers in the banking industry and show that bondholders of both institutions involved in the merger obtain positive returns around the announcement date, after which credit spreads also tend to fall. Both the effects are positively associated with the size of the bank resulting from the merger. Evidence of a flatter relationship between risk and funding cost is also provided by Morgan and Stiroh (2000) that look at the relationship between bond spreads and ratings in a sample of more than 4000 bonds issued between 1993 and 1998. More recently, Acharya, Anginer and Warburton (2015) examine bonds traded in the US between 1990 and 2012 and - employing multiple measures of bank size and risk - find that bond spreads are not sensitive to risk for very large financial institutions.

Exploiting the time heterogeneity of government guarantees over different periods between 1983 and 1991, Flannery and Sorescu (1996) show how the spread on subordinated debt becomes more sensitive to risk when government guarantees are lower, and provide additional evidence of the TBTF phenomenon. Similarly, Sironi (2003) analyzes a sample of subordinated notes and debentures issued in 1991-2000, and finds that credit spreads became more sensitive to the risk profile of the bank - proxied by its stand-alone rating - in the second half of the 90s, consequently "to the joint effect of the loss of monetary policy by national central banks and the public budget constraints imposed by the European Monetary Union." (p. 1).

Alternatively to bond yields, another way to estimate the impact of the TBTF subsidy on banks' funding cost is to look at the risk premium banks pay on uninsured deposits. Jacewitz and Pogach (2013) use this methodology and find that, between 2007 and 2008, the largest US banks benefit from a 39-bps lower risk premium on uninsured deposits when compared to their smaller counterparts. A similar results is also documented by Imai (2006) in a sample of weekly deposit bank rates in Japan. However, as pointed out by Acharya, Anginer and Warburton (2015), this methodology can suffer from a number of limitations since large banks usually offer a different set of services and products than small banks, and this can explain the differentials in deposit rates.

A third approach that has been used to estimate the impact of implicit guarantee is to look at bank ratings. Although rating agencies are not always correct about their assessments, investors do look at ratings when making their investment decisions. Therefore, credit ratings have a considerable effect on banks' funding cost, as shown by Morgan and Stiroh (2005). Moreover, since rating agencies release both long-term and stand-alone ratings, the differential between the former and the latter - *i.e.* the credit rating uplift - can be considered as a reliable proxy for the probability a bank has of receiving external support. This approach has been followed by Rime (2005) that examines a large set of banks from 21 different countries, rated both by Moody's and by Fitch between 1999 and 2003, and documents a positive effect of bank size on credit rating uplifts, the size of the effect being inversely related to banks' stand-alone rating. A similar result is presented by Ueda and Weder di Mauro (2012) that quantify the value of the implicit subsidy embedded in the rating uplifts in 60 to 80 basis points. Focusing on a subsample of European countries, Schich and Lindh (2012) get comparable results.

Thus, economists exhaustively documented the existence of an implicit subsidy for banks that lowers their funding cost and can therefore generate a number of relevant inefficiencies by: i) distorting bankers' incentives to undertake risks; ii) hampering competition in the banking sector; iii) generating an adverse feedback loop between sovereign and bank debt value.

Still relatively unexplored, however, are the determinants of implicit guarantees, other than bank size. The aim of this paper is therefore to analyze another factor that possibly affects implicit guarantees: political connections. By using a continuous and relatively new measure of political connectedness - given by the share of a bank's board members previously employed by the government of the country where the bank is located - I estimate the impact of political connections on implicit guarantees, the latter being measured by credit rating uplifts. Using data from 2007 to 2012 for a sample of European banks rated by Moody's, I document a positive effect of the aforementioned measure of political connections on ratings' uplifts, once controlling for bank size - thus also testing the TBTF hypothesis - and other bank-specific observables.

In support of this *too-connected-to-fail* (TCTF) hypothesis, I find this effect to be lower during the 2010-2011 sovereign debt crisis in those countries that were most severely affected by the latter. In such countries, austerity measures imposed by the European Union indeed reduced governments' ability to provide implicit guarantees to domestic banks, thus lowering the effects of political connectedness on credit rating uplifts. Then, I separately test the extensive and the intensive margin effects of political connections and show that both are relevant: being connected matters, as well as the degree of connectedness. Finally, I use data on state aid in Europe and document that the higher expectation of public support embedded into credit rating uplifts actually translates into a higher *ex-post* probability of receiving state aid for banks with more politically connected directors.

So, this paper also nests the relatively small strand of economic literature on political connections and state aid. Among these studies, Faccio, Masulis and McConnell (2006) use a large sample of companies from 35 countries and find evidence of an increase in the likelihood of receiving a bailout over the period 1997-2002 for politically connected firms, that is, companies with at least a former politician sitting in the board of directors. Focusing on the US banking sector, Thomas, Blau and Brough (2013) use a similar bivariate measure of political connectedness together with a continuous measure given by the amount of banks' lobbying expenditures and document that politically connected banks - and banks that spent more in lobbying - were more likely to receive a bailout during the Troubled Assets Relief Program (TARP) and to receive it sooner than their unconnected counterparts.

The rest of the paper is organized as follows: section 2 describes the dataset and the methodology; results from the analysis are provided in section 3; finally, section 4 concludes.

2.2 Data and Methodology

The dataset used for the analysis is obtained combining four different sources: i) bank-specific data on the composition of the board of directors of European banks, as of 2007, are from BoardEx; ii) long-term and baseline credit ratings are hand-collected from Moody's website; iii) state aid approved by the European Commission come from the European Commission's website; iv) a set of bank-specific observables is obtained from Bankscope.[3]

As regards the key independent variable for the analysis - *i.e.* the measure of political connectedness - this is obtained from BoardEx, a dataset containing information on Board Members and Senior Managers of both private and quoted companies throughout the world. After identifying all European banks - companies identified in BoardEx either as '*Banks*' or as '*Speciality & Other Finance*' - I reconstruct the composition of the board of each bank in 2007. Then I collect the available information about each of the board members of the bank, and in particular: age, education, gender, nationality and previous employments. Using this last piece of information, I identify board members who were previously employed either as a board member or a senior manager by the government - or by a company entirely owned by the government - of the country where the bank is located. I identify these directors as *'politically connected'*. Finally, I compute the ratio of politically connected board members on the total number of board members in 2007, thus getting a continuous measure - ranging from 0 to 1 - of a bank's degree of political connectedness. Using the information on age, education and gender of directors, I also construct a set of bank-specific controls that are: the average *age* of the directors, the share of *male* directors and the share of directors with a *master*'s degree.

The measure of political connectedness I propose in this paper is relatively new in the literature since, in most of the previous studies, political connectedness is treated

[3]Data on the board composition are from 2007 in order to mitigate the possible endogeneity between board composition, bank performance and state aids in the following years, and especially during the crisis.

as a binary variable that equals one if a company has some political connections and zero otherwise. This approach does not allow to look at the intensive margin effect of political connectedness, and poses some identification concerns since connected companies are likely to be systematically different from the unconnected ones. Moreover, to build this measure, I consider as politically connected not only those directors who were former politicians, but all those directors who held a relevant position in the domestic government and therefore developed a network of contacts with local politicians. Even if this methodology can lead to overestimate the number of directors that are effectively linked to the political world of the country, considering as connected only those board members with former political experience would not capture all of those relationships that come from directors' network, thus leading to a systematic underestimation of political connectedness.

Once obtained the bank-specific measure of political connectedness, I merge this information with Moody's ratings. In particular I look for the long-term and the baseline rating for each of the bank in the dataset. As regards the former, I consider the highest among the long-term issuer rating, the senior unsecured rating and the long-term bank deposit rating at the end of each year. Such long-term rating is the assessment made by Moody's about the ability of the bank to honor its liabilities, and factors in the probability of external support the bank might receive, if needed. The baseline one, instead, is the stand-alone rating of the bank that abstracts from the likelihood of receiving external support and can be therefore considered as a measure of a bank's financial strength and its intrinsic capacity to repay its debt. The difference between these two ratings - once converted into two numeric variable ranging from 1 (C) to 21 (Aaa) - is commonly known as the credit rating uplift and can be considered as a measure of implicit guarantees.

The information about banks' board composition and ratings is then merged with a set of yearly bank-specific observables from Bankscope. These include: total assets (*totass*), net income (*netinc*), return on average assets (*roaa*), return on average equity (*roae*) and share of tier 1 capital (*tier1*). The merger of these three sources of data leads to a panel dataset containing information on 103 banks from 25 different

Panel A: Rated banks' distribution by country.

Austria	3	Malta	1
Belgium	2	Netherlands	5
Cyprus	2	Poland	6
Czech Republic	1	Portugal	4
Denmark	6	Republic of Ireland	2
Finland	1	Romania	1
France	9	Slovakia	1
Germany	16	Spain	10
Greece	5	Sweden	4
Italy	10	UK - England	7
Lithuania	1	UK - Scotland	1
Total: 97			

countries of the European Union. Removing banks fully owned by the government and three outliers with more than 50% of politically connected board members, leads to final dataset containing 97 banks from 22 countries.

Panel A provides the distribution of rated banks by country. The countries most represented in the dataset are Germany, France, Italy, Spain and Great Britain, where around 50% of the banks in the sample are located.

The distribution of the share of politically connected directors among the bank in the sample is represented in Figure 1. As one would expect, frequency is decreasing in the measure of political connectedness, *i.e.* *conn*, and the most populated bin is the first one, that includes 23 banks with no directors previously employed by the government. The median of *conn* is 11% and 75% of banks have less than 20% of connected directors.

Summary statistics about the board composition of banks are presented in Panel B. To compare more and less connected banks, I split the sample into two groups. The first one includes banks with less than 11% of politically connected board members -

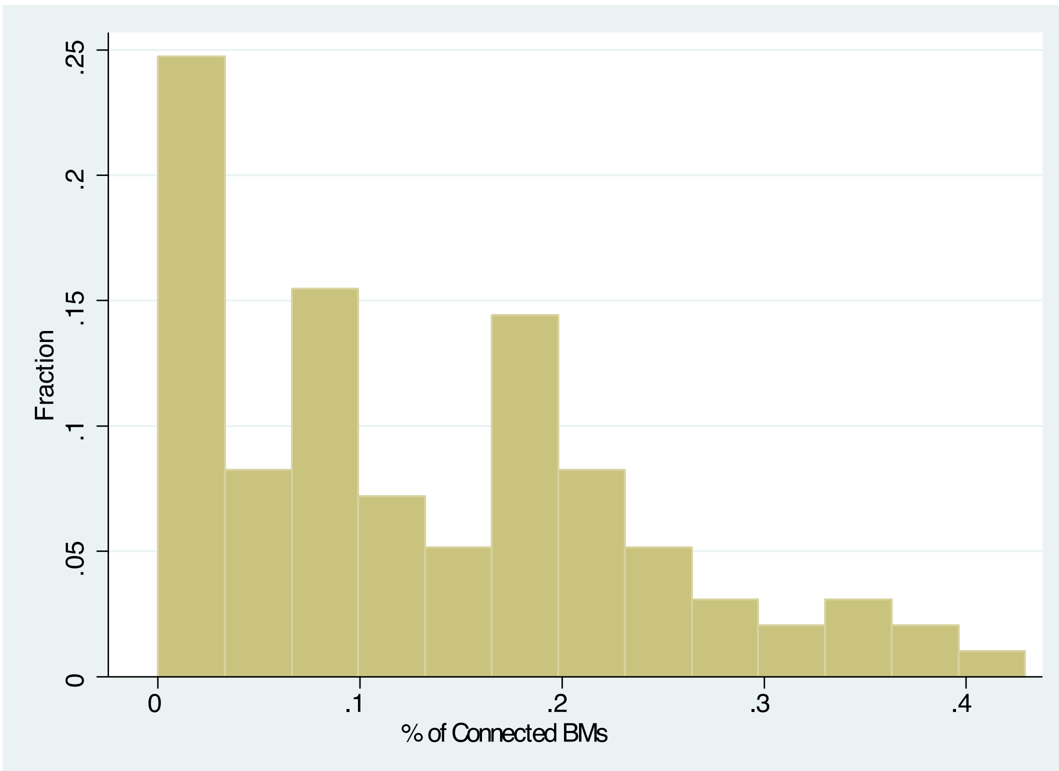

Figure 1: Distribution of the measure of political connectedness

Panel B: Board characteristics for rated banks

	totBM	*male*	*master*	*age*
	Least connected banks			
N	47	47	47	46
mean	12.976	0.911	0.159	55.296
p25	9.25	0.845	0	52.303
p50	13	0.953	0.122	54.953
p75	15.416	1	0.255	58.851
	Most connected banks			
N	50	50	50	50
mean	16.925	0.919	0.244	56.803
p25	12.333	0.861	0.087	54.75
p50	15.916	0.933	0.232	57.019
p75	20.583	1	.411	59.433
	Full sample			
N	97	97	97	96
mean	15.012	0.915	0.203	56.082
p25	11.667	0.857	0.014	53.462
p50	14	0.940	0.168	56.187
p75	18.083	1	0.306	59.253

since 11% is the median of *conn* in the sample - whilst the second one includes banks with more than 11% of connected directors. Then I present the mean, the median, the 25^{th} and the 75^{th} percentiles for the total number of board members (*totBM*), their age (*age*), the share of male directors (*male*), and the share of directors with a master's degree (*master*).

Panel C contains the summary statistics about bank-specific observables, as of 2007, such as the logarithm of total assets (*totass*), net income (*netinc*), share of tier 1 capital (*tier1*), return on average assets (*roaa*) and return on average equity (*roae*). As in Panel B, I report separately the statistics for the most and the least connected banks.[4] The table also includes the t-statistics for the differences between

[4]The two groups are sligthly umbalanced since some data from 2007 are missing in Bankscope.

Panel C: Key financial data for rated banks.

	totass	*netinc*	*tier*	*roae*	*roaa*
		Least connected banks			
N	43	43	39	43	43
mean	18.059	1,358,019	8.07	15.295	1.059
p25	16.779	135,500	6.98	10.263	0.556
p50	18.075	593,100	7.44	16.061	0.838
p75	19.116	1,444,900	8.87	19.293	1.261
		Most connected banks			
N	47	47	45	47	47
mean	18.759	1,898,037	8.385	13.717	0.911
p25	17.591	305,000	6.8	8.763	0.341
p50	19.107	820,572.1	8.06	14.957	0.805
p75	20.035	2,030,428	9.4	18.956	1.36
		t-stat ($\mu_{conn>.11} - \mu_{conn<.11}$)			
	2.03**	0.98	0.71	0.85	0.82
		Full sample			
N	90	90	84	90	90
mean	18.424	1,640,028	8.237	14.471	0.982
p25	17.172	228,887.3	6.85	10.262	0.458
p50	18.513	643,350	7.755	15.357	0.831
p75	19.797	1,644,100	9.25	19.272	1.28

the means of each variable in the two groups. None of this differences is statistically different from zero, except for the difference in total assets that is different from zero at the 95% confidence level. Most connected banks tend to be also larger than the least connected ones.

As regards Moody's ratings, these are converted into a numeric variables ranging from 1, when Moody's assessment is C, to 21 in case of a triple A (Aaa). Then I take the difference between the long-term rating and the baseline one, thus computing the credit rating uplift. Figure 2 reports the trends of the average long-term rating and the average uplift, over the 2007-2012 period. Not surprisingly, long-term ratings are constantly decreasing over the sample period, since the global financial crisis

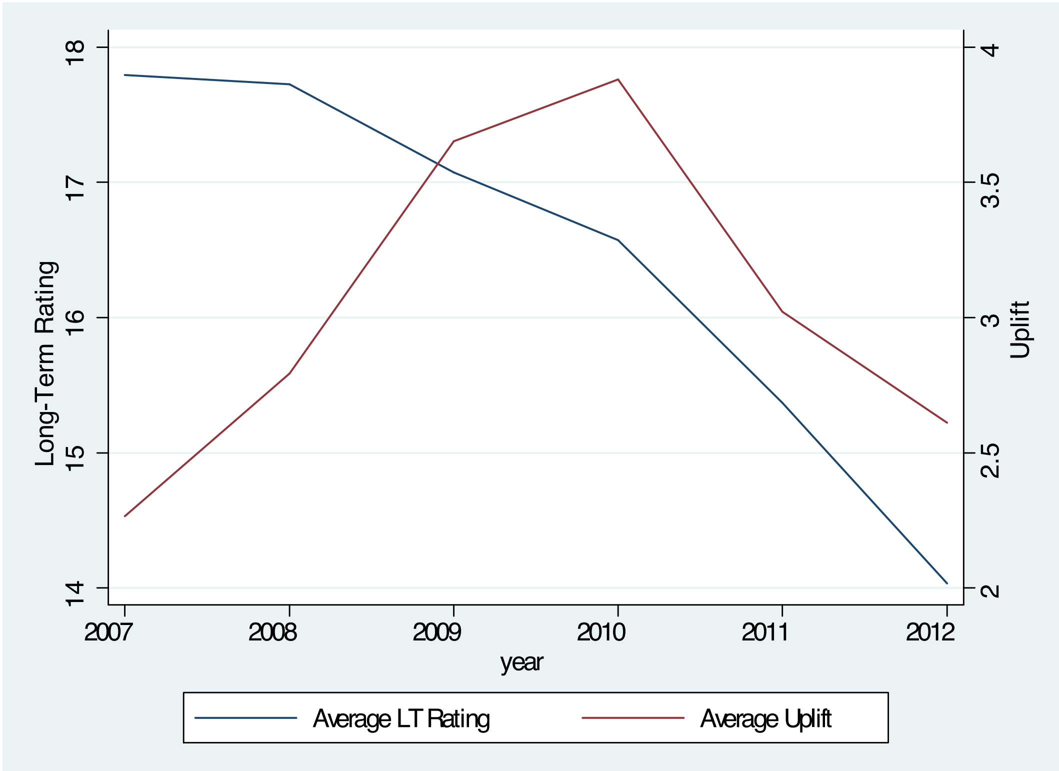

Figure 2: Trends of Ratings

lowered the expectations about banks' ability to repay their debt. At the same time, rating uplifts tend to increase during the onset of the financial crisis, due to both the lowering in the average baseline ratings and the higher probability of state aid. In 2010 - when the European sovereign debt crisis erupted - and 2011 - when also the regulation about bail-ins was announced - uplifts start decreasing.

With the above-described dataset, I first estimate effect of political connections on rating uplifts, as a proxy of implicit guarantees, using the following model:

$$uplift_{it} = \alpha + \theta_{ct} + +\beta conn_i + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \tag{1}$$

where $conn_{ic}$ is the bank-specific and time-invariant measure of political connectedness of bank i, x_i' is a set of time-invariant bank-specific explanatory variables - *i.e.* the characteristics of the board, other than $conn$ - and z_{it}' the bank-specific and time-variant controls, such as $totass$, $netinc$, $tier1$ *etc.*. θ_{ct} captures country-time fixed effects, that is, country-year-specific unobservables such as public debt and public budget constraints that are likely to contemporaneously affect the rating uplifts of all the banks located in a specific country at a given date.

I estimate this model under different specifications - that is, including and excluding fixed effects and controls - and assuming a random-effects model. The coefficient of $conn$ is therefore estimated by GLS with standard errors clustered at the bank level and, if positive, provides evidence in support of the TCTF hypothesis. A positive coefficient for $totass$ would similarly documents the existence of TBTF subsidies.

In the second model I estimate, uplifts are decomposed and the long-term rating of banks is used as dependent variable, while baseline ratings are used as controls. Indeed, changes in the uplift can be due to changes in both the baseline and the long-term ratings. Moreover, the uplift itself might depend on the baseline rating of the bank, being for instance larger for banks with a lower stand-alone rating. Therefore, using baseline ratings as a control that captures banks' financial strength allows me to better isolate the effects of political connections - as well as the one of size - on long-term ratings. The model estimated in this way is the following:

$$lt\_rat_{it} = \alpha + \theta_{ct} + \beta conn_i + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \tag{2}$$

where lt_rat_{it} is the long-term rating of bank i in year t, and z_{it}' now includes also the baseline rating, namely bs_rat_{it}. To estimate the coefficient of $conn$ I use again GLS with clusters at the bank-level and including either country and time fixed effects or country-time fixed effects, depending on the specification.

The β coefficient estimated using (2) is the average effect of political connectedness on long-term ratings. This effect is however likely to vary depending on the baseline rating of the bank itself, since the long-term rating has an upper limit at *Aaa*. For instance, in the extreme case in which the baseline rating of the bank is

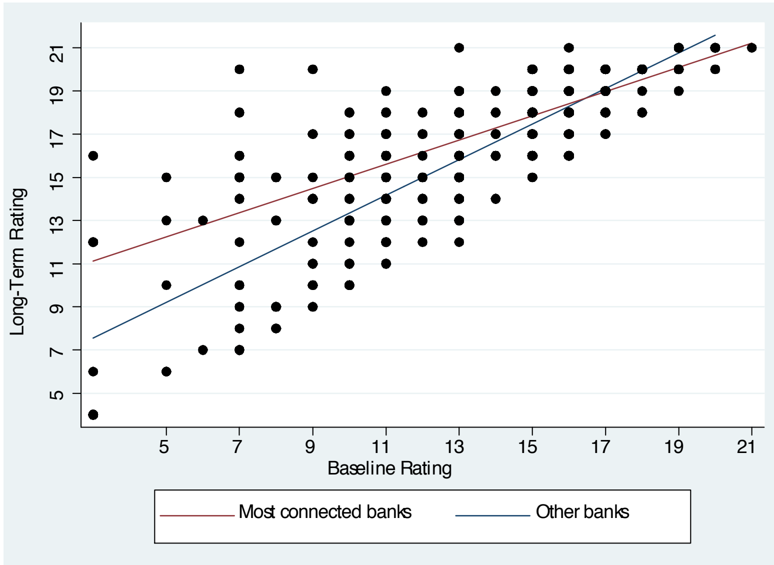

Figure 3: Relationship between long-term and baseline ratings.

Aaa the rating uplift is, by definition, equal to zero and political connections play no role. Therefore, one should expect banks with low baseline ratings to benefit from larger uplifts and the effect of political connections to be stronger among those banks. Preliminary evidence of this heterogeneous effect of political connectedness on long-term ratings, depending on banks' baseline rating, is presented in Figure 3, where I represent the relationship between the two types of ratings for the 30 banks with the highest share of connected directors in my sample and for the rest of the banks, separately.

As one can notice, most connected banks tend to benefit from higher long-term ratings than the rest of the banks in the sample, this effect being decreasing in their financial strength, due to the upper limit of ratings. To account for this heterogeneity in the effect of political connections depending on banks' baseline ratings, I therefore estimate the following model:

$$lt\_rat_{it} = \alpha + \theta_{ict} + \beta conn_i + \psi conn_i \times bs\_rat_{it} + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \tag{3}$$

where ψ captures the marginal variation in the coefficient of political connectedness on long-term ratings, as baseline ratings increase. If the TCTF hypothesis holds, this coefficient should be negative since an increase in $conn$ has a dual effect on the line representing the relationship between baseline and long-term ratings: first, it increases its intercept and, second, it reduces its slope, given the upper limit on ratings. Importantly, since a bank's baseline rating varies over time, I can estimate ψ in (3) including also bank fixed effects (θ_{ict}), thus partially addressing endogeneity problems due to banks' specific unobservables.

The fourth model I estimate to test the TCTF hypothesis exploits the exogenous decrease in implicit guarantees provided by domestic governments during the European sovereign debt crisis of 2010-2011. If the coefficient of $conn$ estimated using (1) and (2) correctly captures the effect of political connectedness on implicit guarantees for bank debt, one should expect this coefficient to be lower during the crisis in those countries whose budgeting constraints were most binding. Therefore, I create a dummy variable, $crisis$, that equals 1 for 6 countries - Cyprus, Greece, Ireland, Italy, Spain and Portugal - in 2010 and 2011, and I estimate again (1) and (2) including both the $crisis$ dummy and its interaction with both $conn$ and $totass$. The model I estimate becomes:

$$\begin{aligned} Y_{it} \;=\;& \alpha + \theta_{ct} + crisis + \beta conn_i + \theta conn_i \times crisis + \\ & + \phi totass_{it} \times crisis + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \end{aligned} \tag{4}$$

where Y_{it} is either the credit rating uplift or the long-term rating of bank i in year t, depending on the different specification I estimate. According to the hypothesis that political connections increase implicit guarantees, when a country experiences a crisis of its sovereign debt such guarantees are lower and the effects of political connections should be reduced. As a result, I expect the coefficients θ to be negative. The same does not necessarily happens with ϕ - the differential effect of size on implicit guarantees during the sovereign debt crisis - since financially constrained governments might prioritize TBTF banks to the detriment of connected banks.

Moreover, large banks can benefit from implicit guarantees provided by supranational authorities since these might have incentives to support systemically important banks whose failure would put at risk the whole banking system. The same, of course, does not happen with political connections, since they plausibly provide implicit guarantees only at the national level.

Then, as anticipated in the previous section, I exploit the continuous nature of the measure of political connectedness in order to look, separately, at the extensive and intensive margin effects of political connections on implicit guarantees. To do so, I create a dummy, D_{conn}, that equals 1 when banks have at least one connected board member and then I replicate the estimates of (1) and (2) after replacing *conn* with D_{conn}; then I estimate (1) and (2) using only the subset of politically connected banks, that is, those banks with *conn* strictly greater than zero. The two estimated models are:

$$Y_{it} = \alpha + \theta_{ct} + \beta D_{conn} + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \tag{5}$$

and

$$Y_{it} = \alpha + \theta_{ct} + \beta conn_i + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \; \forall i : conn_i > 0 \tag{6}$$

where Y_{it} is either the rating uplift or the long-term rating, depending on the specification of the regression. The estimated coefficient β in (5) can be now interpreted as the extensive margin effect of political connections on implicit guarantees. If positive, it documents that connected banks benefit from larger implicit guarantees when compared to banks with no connected directors. In (6), instead, the β coefficient captures the effect of political connectedness at the intensive margins. If positive and significant, it suggests that the increase in rating uplifts induced by political connections increases with the magnitude of political connectedness.

To complete the analysis, I test whether the *ex-ante* higher expectation of public support for connected banks embedded in rating uplifts actually leads to a higher *ex-post* probability of receiving such support from the government. So, I merge the dataset with data on state aid received by European banks during the 2007-2012

Panel D: Banks distribution by country.

Austria	9 (2)	Lithuania	1 (0)
Belgium	6 (3)	Luxembourg	1 (0)
Cyprus	3 (1)	Malta	2 (0)
Czech Republic	1 (0)	Netherlands	12 (2)
Denmark	8 (0)	Poland	7 (0)
Finland	3 (0)	Portugal	5 (2)
France	20 (5)	Republic of Ireland	3 (3)
Germany	30 (9)	Romania	1 (0)
Greece	7 (5)	Slovakia	1 (0)
Hungary	1 (0)	Spain	11 (4)
Italy	23 (4)	Sweden	10 (0)
Latvia	2 (0)	UK	29 (0)
Total**: 196**			

Banks that received state aid in parenthesis.

period. These data are collected from the website of the European Commission, where all state aid cases approved by the Commission are reported (with NACE code K). After identifying all banks in my sample that received public support, I construct a binary variable $state\_aid_{it}$ that equals 1 if bank i received state aid in year t and zero otherwise. To perform this analysis, I do not need to restrict the sample to banks currently rated by Moody's, and therefore I enlarge the sample including all banks I find in BoardEx with non-missing data from Bankscope. As a result, I end up with 196 banks from 25 different countries, with 40 different banks that received public support, in the form of loans, explicit guarantees or capital injections at some point in time. The distribution of banks by country is provided in Panel D. In parentheses, I report the number of banks that received state aid in each country.

Panel E reports the summary statistics of board-specific observables, as of 2007, relative to the group of banks that received and did not receive state aid, separately. The statistics relative to the full sample are also included. For each of the variables, Panel E: Board characteristics.

	totBM	*male*	*master*	*age*
Banks that did not receive state aid				
N	156	156	156	153
mean	11.647	0.928	0.196	54.976
p25	6.375	0.889	0	52.364
p50	11.083	0.959	0.159	55.101
p75	15.667	1	0.299	58.747
Banks that received state aid				
N	40	40	40	40
mean	15.633	0.923	0.179	57.249
p25	12.375	0.873	0.007	54.779
p50	14.5	0.934	0.149	57.667
p75	20.417	1	0.285	59.661
Full sample				
N	196	196	196	193
mean	12.461	0.927	0.193	55.447
p25	7.167	0.888	0	52.669
p50	12	0.952	0.157	55.994
p75	16.083	1	0.292	59

namely *totBM, male, master* and *age,* the mean, the median, 25^{th} and the 25^{th} percentiles are reported.

Panel F reports the summary statistics for the other bank-specific time-varying controls - *totass, netinc, tier, roae* and *roaa* - for banks that received and did not receive state aid, and for the full sample, separately. The t-statistics of the mean differences between the two groups are also included in the Panel.

Preliminary evidence from this summary statistics suggest that banks receiving public support are, on average, larger in terms of total assets. This would be coherent with the TBTF hypothesis. As one would also expect, banks receiving state aid exhibit a lower return on average assets and average equity, given the difficulties these banks faced and that required government intervention.

Panel F: Key financial data.

	totass	*netinc*	*tier*	*roae*	*roaa*
	Banks that did not receive state aid				
N	142	142	89	142	142
mean	16.596	920,473.3	10.359	16.182	2.097
p25	15.188	44,234.12	7.2	9.418	0.592
p50	16.535	163,306.2	8.59	15.499	0.985
p75	18.075	635,200	10.5	21.97	2.201
	Banks that received state aid				
N	37	37	33	37	37
mean	18.765	1,416,346	7.696	12.956	0.931
p25	17.817	335,300	6.62	7.259	0.392
p50	18.903	787,600	7.22	13.896	0.763
p75	19.807	1,704,000	8.7	18.925	1.053
	t-stat ($\mu_{stateaid=1} - \mu_{stateaid=0}$)				
	5.52^{***}	1.27	-1.88^{*}	-1.25	-1.84^{*}
	Full sample				
N	179	179	122	179	179
mean	17.045	1,022,972	9.639	15.515	1.856
p25	15.554	50,800	7	9.313	0.488
p50	17.071	215,400	8.1	15.137	0.907
p75	18.818	857,000	9.76	21.258	1.842

So, finally, I estimate the following model:

$$state\_aid_{it} = \alpha + \theta_{ct} + \beta conn_i + x_i'\gamma + z_{it}'\delta + \epsilon_{ict} \tag{7}$$

where x_i' and z_i' are the usual set of controls, and θ_{ct} captures country-year fixed effects. The β coefficient in this model can be interpreted as the effect of political connections on the likelihood of receiving state aid. It is estimated by GLS with robust standard errors clustered at the bank level. As a robustness check, I also replicate the estimates assuming a logistic model, whose results are provided in the Appendix.

2.3 Results

2.3.1 Political connections and implicit guarantees

In the first set of regressions I estimate the impact of political connections on credit rating uplifts. The GLS estimates from a panel regression estimating (1) are reported in Table 1, under 6 different specifications, from the least to the most conservative. In column 1 the coefficient of *conn* on rating uplifts is estimated with no controls and no fixed effects. In column 2, country and time fixed effects are included, separately. The estimates obtained when adding country-time fixed effects that control for time-varying country-specific macroeconomic unobservable are then reported in column 3. Finally, in columns 4, 5 and 6, I report the results obtained after adding a set of bank-specific controls with and without fixed effects. Controls include: (i) board-specific characteristics of the bank, as of 2007, such as: the share of male directors (*male*), the average age of the board members (*age*) and the share of directors with a master's degree (*master*); (ii) bank-specific time-variant observables, including the logarithm of total assets (*totass*), net income (*netinc*) in thousand of dollars, the share of tier 1 capital (*tier*), the return on average equity (*roae*) and on average assets (*roaa*). In all regressions standard errors are clustered at the bank level to allow within-bank serial correlations in the error terms. I also estimate the same specifications by OLS, whose coefficients are reported in Table 7, in the Appendix and are comparable to the GLS ones, even if slightly lower.

The coefficient of *conn* is statistically significant, at least at the 90% significance level, in all the specifications except (6), when it turns being not significant, but still with a p-value lower 0.15. As regards the magnitude of the effect of political connectedness, it ranges from 2.7 (column 3) to 4.2 (column 1). Therefore a10% increase in the share of politically connected board members translates into an increase of around 0.3 to 0.4 notches in the credit rating uplift of the financial institution, approximately. Except for political connections, none of the other regressors exhibit a coefficient that is statistically significant in at least two different specification, including total assets, that proxies for bank size. However, this does not rule out the

Table 1: Results from a panel regression estimating equation (1).

VARIABLES	(1) *uplift*	(2) *uplift*	(3) *uplift*	(4) *uplift*	(5) *uplift*	(6) *uplift*
totass				0.268^{***}	-0.178	-0.188
				(0.0959)	(0.152)	(0.142)
male				-1.960	-2.312	-2.136
				(1.654)	(1.801)	(1.865)
age				-0.126^{***}	0.00667	0.00421
				(0.0388)	(0.0464)	(0.0479)
master				-2.644^{**}	-0.628	-0.342
				(1.044)	(1.093)	(1.098)
tier				0.0151	-0.00452	0.000962
				(0.0107)	(0.0168)	(0.0105)
netinc				$-4.60e^{-08}$	$-3.67e^{-08}$	$-1.98e^{-08}$
				($4.46e^{-08}$)	($3.91e^{-08}$)	($4.55e^{-08}$)
roaa				0.0560	0.122	-0.319^{**}
				(0.0983)	(0.0991)	(0.129)
roae				-0.000534	-0.00101	0.00129
				(0.00143)	(0.00145)	(0.00105)
conn	4.162^{**}	2.897^{*}	2.723^{*}	4.887^{***}	3.871^{*}	3.095
	(1.637)	(1.498)	(1.624)	(1.658)	(1.983)	(2.018)
Constant	2.533^{***}	3.323^{***}	0.884^{***}	6.690^{**}	8.433^{**}	6.363
	(0.242)	(0.455)	(0.201)	(3.382)	(4.264)	(4.288)
Observations	549	549	549	489	489	489
R^2 (overall)	0.032	0.435	0.556	0.149	0.427	0.582
Number of banks	97	97	97	92	92	92
Country FE	NO	YES	NO	NO	YES	NO
Year FE	NO	YES	NO	NO	YES	NO
Country-Year FE	NO	NO	YES	NO	NO	YES

Robust standard errors in parentheses (clustered at the bank level). *** $p<0.01$,** $p<0.05$, * $p<0.1$.

existence of TBTF subsidies in my sample, since size can have two counteractive effects on credit rating uplifts: (i) it enlarges the uplift by increasing the long-term rating higher; (ii) it reduces the uplift by increasing the baseline rating. As a result, the coefficient of *totass* on uplifts might be statistically not significant.

Table 2: Results from a panel regression estimating equation (2).

VARIABLES	(1) *lt_rat*	(2) *lt_rat*	(3) *lt_rat*	(4) *lt_rat*	(5) *lt_rat*	(6) *lt_rat*
bs_rat	0.751***	0.647***	0.490***	0.614***	0.525***	0.438***
	(0.0602)	(0.0645)	(0.0515)	(0.0544)	(0.0652)	(0.0605)
totass				0.573***	0.410***	0.450***
				(0.0745)	(0.107)	(0.110)
conn	3.531**	3.152***	3.321***	2.610**	2.934**	2.063*
	(1.376)	(1.048)	(1.103)	(1.167)	(1.201)	(1.173)
Constant	5.919***	8.361***	8.842***	3.991	1.857	0.278
	(0.900)	(0.889)	(0.827)	(2.606)	(3.503)	(3.525)
Observations	549	549	549	489	489	489
R^2 (overall)	0.628	0.821	0.890	0.715	0.826	0.899
Number of banks	97	97	97	92	92	92
Controls	YES	YES	YES	YES	YES	YES
Country FE	NO	YES	NO	NO	YES	NO
Year FE	NO	YES	NO	NO	YES	NO
Country-Year FE	NO	NO	YES	NO	NO	YES

Robust standard errors in parentheses (clustered at the bank level). *** $p<0.01$,** $p<0.05$, * $p<0.1$.

Controls include: male, age, master, tier, netinc, roaa, roae.

Exactly for this reason, in the second set of regressions, I decompose the uplifts and use as a dependant variable the long-term rating alone, thus including the baseline rating as a control. This specification allows me to look at the effects of size and political connections, while controlling for the financial strength of the bank embedded in the baseline rating. Actually, this model is nothing but the same of the first one, in which I do not impose a coefficient of 1 to baseline ratings. The estimates of equation (2) are reported in Table 2. As before, in the first three columns I do not include controls, and report the estimates obtained: (i) with no fixed effects; (ii) with country and time fixed effects; (iii) with country-time fixed effects. In specifications 4, 5 and 6 the same estimates are reported, after adding the aforementioned bank-specific controls. The same specifications are also estimated by OLS, whose coefficients are reported in Table 8, in the Appendix.

The coefficient of *conn* is now significant and stable under all the different specifications I use to estimate equation (2): a 10% increase in the share of connected directors increase the long-term rating by around 0.2-0.3 notches. As before, none of the other controls appears as significant in all regressions - they are omitted in the table - except for baseline ratings and total assets. Not surprisingly banks with higher baseline ratings - and therefore characterized by higher financial strength - have also higher long-term ratings. Moreover, the coefficient on bs_rat is lower than one, which implies that uplifts tend to be decreasing in baseline ratings. The positive and highly significant for *totass* confirms the TBTF hypothesis, according to which large banks benefit from higher implicit guarantees for their debt than small banks. This effect, however, does not offset the one of political connections, and the two coexist at the same time.

A further confirmation of this result comes from the estimates of equation (3), that accounts for the upper limit on long-term ratings including an interaction term between baseline ratings and political connections. GLS estimates of (3) are provided in Table 3. In all specifications I include country-time fixed effects and bank-specific controls. Since a bank's baseline rating varies over time, I also include bank fixed effects in columns 3 and 4, thus controlling for bank specific unobservables and partially overcoming endogeneity concerns. Finally, an interaction term between total assets and baseline ratings is included in 2 and 4.

Under all specifications, even the ones with bank fixed effects, the coefficient of the interaction term between political connectedness and baseline ratings is significant and around -0.4. The coefficient of political connections alone is also largely significant under the first two specifications - it can not be estimated when adding fixed effects - and measures around 8. These two estimates, together, document that political connections increase long-term ratings, with the effect being lower when baseline ratings are higher. For instance, a 10% increase in political connections for a bank with a baseline rating equal to *ba1* increases long-term ratings by around $(8 - 0.4 \times 11) \times 10\% = 0.36$ notches. In the extreme case of a bank with a *Aaa*

Table 3: Results from a panel regression estimating equation (3).

VARIABLES	(1) *lt_rat*	(2) *lt_rat*	(3) *lt_rat*	(4) *lt_rat*
bs_rat	0.522***	0.891***	0.526***	0.482
	(0.0433)	(0.332)	(0.0482)	(0.399)
totass	0.436***	0.700***	1.283***	1.250***
	(0.0973)	(0.256)	(0.374)	(0.480)
conn	8.183***	7.885***		
	(2.553)	(2.549)		
bs_rat×*conn*	-0.456***	-0.429**	-0.399**	-0.403**
	(0.172)	(0.175)	(0.189)	(0.193)
bs_rat×*totass*		-0.0201		0.00236
		(0.0178)		(0.0214)
Constant	-0.280	-5.258	-14.29**	-13.27
	(2.502)	(5.027)	(6.906)	(8.891)
Observations	489	489	489	489
R^2 (overall)	0.902	0.904	0.558	0.557
Number of id	92	92	92	92
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Bank FE	NO	NO	YES	YES

Robust standard errors in parentheses. *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae.

rating, instead, the coefficient is approximately zero - $(8 - 0.4 \times 21) \times 10\% = -0.04$ - since the long-term rating can not be larger than the baseline one, regardless of political connections. The corresponding OLS estimates are reported in Table 10 and are extremely close to the GLS ones even if less significant.

Evidence from this first set of estimates confirms the too-connected-to-fail hypothesis made in this paper: banks with more political connections benefit from larger uplifts - or higher long-term ratings when controlling for baseline ratings - than less connected banks. Evidence from these results also confirm the existence of implicit subsidies to large banks, thus providing additional evidence in support of the TBTF doctrine.

2.3.2 Implicit guarantees during the European sovereign debt crisis

Estimates from the previous subsection show how political connections increase, on average, implicit guarantees proxied by rating uplifts. However, this evidence is not conclusive about the causal impact of political connections on implicit guarantees. In order to partially rule out possible endogeneity concerns, I perform an additional analysis, where I exploit the exogenous nature of the 2010-2011 sovereign debt crisis to estimate the differential impact political connections have in countries that were most severely hampered by the crisis.

If the TCTF hypothesis holds, when the domestic government suffers from a crisis of its sovereign debt, it is less able to provide credible implicit guarantees to the domestic banking sector. Therefore, I do identify 6 countries that were particularly affected by the crisis - namely Cyprus, Greece, Ireland, Italy, Portugal, and Spain - and I create a *crisis* dummy that equals 1 in 2010 and 2011 for banks located into these countries. Then I estimate equation (4) by GLS - and by OLS, whose results are in the Appendix (Table 11) - with clustered standard errors, and look at the coefficient of the interaction term between *crisis* and *conn*. Estimates are provided in Table 4.

All of the specifications now include the aforementioned set of bank-specific observables, whose coefficient are omitted, for brevity. In the first specifications (columns 1 to 4) I use the uplifts as my dependant variable and I estimate (3) with and without fixed effects. In column 4, I provide the estimates obtained when including, in the most conservative specification - that is, the one with country-time fixed effects - also an interaction term between *crisis* and *totass*. Finally, in the last two columns, I report the estimates obtained when using the long-term rating as the dependant variable and including the stand-alone rating as a control.

Table 4: Results from a panel regression estimating equation (4).

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*uplift*	*uplift*	*uplift*	*uplift*	*lt_rat*	*lt_rat*
totass	0.287^{***}	-0.172	-0.176	-0.155	0.456^{***}	0.470^{***}
	(0.0947)	(0.155)	(0.141)	(0.148)	(0.110)	(0.109)
conn	5.367^{***}	4.314^{**}	3.552^{*}	3.544^{*}	2.306^{**}	2.299^{**}
	(1.706)	(2.049)	(2.088)	(2.096)	(1.152)	(1.157)
crisis	1.126^{***}	0.773^{**}	1.047^{**}	3.537	0.230	1.946
	(0.263)	(0.321)	(0.496)	(2.754)	(0.499)	(2.413)
$crisis \times conn$	-3.350^{*}	-3.000^{*}	-3.637^{**}	-2.982^{*}	-1.945^{**}	-1.494
	(1.823)	(1.820)	(1.532)	(1.657)	(0.973)	(1.055)
$crisis \times totass$				-0.135		-0.0931
				(0.145)		(0.120)
Constant	7.086^{**}	8.383^{*}	6.195	5.747	0.198	-0.114
	(3.397)	(4.289)	(4.271)	(4.423)	(3.513)	(3.545)
Observations	489	489	489	489	489	489
R^2 (overall)	0.143	0.428	0.585	0.585	0.900	0.900
Number of banks	92	92	92	92	92	92
Controls	YES	YES	YES	YES	YES	YES
Country FE	NO	YES	NO	NO	NO	NO
Year FE	NO	YES	NO	NO	NO	NO
Country-Year FE	NO	NO	YES	YES	YES	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.
Controls include: male, age, master, tier, netinc, roaa, roae and bs_rat in (5) and (6).

Firstly, the GLS estimator of the coefficient on *conn* is positive and significant in every specification. Its magnitude is comparable to the one obtained when estimating (1) and (2). Estimates also confirm the hypothesis according to which implicit guarantees provided by political connections are lower during the 2010-2011 sovereign debt crisis. The coefficient of the interaction term, $crisis \times conn$, is negative and significant in all specifications but the last one, in which the p-value is however only slightly larger than 0.10. Therefore, the effect of political connections on both the uplifts and the long-term ratings is mitigated in the event of a crisis. As regards the coefficient of $crisis \times totass$, this is also negative but statistically not different from zero, thus suggesting that the sovereign debt crisis had no (or a very low) impact on the TBTF mechanism.

This can have different explanations. For instance, it can depend on the different nature of implicit guarantees provided by political connections and bank size. While the former is likely to be a purely domestic phenomenon, with politically connected banks benefiting from an implicit guarantees provided by national politicians, the latter can provide implicit guarantees also at a supranational level. When large banks - that are too big to be saved by domestic governments - encounter difficulties, supranational entities may have incentives to provide support - either directly or indirectly, by relaxing governments' budget constraints - to the bank in order to avoid its failure, since this can put at risk the stability of the whole banking sector. Alternatively, this can be due to the fact that governments with limited capacity of providing support to the banking sector, efficiently prioritize supporting large banks, to the detriment of connected banks.

2.3.3 Intensive margin vs extensive margin effects of political connections

The continuous measure of the measure of political connectedness I propose in this paper allows me to disentangle the extensive and the intensive margin effects of political connections on implicit guarantees. Firstly, I use a dummy that equals 1 when banks have at least a politically connected director and I estimate equation (5), using credit rating uplifts as the dependant variable. Table 5 reports the estimates obtained under 3 different specifications, from the least to the most conservative, - where I control for country-time fixed effects - all of them including the usual set of bank-specific controls.

The GLS coefficient of D_{conn}, confirms the extensive margin effect of political connectedness: banks with at least a politically connected director exhibit larger

Table 5: Results from a panel regression estimating equation (5).

VARIABLES	(1) *uplift*	(2) *uplift*	(3) *uplift*
totass	0.282***	-0.201	-0.210
	(0.108)	(0.160)	(0.148)
D_{conn}	0.843**	0.734**	0.606**
	(0.406)	(0.305)	(0.286)
Constant	5.266	7.595*	5.761
	(3.331)	(4.148)	(4.145)
Observations	489	489	489
R^2 (overall)	0.128	0.423	0.580
Number of banks	92	92	92
Controls	YES	YES	YES
Country FE	NO	YES	NO
Year FE	NO	YES	NO
Country-Year FE	NO	NO	YES

Robust standard errors in parentheses (clustered at the bank level). ***p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae.

uplifts than non-connected banks. The estimated coefficient is indeed positive and significant under all the specifications presented. It ranges from 0.6 in column 3 to 0.85 in column 1, thus documenting that the credit rating uplift of connected banks is around 0.7 notches larger than the one of banks with no director previously employed by the domestic government. Of course, this extensive margin effect can not be interpreted as causal since the two groups of banks are quite different, both in terms of observables and unobservables.

To test whether the effect of political connectedness on implicit guarantees increases with the degree of political connectedness, I estimate equation (6) on the sub-sample of banks with at least a director previously employed by the government (74 in the sample). The GLS estimator of the coefficient of *conn* can be now interpreted as the extensive margin effect of political connections. Table 6 presents the results from the estimation of (6) under six different specifications, all of them including the usual set of bank-level controls. In columns 1 to 3, I use the uplift as

Table 6: Results from a panel regression estimating equation (6).

VARIABLES	(1) *uplift*	(2) *uplift*	(3) *uplift*	(4) *lt_rat*	(5) *lt_rat*	(6) *lt_rat*
totass	0.172	-0.384*	-0.318	0.578***	0.334**	0.491***
	(0.114)	(0.214)	(0.198)	(0.0888)	(0.141)	(0.138)
conn	5.420***	4.642**	3.753	3.453**	3.779**	2.771*
	(2.013)	(2.323)	(2.469)	(1.414)	(1.487)	(1.498)
Constant	11.02***	14.94***	11.82**	5.904**	6.894**	3.752
	(3.534)	(5.278)	(5.308)	(2.389)	(3.462)	(2.978)
Observations	393	393	393	393	393	393
R^2 (overall)	0.187	0.402	0.558	0.742	0.832	0.914
Number of banks	74	74	74	74	74	74
Controls	YES	YES	YES	YES	YES	YES
Country FE	NO	YES	NO	NO	YES	NO
Year FE	NO	YES	NO	NO	YES	NO
Country-Year FE	NO	NO	YES	NO	NO	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae and bs_rat in (5) and (6).

the dependant variable, whilst in 4 to 6, this is replaced by the long-term rating. As usual, standard errors are clustered at the bank-level.

The estimated coefficient for *conn* - that is, the effect of political connections on implicit guarantees conditional on banks having at least a connected director - is positive and significant in all of the regressions except for the one in column 3, where the p-value is slightly larger than 0.10 (0.129). Evidence from these estimates suggests that implicit guarantees increase with the number of a bank's connected board members. This provides evidence in favor of the existence of an intensive margin effect of connectedness on rating uplifts and long-term ratings.

Putting together the evidence presented in Table 5 and Table 6, show that political connections have both at the extensive and at the intensive margin.[5] Banks with

[5] OLS estimates are provided in Table 12 of the Appendix and confirm these findings, even though OLS coefficients are not always significant.

political connections benefit from larger uplifts between their long-term and baseline ratings, if compared to unconnected banks and this difference increases with the share of a bank's board members previously employed by the domestic government. Such evidence also suggests that a continuous measure of political connection, as the one I use in this paper, might be preferred to the bivariate measures often used in the existing literature that do not allow to exploit the heterogeneity in the degree of political connectedness of banks.

2.3.4 Political connections and the probability of state aid

Results from previous estimates documents how a bank's long-term rating increases with its degree of political connectedness, even when controlling for size, financial strength and other observables of the financial institution. It seems that credit rating agencies and investors expect politically connected banks to be therefore more likely to receive support from the domestic governments, if needed. However, this *ex-ante* expectation about state aid might not necessarily translate into an effectively larger probability of receiving public support when the bank encounters difficulties. To test whether investors' expectations are correct, I estimate the effect of political connections on the realized probability for banks to receive such support over the sample period, that is, from 2007 to 2012.

To this end, I estimate equation (7), using as dependant variable $state\_aid_{it}$, a dummy that equals 1 if bank i received state aid in year t and 0 otherwise. Table 7 reports the estimated coefficient of $conn$ on $state\_aid$ under 5 different specifications. In column 1, I report the estimated coefficient for $conn$ on $state\_aid$ with no fixed effects and no controls. The estimates including country, year and country-year fixed effects are in columns 2 and 3. In 4, also the usual bank-specific controls is included - except for $tier$, since this is missing for several banks - together with country-time fixed effects. Finally, in column 5, $tier$ is included, and (7) is estimated on a smaller sample of banks. All coefficients are estimated by GLS, with standard errors

Table 7: Results from a panel regression estimating equation (7).

VARIABLES	(1) *state_aid*	(2) *state_aid*	(3) *state_aid*	(4) *state_aid*	(5) *state_aid*
totass				0.0177***	0.0246***
				(0.00451)	(0.00845)
conn	0.174**	0.252***	0.252**	0.210***	0.370**
	(0.0864)	(0.0953)	(0.1007)	(0.0816)	(0.166)
Constant	0.0565***	-0.0313	-0.0154	-0.317*	-0.468*
	(0.0128)	(0.0394)	(0.008)	(0.168)	(0.264)
Observations	1,176	1,176	1,176	1,068	746
R^2 (overall)	0.008	0.181	0.335	0.388	0.432
Number of banks	196	196	196	193	145
Controls	NO	NO	NO	YES	YES
Country FE	NO	YES	NO	NO	NO
Year FE	NO	YES	NO	NO	NO
Country-Year FE	NO	NO	YES	YES	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, netinc, and tier in (5).

clustered at the bank level.[6]

The coefficient of *conn* is positive and significant at the 95% confidence level in all of the specifications, thus showing that political connections actually increase the likelihood of state aid for European banks in the 2007-2012 period. According to the estimates obtained under the most conservative specification, a 10% increase in the share of connected directors increases the likelihood of receiving state aid by 3.7%. Not surprisingly, the coefficient of *totass* is also positive and statistically different from zero, thus confirming that size increase the likelihood of receiving state aid, as the TBTF doctrine claims.

This last piece of evidence completes the picture on the effects of political connections on implicit guarantees for bank debt. Not only political connectedness increases

[6] The coefficients obtained with a logistic model are provided in Table 11 of the Appendix.

investors and rating agencies' expectations of public support - as documented by the positive effect of political connections on rating uplifts - but this expectation appears to be correct, being political connectedness positively associated with the realized probability of state aid.

2.4 Conclusions

A relevant literature already documented the existence of implicit guarantees for bank debt for large banks. However, little or no attention has been put so far on other possible determinants of implicit guarantees, including political connections. The aim of this paper is exactly to analyze the link between banks' political connectedness and the implicit guarantees embedded in credit rating uplifts to test whether - on top of being *too-big-to-fail* - some European banks are also *too-connected-to-fail*.

By using a continuous measure of political connections, given by the share of a bank's board members that were previously employed in the domestic government, I first estimate the effect of political connections on credit rating uplifts, and provide evidence that larger shares of connected board members are associated with larger average uplifts. This result holds when controlling for a number of bank-specific observables - including other characteristics of bank directors - and when including country, year and country-year fixed effects. I find that, on average, a 10% increase in the share of connected directors determines an increase in rating uplifts by around 0.3 notches, when estimated under the most conservative specification.

Estimating a slightly different underlying equation, where the dependant variable is the long-term rating and the stand-alone rating is included as a control - thus capturing the intrinsic ability of the bank to honor its liabilities - leads to a similar result. In this case the coefficient of my measure of political connectedness ranges from 2 to 3, depending on the specification I use, thus confirming that a 10% increase in the share of a bank's board members previously employed by government increases long-term ratings by 0.2 to 0.3 notches, on average. I also show that this effect is heterogeneous across banks, since it is inversely related to banks' financial strength.

Then I also exploit the 2010-2011 sovereign debt crisis as an exogenous shock that affects some of the banks in my sample and estimate the differential effect of political connections on implicit guarantees in the event of a crisis. According to the *too-connected-to-fail* hypothesis, when governments experience a crisis of their sovereign debt their ability to provide implicit guarantees for domestic banks lower. Empirical evidence confirms this hypothesis, being the coefficient of political connections lower in those countries most exposed to the crisis in 2010 and 2011. Much lower, when not zero, is the reduction of implicit guarantees provided by bank size in the event of a crisis. A possible explanation for this result is that large banks benefit from implicit guarantees provided by supranational agencies, since their failure would hamper the whole banking system. Alternatively, it could be that, when financially constrained, governments can provide limited support and prefers to allocate it to TBTF banks, more than to connected banks.

The continuous nature of my measure of political connections also allows me to separately estimate the effect of political connections on implicit guarantees at the extensive and the intensive margins. I provide evidence that both play a role in increasing implicit guarantees, since not only connected banks exhibit larger uplifts than non-connected banks, but the increase in the uplift is proportional to the share of politically connected directors.

Finally, I test whether the higher expectation of public support embedded in credit rating uplifts is also reflected by an actually higher likelihood of receiving state aid over the sample period. Therefore I estimate how the realized probability of receiving state aid covariates with political connectedness and I show that the likelihood of receiving government support increases with political connections.

To conclude, political connectedness seem to be a relevant determinant - so far unexplored - of implicit guarantees for bank debt in Europe. This *too-connected-to-fail* hypothesis appears to coexist with the *too-big-too-fail* one. Future research can deepen the understanding of this link, testing whether the results presented in this paper hold also outside Europe, and whether different measures of political connections - as for instance the degree of political connectedness among banks' main shareholders - lead to the same conclusions. Also, a number of additional controls, including banks' holding of domestic sovereign debt can be added to test the robustness of the results and to limit possible endogeneity coming from banks' unobservables not included in this analysis.

References

[1] Acharya, Viral, Anginer, Deniz and Warburton A. Joseph, (2013), "The End of Market Discipline? Investor Expectations of Implicit State Guarantees", SSRN Working paper.

[2] Baker, Dean and McArthur, Travis, (2009), "The Value of the "Too Big to Fail" Big Bank Subsidy," CEPR Reports and Issue Briefs, Center for Economic and Policy Research (CEPR).

[3] Blau, Benjamin, Brough, Tyler J. and Thomas, Diana, (2013), "Corporate Lobbying, Political Connections, and the Bailout of Banks", *Journal of Banking & Finance*, 37, issue 8, p. 3007-3017.

[4] Faccio, Mara, Masulis, Ronald and McConnell, John, J., (2006), "Political Connections and Corporate Bailouts", *Journal of Finance*, 61, issue 6, p. 2597-2635.

[5] Faccio, Mara, (2006), "Politically Connected Firms", *American Economic Review*, 96, issue 1, p. 369-386.

[6] Flannery, Mark, (1998), "Using Market Information in Prudential Bank Supervision: A Review of the U.S. Empirical Evidence", *Journal of Money, Credit & Banking*, 30, issue 3, p. 273-305.

[7] Flannery, Mark and Sorescu Sorin M., (1996), "Evidence of Bank Market Discipline in Subordinated Debenture Yields: 1983-1991", *Journal of Finance* 51, issue 4, p.1347-77.

[8] Imai, Masami, (2006), "Market Discipline and Deposit Insurance Reform in Japan", *Journal of Banking & Finance*, 30, issue 12, p. 3433–3452

[9] Jacewitz, Stefan, and Pogach, Jonathan, (2013), "Deposit Rate Advantages at the Largest Banks", FDIC Working Paper.

[10] Kane, Edward J., (2000), "Incentives for Banking Megamergers: What Motives Might Regulators Infer from Event-Study Evidence?", *Journal of Money, Credit and Banking*, 32, issue 3, p. 671-701.

[11] Morgan, Donald and Stiroh, Kevin J., (2000), "Bond Market Discipline of Banks", Federal Reserve Bank of Chicago, Proceedings, No 687, p. 494-526.

[12] Morgan, Donald and Stiroh, Kevin J., (2005), "Too Big to Fail After all These Years", Federal Reserve Bank of New York, Staff Reports, No 220.

[13] O'Hara, Maureen and Shaw, Wayne, (1990), "Deposit Insurance and Wealth Effects: The Value of Being 'Too Big to Fail'", *Journal of Finance*, 45, issue 5, p. 1587-1600.

[14] Penas, María and Unal, Haluk, (2004), "Gains in Bank Mergers: Evidence from the Bond Markets", *Journal of Financial Economics*, 74, issue 1, p..149-179.

[15] Rime, Bertrand, (2005), "Do 'Too Big To Fail' Expectations Boost Large Banks Issuer Ratings?" Unpublished Manuscript, Swiss National Bank.

[16] Sapienza, Paola, (2004), "The Effects of Government Ownership on Bank Lending", *Journal of Financial Economics*, 72, issue 2, p. 357-384.

[17] Schich, Sebastian, Kim, Byoung-Hwan, (2013), "Developments in the Value of Implicit Guarantees for Bank Debt: the Role of Resolution Regimes and Practices", *OECD Journal: Financial Market Trends*, 2012, issue 2, p. 35-65.

[18] Schich, Sebastian and Lindh, Sofia, (2012), "Implicit Guarantees for Bank Debt: Where Do We Stand?", *OECD Journal: Financial Market Trends*, 2012, issue 1, p. 45-63.

[19] Sironi, Andrea, (2003), "Testing for Market Discipline in the European Banking Industry: Evidence from Subordinated Debt Issues", *Journal of Money, Credit and Banking* 35, issue 3, 443-472.

[20] Ueda, Kenichi, and Weder di Mauro, Beatrice, (2012), "Quantifying Structural Subsidy Values for Systemically Important Financial Institutions," *Journal of Banking & Finance*, 37, issue 10, p. 3830-3842.

2.5 Appendix

Table 8: OLS estimates of equation (1).

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*uplift*	*uplift*	*uplift*	*uplift*	*uplift*	*uplift*
totass				0.290***	-0.109	-0.137
				(0.0945)	(0.117)	(0.123)
male				-2.185	-1.722	-1.332
				(1.569)	(1.787)	(1.834)
age				-0.113***	0.00173	0.00397
				(0.0364)	(0.0394)	(0.0388)
master				-2.342**	-0.381	-0.00371
				(0.980)	(0.984)	(0.989)
tier				0.0384**	0.0142	0.0238
				(0.0175)	(0.0264)	(0.0171)
netinc				-1.19e-07*	-5.05e-08	-4.89e-08
				(7.07e-08)	(4.55e-08)	(6.09e-08)
roaa				-0.0157	-0.0936	-0.677***
				(0.123)	(0.118)	(0.191)
roae				7.73e-05	0.000938	0.00278
				(0.00254)	(0.00234)	(0.00233)
conn	3.667**	2.504*	2.453	4.422***	2.959*	2.281
	(1.533)	(1.383)	(1.521)	(1.414)	(1.709)	(1.735)
Constant	2.576***	3.280***	0.848***	5.535*	6.857*	4.611
	(0.238)	(0.452)	(0.100)	(3.242)	(3.504)	(3.414)
Observations	549	549	549	489	489	489
R-squared	0.032	0.437	0.562	0.168	0.438	0.598
Country FE	NO	YES	NO	NO	YES	NO
Year FE	NO	YES	NO	NO	YES	NO
Country-Year FE	NO	NO	YES	NO	NO	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.

Table 9: OLS estimates of equation (2).

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*lt_rat*	*lt_rat*	*lt_rat*	*lt_rat*	*lt_rat*	*lt_rat*
bs_rat	0.719***	0.623***	0.546***	0.546***	0.470***	0.414***
	(0.0607)	(0.0602)	(0.0728)	(0.0552)	(0.0650)	(0.0688)
totass				0.635***	0.453***	0.459***
				(0.0822)	(0.0998)	(0.113)
conn	2.967**	2.892***	2.936***	1.980*	2.423**	1.743
	(1.301)	(0.939)	(0.993)	(1.063)	(1.040)	(1.057)
Constant	6.412***	8.632***	7.849***	2.869	0.976	-0.609
	(0.947)	(0.853)	(1.140)	(2.724)	(3.457)	(3.482)
Observations	549	549	549	489	489	489
R-squared	0.628	0.821	0.892	0.719	0.829	0.902
Controls	YES	YES	YES	YES	YES	YES
Country FE	NO	YES	NO	NO	YES	NO
Year FE	NO	YES	NO	NO	YES	NO
Country-Year FE	NO	NO	YES	NO	NO	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae.

Table 10: OLS estimates of equation (3).

VARIABLES	(1) lt_rat	(2) lt_rat	(3) lt_rat	(4) lt_rat
bs_rat	0.512***	1.495***	0.526***	0.482
	(0.0512)	(0.342)	(0.0591)	(0.480)
$totass$	0.430***	1.143***	1.283***	1.250*
	(0.0727)	(0.285)	(0.485)	(0.666)
$conn$	9.322***	7.720**		
	(3.413)	(3.651)		
$bs\_rat \times conn$	-0.559**	-0.434*	-0.399	-0.403
	(0.235)	(0.252)	(0.250)	(0.258)
$bs\_rat \times totass$		-0.0535***		0.00236
		(0.0194)		(0.0257)
Constant	-0.923	-14.10**	-13.25	-12.62
	(1.798)	(5.555)	(9.099)	(12.57)
Observations	489	489	489	489
R-squared	0.905	0.907	0.960	0.960
Controls	YES	YES	YES	YES
Country-Year FE	YES	YES	YES	YES
Bank FE	NO	NO	YES	YES

Robust standard errors in parentheses. *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae.

Table 11: OLS estimates of equation (4).

VARIABLES	(1) *uplift*	(2) *uplift*	(3) *uplift*	(4) *uplift*	(5) *lt_rat*	(6) *lt_rat*
totass	0.285***	-0.100	-0.127	-0.115	0.465***	0.485***
	(0.0939)	(0.116)	(0.123)	(0.135)	(0.115)	(0.120)
conn	4.899***	3.410*	2.785	2.773	2.080*	2.058*
	(1.476)	(1.878)	(1.934)	(1.945)	(1.091)	(1.100)
crisis	0.556	0.587	1.023*	2.586	0.356	3.024
	(0.379)	(0.370)	(0.560)	(4.060)	(0.545)	(3.789)
crisis×*conn*	-4.740*	-3.415	-3.959*	-3.547	-2.632*	-1.927
	(2.499)	(2.167)	(2.180)	(2.444)	(1.543)	(1.707)
crisis×*totass*				-0.0847		-0.144
				(0.213)		(0.195)
bs_rat					0.415***	0.415***
					(0.0684)	(0.0685)
Constant	5.606*	6.777*	4.497	4.238	-0.671	-1.117
	(3.224)	(3.491)	(3.401)	(3.638)	(3.481)	(3.607)
Observations	489	489	489	489	489	489
R-squared	0.173	0.440	0.601	0.601	0.903	0.903
Controls	YES	YES	YES	YES	YES	YES
Country FE	NO	YES	NO	NO	NO	NO
Year FE	NO	YES	NO	NO	NO	NO
Country-Year FE	NO	NO	YES	YES	YES	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae.

Table 12: OLS estimates of equations (4) and (5)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*uplift*	*uplift*	*uplift*	*uplift*	*lt_ rat*	*lt_ rat*
totass	-0.144	-0.166	-0.278	-0.223	0.398***	0.490***
	(0.123)	(0.130)	(0.168)	(0.190)	(0.127)	(0.137)
D_{conn}	0.635**	0.478*				
	(0.255)	(0.249)				
conn			3.366*	2.642	3.146**	2.328
			(2.009)	(2.135)	(1.321)	(1.402)
Constant	6.465*	4.366	12.41***	8.648*	5.740*	3.075
	(3.430)	(3.362)	(4.293)	(4.462)	(3.365)	(2.902)
Observations	489	489	393	393	393	393
R-squared	0.433	0.596	0.412	0.575	0.835	0.916
Controls	YES	YES	YES	YES	YES	YES
Country FE	YES	NO	YES	NO	YES	NO
Year FE	YES	NO	YES	NO	YES	NO
Country-Year FE	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses (clustered at the bank level). *** p<0.01,**p<0.05, * p<0.1.

Controls include: male, age, master, tier, netinc, roaa, roae and bs_rat in (5) and (6).

Table 13: Results from a logit regression estimating equation (7)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	*state_aid*	*state_aid*	*state_aid*	*state_aid*	*state_aid*
totass				0.848^{***}	0.760^{***}
				(0.162)	(0.190)
conn	1.961^{**}	4.639^{***}	5.714^{***}	4.603^{**}	6.365^{**}
	(0.812)	(1.558)	(2.122)	(2.091)	(2.771)
Constant	-2.750^{***}	-6.268^{***}	-0.987	-16.12^{***}	-10.05^{*}
	(0.204)	(1.258)	(0.644)	(5.960)	(5.683)
Observations	1,176	774	430	360	269
Controls	NO	NO	NO	YES	YES
Country FE	NO	YES	NO	NO	NO
Year FE	NO	YES	NO	NO	NO
Country-Year FE	NO	NO	YES	YES	YES

Robust standard errors in parentheses. *** $p<0.01$,** $p<0.05$, * $p<0.1$.

Controls include: male, age, master, netinc, and tier in (5).

Chapter 3

3 The Production of Knowledge after Tenure: Evidence from the Italian National Scientific Habilitation

Abstract

In this paper, we investigate how the introduction of a centralized evaluation process for hiring associate and full professors in the Italian university system - the National Scientific Habilitation (NSH) - affected the subsequent research production of successful and unsuccessful candidates. The reform - introduced in 2010 to mitigate the extent of patronage in local competitions - restricted the access to university positions to candidates complying with minimum requirements, defined at the national level, in terms of quality and quantity of research production. The main criteria used to define the sector-specific thresholds were: number of articles, number of citations and h-index, computed in the ten years prior to the NSH. To get the habilitation, candidates have to score above the median professor in their specific field. Therefore, we exploit the discontinuity in the probability of obtaining the habilitation induced by the minimum threshold and estimate a novel triple fuzzy regression discontinuity design to estimate the effect of (getting closer to) tenure on the quantity and the quality of scholars' scientific production in the years following the habilitation. We find that candidates that qualify as full professors publish on average fewer articles than rejected candidates but on higher-quality journals, even though this *qualitative* effect is small and barely significant. No significant effect emerges in the sample of candidates for associate professorship.

3.1 Introduction

There is an open debate on the mechanisms and the incentives leading to the production of knowledge, which is a crucial research question in the education and economic literature. Still, little is known about how scholars react, in terms of research production to different employment contracts and under different hiring and promotion mechanisms, even when these mechanisms are targeted to encourage and reward 'productive' researchers, as for the case of tenure tracks. Do academics' productivity fall after they earn tenure, or get closer to it? Or is it the case that the research habits developed during the tenure track persist after the goal is achieved?

We tackle this research question exploiting the introduction of a centralized evaluation process for hiring associate and full professors in the Italian university system - the National Scientific Habilitation (NSH) - that became a necessary step for earning tenure in Italy. Candidates are evaluated on the basis of their academic *cv*, and depending on their previous research production, the latter being measured by a set of bibliometric indicators. Therefore, we exploit the discontinuity in the probability of obtaining the NSH induced by overcoming or not specific thresholds, defined at the academic field level, to compare the subsequent research patterns of barely accepted and rejected candidates.

There is a small literature focusing on the *post*-tenure academic productivity of scholars, both from a theoretical and empirical perspective. Most studies suggest that research productivity falls after tenure: Holley (1997) shows that lifetime employment contracts lower the amount of published research among sociologists. However, it is hard to disentangle a sharp fall in productivity due to the achievement of tenure from a decline associated with age of scholars, documented, among others, by Oster and Hamermesh (1998) and Levin and Stephan (1992). In contrast with the *lost of motivation* arguments, other authors emphasize that this negative effect might be offset by the development of persistent academic habits during the tenure process, which are not likely to vanish after tenure is achieved.[7]

[7] Faria and Monteiro (2008) provide a theoretical framework for the development of those habits.

However, earning tenure might represent a broader achievement than just moving to a lifetime contract: scholars, for instance, might also perceive it as a reward for their research career. According to this perspective, our work is also related to the ones of Borjas and Doran (2014) who investigate the productivity and research choices of mathematicians before and after winning a prestigious award, the Field Medal. Interestingly, their result suggest that the award of the prize is associated with a decline in productivity, but also with a higher degree of cognitive mobility, defined as the probability of switching to a new research field, different from the *pre*-prize baseline ones.[8]

Finally, we are not the first who studied the functioning of a national habilitation system as a recruitment and promotion procedure for academics in public universities. However, most authors focus on the importance of candidates' characteristics not related to their research production in fostering the probability of achieving the habilitation. Zinovyeva and Bagues (2015) analyze the centralized selection exams in Spanish academia, providing strong evidences in support of the hypothesis that candidates with direct connections with committee members are more likely to obtain the habilitation. According to the authors, it is unlikely that the impressive connection premium (50%) might be the consequence of information asymmetries - connected committee members might have private information about other dimensions where the candidate excels - while it should be seen as an evidence of a preferential treatment. Similarly, for the case of Italy, Abramo and D'Angelo (2015) provide descriptive evidence that candidates already 'on staff' are more likely to succeed than those external to academia.

Our findings document that, on average, candidates who passed the habilitation for full professors publish less than rejected candidates in the three years following the achievement of the habilitation. Even though we can not precisely distinguish whether this result comes from a decrease in productivity for habilitated candidates,

[8] Similar to this paper are the works of Bricogne (2014) and Chan et Al (2013), analysing the *post*-prize productivity of economists. In both cases, empirical results show that the award of a prize increases subsequent scholars' productivity.

or from an increase in productivity of rejected candidates, looking at the overall trend in the average number of publications seems to confirm the second hypothesis.

Our contribution is twofold; first, we contribute to the literature analyzing the link between productivity-based promotion schemes and scholars' research outcomes; then we propose a novel empirical approach to estimate a fuzzy regression discontinuity design with multiple cutoffs and three running variable. This *triple-discontinuity* design can find application in several empirical settings, where the discontinuity in the probability of receiving the treatment has multiple determinants.

Furthermore, apart from representing an ideal natural experiment for addressing our general research question, we think that an analysis of the functioning and consequence of the NSH might be interesting *per se*. Indeed, similar hiring processes have been established in many countries (Spain and Germany, among others) and yet little is known on the welfare effects associated with the system of incentives implied by such reforms. Therefore, our contribution can be seen also as a first attempt to shade a light on how the production of knowledge of scholars reacts to centralized evaluation systems based on 'bibliometric' criteria.

The rest of the paper is organized as follows: section 2 describes the regulatory framework, summarizing the key aspects that characterize the National Scientific Habilitation; section 3 explains the identification strategy adopted for our analysis; a description of data and some summary statistics are provided in section 4; then, the results from the first stage estimations - where we estimate the discontinuity in the probability of passing the habilitation for candidates scoring above the thresholds - are provided in section 5 while in section 6 we present the main results, that is, the effect of the habilitation on candidates' scientific production; finally, section 7 concludes.

3.2 The National Scientific Habilitation

The Law 240 of 2010 deeply reformed the Italian university system. One of the aspects mostly affected by the reform was the recruiting mechanism, with the introduction of the National Scientific Habilitation (NSH) which effectively came into force with the Presidential decree 222 of 2011. Prior to the NSH, the recruitment process for Italian public universities was decentralized and each university could hire or promote a faculty member autonomously. To mitigate the effects of local patronage and favoritisms, the Law 240 introduced a new centralized system that restricts the access to tenured university positions to those candidates complying with minimum requirements established at the national level.

According to the reform, the selection of tenured academics now consists of two steps. Firstly, academics have to apply and pass the NSH. Second, local universities can hire as associate or full professors only those candidates who achieved the habilitation. The rationale for the habilitation is therefore to limit favoritisms at the local level, restricting the access to local competitions only to candidates that were *pre*-screened by national sector-specific committees, that evaluate all candidates in a subject area depending on the quality and the quantity of their research production.

The reform identifies 184 competition sectors, depending on the subject area, grouped into 14 disciplinary macro-areas. A judging committee composed by full professors is therefore appointed by the ANVUR - the National Agency for the Evaluation of University and Research systems, also introduced by Law 240/2010 - for each competition sector and candidates can apply to one or more sectors, to get the habilitation either as associate or as full professors. To do so, they have to submit their application accompanied by a curriculum vitae and a list of publications. The committee then evaluates candidates on the basis of the scientific production of each candidate - evaluated through bibliometric indicators computed by the ANVUR - and taking into considerations a number of additional criteria including grants, teaching experience, and others.

As regards the judgement on the scientific production of candidates, the reform establishes that this has to be made on the basis of three individual bibliometric indicators computed directly by the ANVUR. In particular, the reform establishes that, in order to receive a positive assessment on her scientific production, a candidate needs at least one or two - depending on whether the sector is considered as *bibliometric* or not - of the three indicators be above a given sector-specific threshold. In principle, complying with this bibliometric rule should be the main criterion when evaluating a candidate's scientific production, but commissioners can decide whether to rely more or less strictly on this parameter. In the end, the weight that has to be given to the indicators alone in order to decide whether to habilitate a candidate or not is at the discretion of the committee and therefore varies across sectors. In any case, the weights and the additional criteria adopted by the committee have to be *pre*-determined and indicated in a formal statement that is then published on the website of the Italian Ministry of Education.

Possible favoritism in the habilitation process is extremely limited by the fact that commissioners are randomly drawn from a set of eligible candidates. Full professors can indeed apply to be appointed as commissioners in their specific sectors and all those applicants whose bibliometric indicators are above the median full professor can be part of the committee. Then, among the eligible professors, four commissioners are randomly drawn and a fifth member is then randomly selected from a list of eligible professors working in a OECD country. Finally, commissioners remain in office for a two-years period, at the end of which a new committee is appointed.

As regards the indicators the ANVUR calculates for each candidate, these vary depending on whether the sector is considered as *bibliometric* or *non-bibliometric*.

For the 109 *bibliometric* sectors, the three indicators that summarize candidates' scientific production are:

- i) number of articles published on journals covered by the main international databases of peer-reviewed literature (Web of Science, Scopus) in the 10 years preceding the application (normalized for academic age);

- ii) number of citations received by the candidate for its overall scientific production (normalized for academic age);
- iii) H-index, normalized for academic age.

In these sectors, a candidate to the habilitation as associate professor should receive a positive assessment for her scientific production if she scores above the median associate professor in her competition sector in at least two of the three indicators. Similarly, a candidate to the habilitation as full professor needs two indicators to be above the indicators of the median full professor in her competition sector to receive a positive assessment.[9]

For the remaining 75 *non-bibliometric* competition sectors, the three indicators are the following:

- i) number of monographs with ISBN code published in the 10 years preceding the application (normalized for academic age);
- ii) number of chapter in books (with ISBN) and articles in journals published in the 10 years preceding the application (normalized for academic age);
- iii) number of articles published in A-ranked journals, the latter being established by the ANVUR.

In such *non-bibliometric* sectors, candidates as associate (full) professors, need to score above the median associate (full) professor in their competition sector in one indicator to receive a positive assessment on the importance and the impact of their scientific production.

[9] The indicators for the median associate and full professor are computed after calculating each indicator on the whole population of associate and full professors, respectively, in each competition sector and then computing the corresponding medians

Complying with the afore-mentioned bibliometric rule, based on individual indicators, should constitute the main criterion commissioners take into considerations when making their decision on candidates. However, the committee can decide whether to rely more or less strictly on it but any judgment made on basis others than the indicators need to be formally motivated by the committee in their final assessment. Habilitated candidates have then eight years to get a placement in a public university. If this is not the case, the habilitation exhausts and the candidate needs to apply to the NSH again. Finally, in all sectors, failure to obtain the habilitation precludes the participation to the habilitation in the same competition sector for two years.

The first round of the NSH took place in 2012, when 40.228 candidates presented 59.148 applications. The time-line for the first round of the NSH was the following:

- On the 27^{th} of June, the first call for commissioners was published on the website of the Ministry of Education. The deadline for the application was set on the 28^{th} of August.
- One month later, on the 20^{th} of July 2012, the first call for candidates to the NSH was also published. The deadline was set for the 20^{th} of November.
- After the closure of the applications, the ANVUR published candidates' indicators and the selected commissioners were made public. After a few weeks, the committees made public the criteria for their work and started evaluating candidates.
- Most committees completed the evaluation procedure by June 2013, and all of them by December of the same year, when results were made public.

3.3 Identification

In order to receive a positive assessment on their scientific production in the NSH process, academics need to score above the median (associate or full) professor in their competition sector, in at least two of the three indicators calculated by the ANVUR and discussed in the previous section. For instance, a candidate in a *bibliometric* sector scoring below the median in the first indicator and above the median in the third one, would need indicator two to be strictly above the median in order to get a positive assessment, that in turn increases his chances to successfully pass the habilitation. Therefore, medians constitute sector-specific thresholds above which candidates' probability of getting habilitated increases discontinuously.[10]

Thus, the NSH offers the perfect framework to estimate a (fuzzy) RD model to determine the effect of passing the habilitation - and getting closer to a tenured position - on a number of research outcomes in the years following the first NSH. In particular, we can explore both the quantity (number of published research items) and quality (share of articles published in journals ranked in the first quartile of the SCIMAGO Journal Ranking) research pattern of candidates prior and after the NSH. From an econometric point of view we face a framework with multiple (sector-specific) cutoffs and three different running variables, that we can either combine in a triple-rd model or consider separately. Since committees have some discretion on how strictly enforce the bibliometric rule, scoring above the medians is neither a sufficient nor a strictly necessary condition to achieve the habilitation, the latter being the treatment variable in our scenario. It follows that the probability of receiving the treatment does not jump from 0 to 1 when candidates score above the thresholds. Still, overcoming the thresholds set by the Ministry discontinuously increases the probability to pass the habilitation. Thus, we can estimate a fuzzy RD where the probability of receiving the treatment is a function of candidate i's distance from the median in each of the three individual indicators, that we define as x_{1i}, x_{2i} and x_{3i}.

In particular, let D_{ki} be an indicator function that equals 1 when indicator k of candidate i is above the cutoff, that is:

$$D_{ki} = \begin{cases} 0 \text{ if } x_{ki} \leq c_k \\ 1 \text{ if } x_{ki} > c_k \end{cases} \text{for each } k \in \{1, 2, 3\}.$$

[10]In non-*bibliometric* sector, the condition for obtaining the habilitation was to score above the median in one out of three indicators

Then, in *bibliometric* sectors candidates need at least two indicators to be above the corresponding sector-specific cutoff, whilst in *non-bibliometric* sectors only one indicator above the threshold is sufficient to satisfy the indicators-based rule. Therefore, we can define an indicator D_i that is equal to 1 when candidates comply with the afore-mentioned rule. Formally:

$$D_i = \begin{cases} 0 \text{ if } \sum_{k=1}^{3} D_{ki} < 2 \\ 1 \text{ if } \sum_{k=1}^{3} D_{ki} \geq 2 \end{cases}$$

if the competition sector for candidate i is *bibliometric*, and

$$D_i = \begin{cases} 0 \text{ if } \sum_{k=1}^{3} D_{ki} < 1 \\ 1 \text{ if } \sum_{k=1}^{3} D_{ki} \geq 1 \end{cases}$$

if the sector is *non-bibliometric.*

Let $\Pr(T_i = 1|X_i)$ be the probability of passing the habilitation for candidate i, conditional on his indicators, our first-stage equation is:

$$\Pr(T_i = 1\,|X_i) = \alpha + f(X_i) + \rho D_i + \gamma Z_i + \epsilon_i, \tag{1}$$

where: $f(X_i)$ is a generic nth order polynomial in the three running variables, $\hat{x}_1$, $\hat{x}_2$ and $\hat{x}_3$, that equal candidate i's distance from the median in each of the three indicators; D_i is the indicator variable that equals 1 when candidates score above the median in at least 2 (1) indicators in a *bibliometric* (*non-bilbliometric*) competition sector; and Z_i a set of controls that include sector fixed effects. Finally ϵ_i contains candidate-specific unobservables.

In equation (1), the coefficient ρ measures the jump in the probability of getting the habilitation that comes from complying with the bibliometric rule established by the ANVUR. Estimating ρ on different subsamples allows us to measure how strictly the bibliometric criterion was enforced in different sectors.

Then, we estimate the effects of getting the habilitation on a set of research outcomes, that we define as Y_i. Our second stage regression is therefore:

$$Y_i = \mu + f(X_i) + \beta\widehat{T_i} + \theta Z_i + \eta_i, \tag{2}$$

where $\widehat{T_i}$ is estimated through (1) and therefore instrumented by D_i. The estimated 2SLS coefficient β, in (2), captures the causal effect of the habilitation on Y_i and therefore corresponds to the Local Average Treatment Effect (LATE). Then, we also estimate the following reduced-form equation

$$Y_i = \mu + f(X_i) + \beta D_i + \theta Z_i + \eta_i, \tag{3}$$

where the coefficient of D_i is the Intention-To-Treat (ITT) effect.

Three main concerns might prejudice the validity of our estimates, since would affect the validity of the RD setting itself: (i) the presence of manipulation, (ii) possible selection in the sample and (iii) discontinuities in covariates.

The risks associated with an either selected or manipulated sample are particularly severe in a regression discontinuity framework, where the main identifying assumption relies on a local quasi-random assignment of the treatment around the eligibility cutoff. Thus, we have to rule out the possibility that candidates' indicators - our running variables - were manipulated in order to increase the probability of habilitation of individuals *just below* (or above) the cutoff. At the same time, given that our initial sample is not the population of eligible scholars, but rather the sample of those who decided to apply, we need to prove that we are not comparing, on the two sides of the cutoff, two different and selected sample of candidates.[11]

We tackle both issues in two different ways. Firstly, we look at the continuity in the *density* on the two sides of the cutoff. Indeed, in presence of manipulation or

[11] With respect to the manipulation concern, it seems anyway unrealistic since indicators were calculated by the ANVUR, and not by the different committees.

selection, we should observe a discontinuity in the number of observations around the different thresholds. By looking at figures 1 to 3, where we report the distribution of applicants to the habilitation as associate and full professors for the three different indicators, this seems not to be the case, as no evident jumps occurs in the neighborhood of the cutoffs. Then, we also look at the continuity in *pre-treatment* covariates around the cutoffs. More precisely, we estimate equation (2) using as dependant variables a set of research outcomes, analogous to the one discussed above, but prior to the introduction of the NSH. We find covariates to be continuous around the thresholds, as a further confirmation of the validity of our settings. Results from these estimates are included in the section devoted to the results from the second stage.

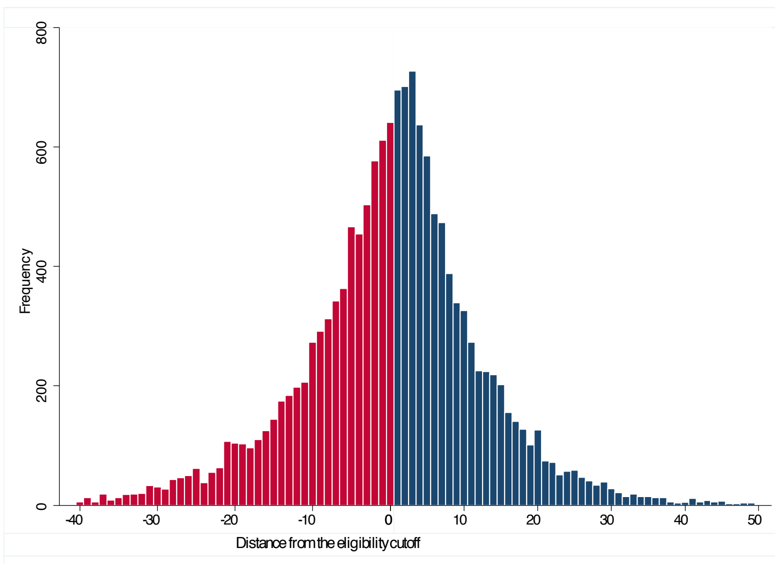

Figure 1: Distance from cutoff 1

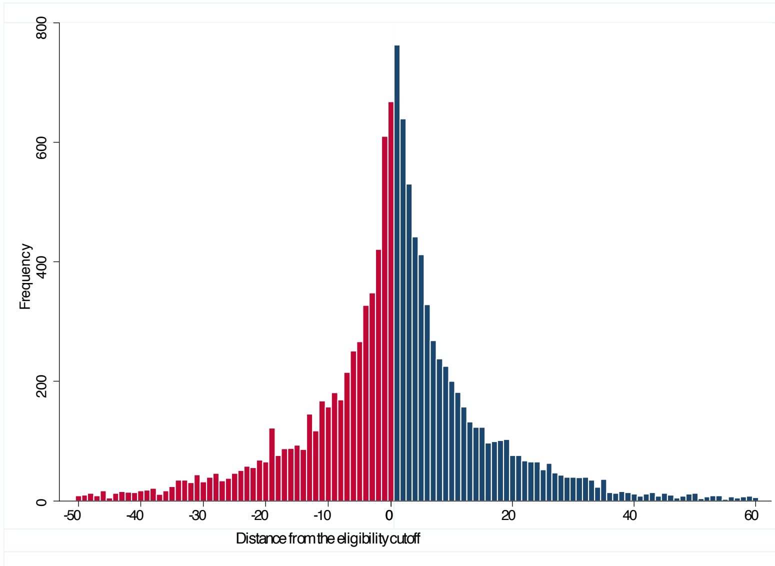

Figure 2: Distance from cutoff 2

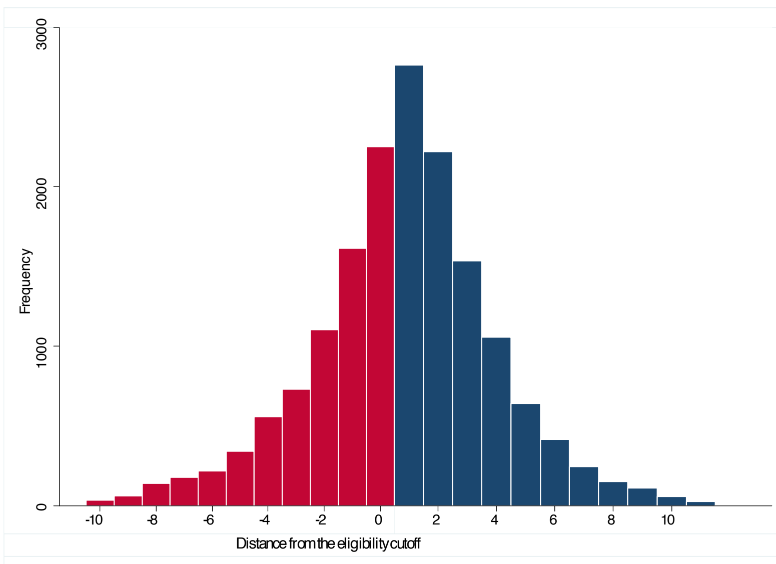

Figure 3: Distance from cutoff 3

Finally, and most importantly, our strategy relies on the validity of our first-stage, since the effect of the treatment on research outcomes can only be estimated if getting a positive assessment on scientific production effectively determines a jump in the probability of getting the tenure. We find this is the case in most sectors, and we devote one section to present the results from our first stage.

3.4 Data

For each call of the NSH, the National Agency for the Evaluation of the University and Research System (ANVUR) publishes the list of the candidates, including names, academic field of application and the score in each of the three relevant indicators. Also, the sector specific medians, which represent the relevant thresholds for obtaining a positive assessment on the scientific production, are published before each call is opened.

These administrative records represent the main dataset for our analysis. Starting from the list of candidates we retrieved the whole list of publications, by performing for each professor a separate query in the SCOPUS website, the largest database of peer-reviewed literature. Accessing the SCOPUS records of each author who participated to the 2012 NSH call allowed us to acquire a rich set of detailed and constantly updated information about her scientific production. Since the SCOPUS database is structured at the publication level, it was possible to identify not only the aggregate amount of the scientific production, but also the specific features of each article, conference paper, book chapter and review published during the research career. Therefore, we were able to construct a *panel* dataset at the author/year level for a wide set of indicators including the number of research items in each year, their quality - proxied by the quality of the journal where they were published - and their specific research field.

With respect to the quality dimension, our indicators follow the SCIMAGO Journal Ranking (*SJR*) 2015. More precisely, for each publication in our dataset we searched the SCIMAGO dataset to determine the quality of the journal where it was published, obtaining both a continuous quality measure - the *SJR* index - and a discrete indicator for the research field specific *SJR* quartile.[12]

The list of the candidates who participated to the NSH in 2012 includes 39581 professors. Since the system allowed for multiple applications per candidate - to apply in different academic fields and rank - we had to select, among the 59 thousands applications, only the one more 'relevant' for each candidate. Dropping those individuals for which it was not possible to select only one academic field led us to a final sample of 32800 candidates.[13]

Furthermore, to estimate our second stage equations, we also drop all candidates in non-bibliometric sectors, ending up with a list of 19415 candidates. The reason why we exclude non-bibliometric fields is directly related to their particular nature. Those fields - including Law, Humanities, Philosophy and Sociology - are characterized by a research production hardly evaluable by pure bibliometric indicators. As a consequence of this - and as it can be seen in the section where we present our first stage results - the committees in most cases assigned little weight to the bibliometric rule and considered a larger and more heterogenous set of criteria other than the indicators.

The wide coverage of the scientific production of SCOPUS allowed us to retrieve the list of publications for more than 90% of the professors in our restricted sample of candidates, leading to a final sample size of 17703 applicants. Table 1 provides some descriptive statistics about our final sample.

Finally, we provide in the Appendix - figures 7 to 12 - a graphical representation of the heterogeneity in the thresholds across the different competition sectors. On

[12] We also performed the same merge with the SCOPUS journal list, that includes different proxies for the quality of the journal, including the *IPP* and *SNIP* indices

[13] The criteria chosen for selecting a specific field for each candidate was the performance with respect to the field median. Thus, for those candidates whose indicators were *below* the requirements for a positive assessment we kept the academic field where the distance from the threshold was the smallest. Coherently, for those candidates whose indicators were above the requirements, we defined as the relevant academic field the one guaranteeing the largest distance from the threshold.

Table 1: Descriptive statistics

	Candidates for associate professor				Candidates for full professor			
	mean	sd	max	min	mean	sd	max	min
female	0.371	0.483	1.0	0.0	0.257	0.437	1.0	0.0
academic age	12.897	7.995	78.0	-5.0	17.962	9.855	129.0	-5.0
indicator 1	30.574	42.921	726.4	0.0	46.449	55.625	698.2	0.0
indicator 2	44.257	93.284	1464.2	0.0	72.401	115.750	1498.4	0.0
indicator 3	9.056	6.889	59.0	0.0	12.481	8.383	71.0	0.0
cutoff 1	20.548	11.325	59.5	2.5	31.091	19.968	89.0	1.5
cutoff 2	21.108	19.597	104.1	0.3	34.119	30.981	155.1	0.1
cutoff 3	7.325	3.333	18.0	1.0	9.462	4.761	22.0	1.0
dist from cutoff 1	10.025	37.922	666.9	-56.5	15.358	47.877	620.2	-83.0
dist from cutoff 2	23.149	82.934	1360.2	-102.3	38.282	101.810	1410.7	-152.3
dist from cutoff 3	1.731	5.321	45.0	-15.0	3.019	5.990	55.0	-17.0
articles 2009-2011	7.277	9.214	208.0	0.0	10.226	11.269	156.0	0.0
articles 2014-2016	11.269	21.476	279.0	0.0	16.167	28.660	577.0	0.0
Observations	13558				5857			

average, the cutoffs are higher for candidates applying for full professors than for candidates applying for associate professors. Moreover, competition sectors belonging to physics and health sciences, are the ones characterized by the highest thresholds.

3.5 First Stage Results

Our identification strategy relies on one key assumption: satisfying the indicators-based rule produces a jump in candidates' probability of getting habilitated. To test whether this identifying assumption is satisfied in our dataset, we estimate equation (1) discussed in the section 3. To do so, we firstly estimate (1) on the sub-samples of *bibliometric* and *non-bibliometric* sectors, separately. The results from these estimates are provided in Table 2, where the discontinuity in the probability of passing the habilitation when scoring above the medians is estimated under different specifications. In particular, in column 1 we estimate a quadratic polynomial on the whole sample of candidates. In column 2 we also include academic field fixed effects that

Table 2: First stage estimates

Panel A: Candidates in bibliometric sectors			
	Dependant variable: Prob(Habilitated)		
	(1)	(2)	(3)
	Quadratic	Quadratic	LLR-MSE
Scores above cutoffs	0.342***	0.350***	0.219***
	(0.015)	(0.013)	(0.046)
Sector FE	No	Yes	Yes
Mean Y_i	0.541	0.541	0.428
Observations	17703	17703	2941
BW_1			5.511
BW_2			5.594
BW_3			2.040
Panel B: Candidates in non-bibliometric sectors			
	Dependant variable: Prob(Habilitated)		
	(1)	(2)	(3)
	Quadratic	Quadratic	LLR-MSE
Scores above cutoffs	0.110	0.097	0.135
	(0.100)	(0.084)	(0.091)
Sector FE	No	Yes	Yes
Mean Y_i	0.563	0.563	0.446
Observations	7392	7392	2001
BW_1			5.511
BW_2			5.594
BW_3			2.040

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In (1) and (2) a quadratic specification is estimated on the full sample; in (3) a local linear regression within the optimal bandwidths is estimated. Optimal MSE-bandwidths in the LLR are computed following Imbens and Kalyanaraman (2009). Score above cutoffs is a dummy that equals 1 when a candidate scores above the relevant median in at least 1 (2) out of 3 indicators in a *non-bibliometric* (*bibliometric*) sector. Y_i is a dummy that equals 1 if candidate i passes the habilitation and zero otherwise.

account for heterogeneity in the medians across sectors. In column 3 we report the estimates from a local linear regression around the thresholds.

We find that the coefficient of D_i is highly significant and ranges between 22% and 30% in *bibliometric* sectors, whilst it is much lower (around 10%) and not statistically different from 0 in *non-bibliometric* sectors. As mentioned in the previous section, a possible explanation for this relies on the different nature of *bibliometric* and *non-bibliometric* sectors. Since in the latter, by definition, bibliometric indicators are less

Table 3: First stage estimates, bibliometric sectors only

Panel A: Candidates for full professor			
	Dependant variable: Prob(Habilitated)		
	(1)	(2)	(3)
	Quadratic	Quadratic	LLR-MSE
Scores above cutoffs	0.453***	0.448***	0.280***
	(0.029)	(0.026)	(0.084)
Sector FE	No	Yes	Yes
Mean Y_i	0.628	0.628	0.487
Observations	5383	5383	924
BW_1			7.689
BW_2			7.652
BW_3			3.025

Panel B: Candidates for associate professor			
	Dependant variable: Prob(Habilitated)		
	(1)	(2)	(3)
	Quadratic	Quadratic	LLR-MSE
Scores above cutoffs	0.310***	0.319***	0.255***
	(0.017)	(0.015)	(0.043)
Sector FE	No	Yes	Yes
Mean Y_i	0.503	0.503	0.417
Observations	12320	12320	2847
BW_1			5.702
BW_2			7.211
BW_3			4.030

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In (1) and (2) a quadratic specification is estimated on the full sample; in (3) a local linear regression within the optimal bandwidths is estimated. Optimal MSE-bandwidths in the LLR are computed following Imbens and Kalyanaraman (2009). Score above cutoffs is a dummy that equals 1 when a candidate scores above the relevant median in at least 2 out of 3 indicators. Y_i is a dummy that equals 1 if candidate i passes the habilitation and zero otherwise.

appropriate to capture the impact and the quality of candidates' scientific production, committees did not enforce the indicators-based rule strictly, thus lowering the impact this had in the habilitation procedure.

For this reason, from now on, we focus only on *bibliometric* sectors. Table 3 presents the results from the estimation of the first stage on the two subsamples of candidates applying for being habilitated as associate and full professors, separately. In both cases the coefficients are highly significant under all the different specifications. For applicants as full professors, coefficients ranges between 28% to 45%, depending on the functional form we assume, whilst they are slightly lower and around 25% to 30% for applicants as associate professors.

Then, we also check the robustness of our findings by looking at indicators 1, 2 and 3, separately, so as to provide also a graphical representation of the results. In this case, we estimate the following equation:

$$\Pr(T_i = 1 \,| X_i) = \alpha + f(X_i) + \rho_k D_{ki} + \gamma Z_i + \epsilon_i, \tag{4}$$

for $k = 1, 2, 3$.

Figures 4 to 6 provide an illustration of the discontinuity around the cutoffs for indicator 1, 2 and 3, respectively, and confirm our findings, while OLS estimates of equation (4) are presented in Table 4. Estimates confirm that scoring above the median in each of the three indicators leads to a jump in the probability of getting habilitated, regardless of the other indicators. It is not surprising that the magnitude of the jump is lower than the one we estimate under equation (1), since, by looking at each indicator separately, we are not taking into account candidates' performance in the other indicators. Candidates might get a positive assessment also if below the median, if the other two indicators are above the cutoffs, as well as being above the median does not imply to comply with the bibliometric rule. For this reason, our preferred specification is the 'triple' regression discontinuity equation defined by equation (1).

Table 4: First stage estimates, by indicator

Panel A: Candidates for full professor			
	Dependant variable: Prob(Habilitated)		
	(1)	(2)	(3)
	Quadratic	Quadratic	LLR-MSE
Score above cutoff 1	0.165***	0.180***	0.156***
	(0.020)	(0.019)	(0.037)
Score above cutoff 2	0.284***	0.303***	0.287***
	(0.025)	(0.024)	(0.031)
Score above cutoff 3	0.236***	0.248***	0.242***
	(0.028)	(0.027)	(0.048)
Sector FE	No	Yes	Yes
Mean Y_i	0.628	0.628	0.572
Observations	5383	5383	1949
BW_1			7.689
BW_2			7.652
BW_3			3.025

Panel B: Candidates for associate professor			
	Dependant variable: Prob(Habilitated)		
	(1)	(2)	(3)
	Quadratic	Quadratic	LLR-MSE
Score above cutoff 1	0.164***	0.153***	0.113***
	(0.013)	(0.012)	(0.023)
Score above cutoff 2	0.179***	0.191***	0.149***
	(0.015)	(0.014)	(0.018)
Score above cutoff 3	0.167***	0.189***	0.196***
	(0.018)	(0.016)	(0.024)
Sector FE	No	Yes	Yes
Mean Y_i	0.503	0.503	0.459
Observations	12320	12320	4713
BW_1			5.702
BW_2			7.211
BW_3			4.030

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In (1) and (2) a quadratic specification is estimated on the full sample; in (3) a local linear regression within the optimal bandwidths is estimated. Optimal MSE-bandwidths in the LLR are computed following Imbens and Kalyanaraman (2009). Score above cutoff k is a dummy that equals 1 when indicator k is above the relevant median and zero otherwise. Y_i is a dummy that equals 1 if candidate i passes the habilitation and zero otherwise.

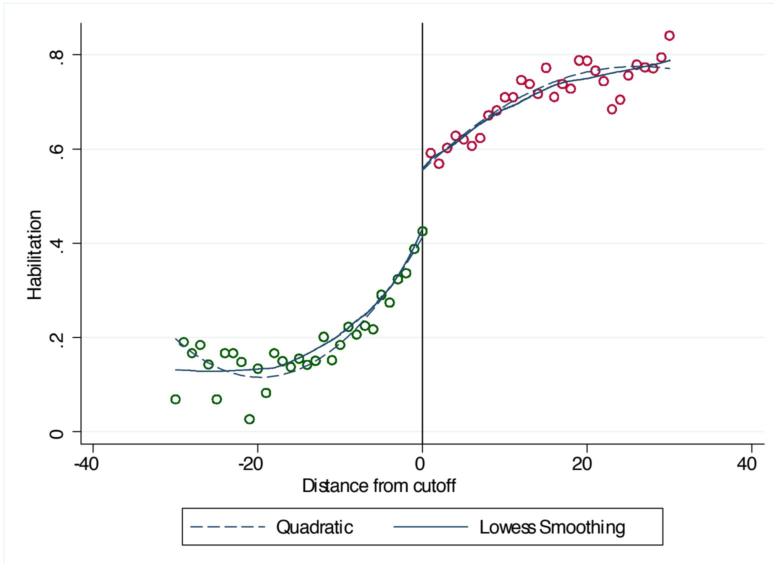

Figure 4: Probability of passing the habilitation around cutoff 1.

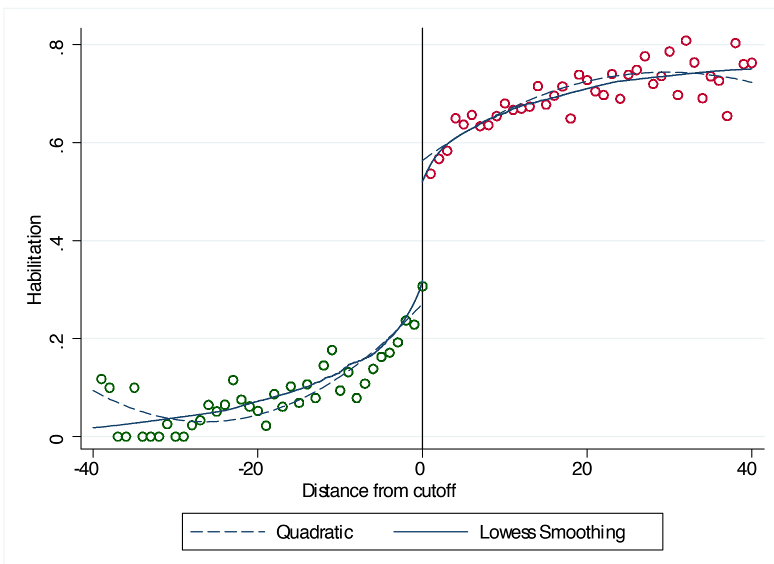

Figure 5: Probability of passing the habilitation around cutoff 2.

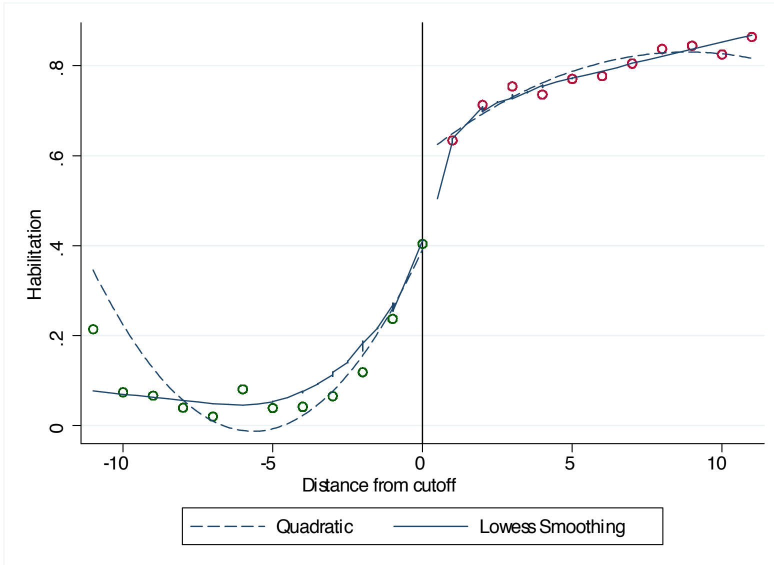

Figure 6: Probability of passing the habilitation around cutoff 3.

3.6 Main Results

In this section we focus on bibliometric sectors and we investigate the effect of the habilitation on two main research outcomes: (i) the number of articles published in the three years following the first NSH, and (ii) the ratio of articles published in high-quality journals on the total number of published articles.[14] Estimates of the former capture the quantitative effect of getting (closer to) a tenured position, whilst the latter capture the qualitative effect. We show both the Intention-To-Treat and Local Average Treatment effects, under different specifications. In order to explore the time pattern of the estimated coefficients, and as a further confirmation of the validity of our design, we also replicate our estimates by looking at the three years before and after the NSH, separately. Finally, we explore possible heterogeneity in the effect of the habilitation across different macro-areas.

[14]High-quality journals are defined as those journals that rank in the top 25% of the sector-specific distribution of the Scimago Journal Ranking (*SJR*).

Table 5: Quantitative effect of the habilitation

Panel A: Candidates for full professor						
	Dependant variable: Articles 2014-2016					
	(1)	(2)	(3)	(4)	(5)	(6)
	Quadratic	Quadratic	LLR-MSE	Quadratic	Quadratic	LLR-MSE
ITT	-1.698	-1.322	-1.298			
	(1.040)	(0.959)	(1.141)			
LATE				-4.575**	-4.639**	-4.635
				(2.295)	(2.257)	(4.023)
Sector FE	No	Yes	Yes	No	Yes	Yes
Mean Y_i	17.591	17.591	7.644	17.591	17.591	7.644
Observations	5383	5383	924	5383	5383	924
BW_1			7.689			7.689
BW_2			7.652			7.652
BW_3			3.025			3.025

Panel B: Candidates for associate professor						
	Dependant variable: Articles 2014-2016					
	(1)	(2)	(3)	(4)	(5)	(6)
	Quadratic	Quadratic	LLR-MSE	Quadratic	Quadratic	LLR-MSE
ITT	-0.844*	-0.135	0.129			
	(0.497)	(0.461)	(0.459)			
LATE				-2.824*	-0.871	0.507
				(1.572)	(1.456)	(1.755)
Sector FE	No	Yes	Yes	No	Yes	Yes
Mean Y_i	12.402	12.402	6.570	12.402	12.402	6.570
Observations	12320	12320	2847	12320	12320	2847
BW_1			5.702			5.702
BW_2			7.211			7.211
BW_3			4.030			4.030

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In (1), (2), (4) and (5) a quadratic specification is estimated on the full sample; in (3) and (6) a local linear regression within the optimal bandwidths is estimated. Optimal MSE-bandwidths in the LLR are computed following Imbens and Kalyanaraman (2009). ITT coefficients are the OLS coefficients of D_i in equation (3); LATE coefficients are the 2SLS estimates of β in equation (2). Y_i equals the number of articles published between 2014 and 2016 by scholar i.

The first set of result we present is about the quantitative effect of the habilitation. Table 5 presents the results from the estimation of equations (2) and (3) with OLS on candidates applying for full and associate professor, separately. Our dependant variable is defined as the sum of the articles published by each author in the period 2014-2016, that is, after the results from the first round of the NSH were made public. The estimated coefficient in the first three columns therefore represents the quantitative Intention-To-Treat effect of the habilitation, while its 2SLS counterpart - in columns 4 to 6 - captures the Local Average Treatment Effect (LATE).

Results from this first set of regressions document the presence of a negative effect of the achievement of the habilitation on the number of articles published in three subsequent years. Candidates habilitated as full professors published around 4.5 papers less than rejected candidates and the results are robust to different specifications and to the inclusion of academic field fixed effects. This does not seem to be the case when looking at the sub-population of candidates applying for associate professor, for which the effect of the habilitation is much smaller, if not zero.

To dig deeper the time pattern of the quantitative effect of the habilitation, we replicate previous estimates on the three years prior and after the first round of the NSH, separately. Results are provided in Table 6.

On the one hand, it is reassuring that we do not see any significant discontinuity in the years prior to the first call of the NSH, thus confirming the validity of our design. On the other hand, we find that the overall negative effect found before is mainly driven by 2015 and 2016, those years that are further in time from the date of the NSH. The magnitude of the coefficients suggest that candidates for full professor that passed the habilitation published around 2 papers less than rejected candidates in both years while the effect is much lower - around 0.6 articles - for candidates for associate professor, and not significant.

Two competing mechanisms can explain these results: either habilitated candidates decreased their publications after the NSH, or rejected candidates increased their scientific production. The nature of our data does not allow us to disentangle between the two. However, some back-of-the-envelop calculations suggest that, since the average number of published articles is higher in the years following the NSH, the second explanation holds. Indeed, the effect of the habilitation seems to partially offset the increasing trend in the number of articles for habilitated candidates at

Table 6: Quantitative effect of the habilitation, by year

Panel A: Candidates for full professor						
	Dependant variable: Number of Articles					
	2009	2010	2011	2014	2015	2016
LATE	0.300	-0.487	0.285	0.255	-1.926**	-2.176**
	(0.400)	(0.427)	(0.507)	(0.752)	(0.913)	(1.096)
ITT	0.104	-0.196	0.123	0.086	-0.608	-0.632
	(0.172)	(0.182)	(0.219)	(0.329)	(0.383)	(0.469)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	3.732	3.748	3.946	5.365	5.935	6.338
Observations	5221	5245	5259	5341	5379	5383

Panel B: Candidates for associate professor						
	Dependant variable: Number of Articles					
	2009	2010	2011	2014	2015	2016
LATE	0.081	-0.225	-0.076	0.298	-0.652	-0.638
	(0.278)	(0.319)	(0.305)	(0.693)	(0.619)	(0.706)
ITT	0.035	-0.066	-0.031	0.110	-0.134	-0.139
	(0.086)	(0.097)	(0.095)	(0.165)	(0.195)	(0.223)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	2.622	2.711	2.886	3.756	4.203	4.473
Observations	11902	12019	12085	12247	12302	12317

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In all columns a quadratic specification is estimated on the full sample. ITT coefficients are the OLS coefficients of D_i in equation (3); LATE coefficients are the 2SLS estimates of β in equation (2). Y_i equals the number of articles published, in each year, by scholar i.

the cutoffs, that keep publishing on average the same amount of papers than before (around 4) while the overall increase is mainly driven by those who were rejected in the first round of the NSH. Candidates who are closer to the medians but still below them might indeed have higher incentives to increase the quantity of published papers before the next call of the NSH in order to increase their chances to pass it.

To conclude our analysis on the quantitative effect of the habilitation, we split our sample in 6 distinct macro-areas - Mathematics and Physics, Chemistry and Biology, Medicine, Agriculture, Engineering and Psychology - and we check whether the effect of the habilitation is heterogeneous across sectors. Table 7 reports the estimated Intention-To-Treat and Local Average Treatment effects on the number of published articles after 2012.

Table 7: Quantitative effect of the habilitation, by macro-areas

Panel A: Candidates for full professor						
	Dependant variable: Number of Articles					
	Math&Phys.	Chem&Biol.	Medic.	Agric.	Engin.	Psych.
LATE	-7.492	-4.649	-4.699*	11.873	-4.736	-1.180
	(5.479)	(6.990)	(2.757)	(15.039)	(7.861)	(3.941)
ITT	-2.431*	-2.082	-2.871*	2.109	-0.754	-1.076
	(1.361)	(3.215)	(1.584)	(2.030)	(1.217)	(3.490)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	6.579	29.901	19.689	11.869	11.690	8.592
Observations	435	1166	1613	397	990	152
Panel B: Candidates for associate professor						
	Dependant variable: Number of Articles					
	Math&Phys.	Chem&Biol.	Medic.	Agric.	Engin.	Psych.
LATE	-3.039	-1.004	0.017	4.672	5.746	6.919
	(2.020)	(4.991)	(3.192)	(2.841)	(6.254)	(7.957)
ITT	-0.716	-0.396	0.141	1.418	0.555	1.798
	(0.617)	(1.412)	(1.113)	(0.953)	(0.757)	(1.881)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	5.820	22.614	12.436	9.180	8.829	8.504
Observations	986	2541	3633	962	1992	534

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In all columns a quadratic specification is estimated on the full sample. ITT coefficients are the OLS coefficients of D_i in equation (3); LATE coefficients are the 2SLS estimates of β in equation (2). Y_i equals the number of articles published, between 2014 and 2016, in each macro-area, by scholar i.

Results from Table 7 document that a negative discontinuity following from getting the habilitation as full professors is present is all sectors, except than in Agriculture. Coefficients are not significant in most cases, due to the small number of observations in each sector that reduces the power of the estimation.

Apart from looking at the quantitative effect of the habilitation, we also explore whether this affected the quality of the publications in the years following the first call. We replicate the estimation of equations (2) and (3) using as dependant variable the share of articles published in journals ranked in the first quartile of the distribution of the field-specific *SJR*, over the total number of published articles. It is important to point out that the share of articles published in journals belonging to the first quartile of the *SJR* should not be interpreted as the probability of publishing in a top-field journal but rather as the share of *relatively good* publications,

Table 8: Qualitative effect of the habilitation

Panel A: Candidates for full professor

	Dependant variable: % of articles published in Q1 Journals					
	(1)	(2)	(3)	(4)	(5)	(6)
	Quadratic	Quadratic	LLR-MSE	Quadratic	Quadratic	LLR-MSE
ITT	0.033	0.033	0.022			
	(0.023)	(0.021)	(0.059)			
LATE				0.080	0.071	0.073
				(0.050)	(0.047)	(0.183)
Sector FE	No	Yes	Yes	No	Yes	Yes
Mean Y_i	0.768	0.768	0.641	0.768	0.768	0.641
Observations	5188	5188	868	5188	5188	868
BW_1			7.689			7.689
BW_2			7.652			7.652
BW_3			3.025			3.025

Panel B: Candidates for associate professor

	Dependant variable: % of articles published in Q1 Journals					
	(1)	(2)	(3)	(4)	(5)	(6)
	Quadratic	Quadratic	LLR-MSE	Quadratic	Quadratic	LLR-MSE
ITT	-0.008	0.002	-0.002			
	(0.014)	(0.013)	(0.032)			
LATE				-0.027	0.005	-0.007
				(0.044)	(0.040)	(0.119)
Sector FE	No	Yes	Yes	No	Yes	Yes
Mean Y_i	0.746	0.746	0.663	0.746	0.746	0.663
Observations	11520	11520	2639	11520	11520	2639
BW_1			5.702			5.702
BW_2			7.211			7.211
BW_3			4.030			4.030

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In (1), (2), (4) and (5) a quadratic specification is estimated on the full sample; in (3) and (6) a local linear regression within the optimal bandwidths is estimated. Optimal MSE-bandwidths in the LLR are computed following Imbens and Kalyanaraman (2009). ITT coefficients are the OLS coefficients of D_i in equation (3); LATE coefficients are the 2SLS estimates of β in equation (2). Y_i equals the share of articles published in journals in the first quartile of the *SJR* between 2014 and 2016 by scholar i.

and therefore is a broader proxy for the quality of the scientific production. The coefficients of the Intention-To-Treat and the Local Average Treatment Effect are reported in Table 8, estimated, as usual, under different specifications.

The LATE of the habilitation on candidates as full professors is stable across

Table 9: Qualitative effect of the habilitation, by year

Panel A: Candidates for full professor						
	Dependant variable: % of articles published in Q1 Journals					
	2009	2010	2011	2014	2015	2016
LATE	0.010	0.005	-0.020	0.036	0.002	0.120*
	(0.091)	(0.076)	(0.079)	(0.068)	(0.071)	(0.066)
ITT	0.000	0.003	-0.013	0.014	0.006	0.061**
	(0.037)	(0.036)	(0.036)	(0.030)	(0.031)	(0.029)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	0.778	0.788	0.801	0.777	0.782	0.803
Observations	4496	4467	4547	4699	4678	4679
Panel B: Candidates for associate professor						
	Dependant variable: % of articles published in Q1 Journals					
	2009	2010	2011	2014	2015	2016
Habilitated	0.035	-0.014	0.028	0.002	0.003	0.023
	(0.065)	(0.073)	(0.072)	(0.058)	(0.059)	(0.061)
Scores above cutoffs	0.018	-0.000	0.007	0.001	0.001	0.007
	(0.021)	(0.023)	(0.022)	(0.019)	(0.020)	(0.020)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	0.745	0.755	0.770	0.746	0.757	0.784
Observations	9450	9463	9665	10024	9924	9892

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In all columns a quadratic specification is estimated on the full sample. ITT coefficients are the OLS coefficients of D_i in equation (3); LATE coefficients are the 2SLS estimates of β in equation (2). Y_i equals the share of articles published in journals in the first quartile of the *SJR,* in each year, by scholar i.

the three different specifications and close to 7%, even if not significant. As regards candidates as associate professors, coefficients are very close to 0. However, given our time span, we might not be able to capture a change in the quality of the scientific production: the reward from devoting more effort to produce high-quality research is likely to arise later in time. Coherently with this view, the only coefficient that is significant and around 12% is the one relative to 2016, as documented by Table 9.

As for the number of publications, this result can come from two possible explanations: either habilitated professors increased the average quality of their publications, or rejected candidates did the opposite. Performing the same back-of-the-envelop calculation we did when discussing the quantitative effect, together with the timing of the effect itself, suggests that this qualitative effect is mainly due to the increase

Table 10: Quantitative effect of the habilitation, by macro-area

Panel A: Candidates for full professor						
	Dependant variable: % of articles published in Q1 Journals					
	Math&Phys.	Chem&Biol.	Medic.	Agric.	Engin.	Psych.
LATE	0.162	0.113	0.113	0.283	-0.288	0.496**
	(0.517)	(0.126)	(0.123)	(0.325)	(0.462)	(0.238)
ITT	0.060	0.047	0.063	0.130	-0.112	0.177
	(0.163)	(0.059)	(0.070)	(0.153)	(0.103)	(0.288)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	0.634	0.831	0.833	0.725	0.772	0.671
Observations	334	1072	1410	346	835	117
Panel B: Candidates for associate professor						
	Dependant variable: % of articles published in Q1 Journals					
	Math&Phys.	Chem&Biol.	Medic.	Agric.	Engin.	Psych.
LATE	0.137	0.121	-0.104	-0.007	-0.016	0.074
	(0.246)	(0.124)	(0.212)	(0.239)	(0.368)	(0.320)
ITT	-0.010	0.036	-0.052	0.006	-0.005	0.031
	(0.080)	(0.034)	(0.078)	(0.074)	(0.054)	(0.122)
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_i	0.652	0.828	0.799	0.701	0.732	0.769
Observations	707	2190	2881	808	1562	407

Robust standard errors in parenthesis. ***p<0.01, **p<0.05, *p<.0.10. In all columns a quadratic specification is estimated on the full sample. ITT coefficients are the OLS coefficients of D_i in equation (3); LATE coefficients are the 2SLS estimates of β in equation (2). Y_i equals the share of 2016-articles published in journals in the first quartile of the *SJR*, in each macro-area, by scholar i.

in the average quality of publications for habilitated candidates. Indeed, while increasing research quality takes time, reducing it would not require such a time lag to emerge.

Finally, we present the estimated discontinuity in the share of publications in top-25% journals, in 2016, by sector. Results are reported in Table 10.

Results in Panel A confirm that, in 2016, the share of articles published in journals in the first quartile of the distribution of the *SJR* is larger for candidates that passed the habilitation as full professor in all sectors but engineering.

To conclude, the NSH also had a qualitative effect - although not significant - on candidates' scientific production, at least in the subsample of associate professors that applied to get the habilitation as full professors. This qualitative effect is mainly present in 2016, when habilitated candidates published a 12% larger share of articles in top-25% journals than rejected candidates. The same effect is not present in the subsample of candidates for associate professor.

3.7 Conclusions

In this paper we exploit the introduction of the National Scientific Habilitation - a centralized evaluation process for hiring full and associate professors in public Italian universities - to estimate the effect of passing the habilitation, thus getting closer to a tenured position, on academics' scientific production. The discontinuity in the probability of getting the habilitation induced by the introduction of a bibliometric rule - based on three different candidate-specific indicators - allow us to estimate a triple-rd setting, with multiple cutoffs and three running variables - in which we compare habilitated and rejected candidates around the cutoffs.

The first stage estimation shows that the bibliometric rule was strictly enforced only in those sectors that the reform identifies as *bibliometric*, whilst in *non-bibliometric* sectors other criteria were predominant. Indeed, scoring above the median professor sector leads to a 30% to 40% increase in the probability of getting the habilitation in *bibliometric* sectors - for candidates both for full and for associate professor - whilst no significant discontinuity emerges in the *non-bilbliometric* ones. Therefore, focusing on those bibliometric sectors, we estimate both the quantitative and the qualitative effects of getting the habilitation.

Our results document that the introduction of the NSH had a quantitative effect on academics scientific production, at least in the subsample of associate professors that applied to get the habilitation as full professors. Barely habilitated candidates published on average 4 papers less than their unsuccessful counterparts from 2014 to 2016, with the effect being stronger in the last two years. This result is common to all sectors with the only exception of Agriculture. We interpret this result as the consequence of the increase in the average number of published articles for rejected candidates more than the result of a decrease in the scientific production for the habilitated ones. Indeed, this explanation is consistent with the observed overall increase, in the *post*-NSH period, of the average number of published articles per year.

We do not find a significant effect in terms of the average quality of candidates' research production in the three years after the NSH. However, this might be due to the short-time horizon we are considering. Indeed, we find that candidates habilitated as full professors published a 16% larger share of articles in the first quartile of the distribution of the SCIMAGO Journal Ranking in 2016, if compared to rejected candidates. Furthermore, by applying the same reasoning as for the explanation of our quantitative result, we interpret this finding as a behavioral change of those on the right of the cutoff, rather than on its left. In other words, this result could be consistent with the idea that once achieved the habilitation, scholars invested in producing high quality research, for which rewards might arise later in time.

References

[1] Abramo, Giovanni and D'Angelo, Ciriaco, (2015), "An Assessment of the First "Scientific Habilitation" for University Appointments in Italy", *Economia Politica: Journal of Analytical and Institutional Economics*, 32, issue 3, p. 329-357.

[2] Bertanha, Marinho, (2016), "Regression Discontinuity Design with Many Thresholds", CORE Discussion Papers No 2016026, Université catholique de Louvain, Center for Operations Research and Econometrics (CORE).

[3] Borjas, George J. and Doran, Kirk, (2015), "Prizes and Productivity: How Winning the Fields Medal Affects Scientific Output", *Journal of Human Resources*, 50, issue 3, p. 728-758.

[4] Bricongne, Jean-Charles, (2015), "Do Prizes in Economics Affect Productivity? A Mix of Motivation and Disappointment", Working Papers, HAL.

[5] Brusa, Jorge, Carter, Michael and Heilman, George, (2010), "Academic Content, Research Productivity, and Tenure", *Journal of Economics and Finance*, 34, issue 1, p. 46-60.

[6] Calonico, Sebastian, Cattaneo, Matias and Titiunik, Rocio, (2014), "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs", *Econometrica*, 82, issue , p. 2295-2326.

[7] Chan, Ho Fai, Frey, Bruno, Gallus, Jana and Torgler, Benno, (2013), "Does The John Bates Clark Medal Boost Subsequent Productivity And Citation Success?", CREMA Working Paper Series, Center for Research in Economics, Management and the Arts (CREMA).

[8] Faria, Joao and McAdam, Peter, (2015), "Academic productivity before and after tenure: the case of the 'specialist'", *Oxford Economic Papers*, 67, issue 2, p. 291-309.

[9] Holley, John W.,(1977), “Tenure and research productivity”, *Research in Higher Education*, 6, issue.2, p. 181-192.

[10] Imbens, Guido and Kalyanaraman, Karthik, (2012), “Optimal Bandwidth Choice for the Regression Discontinuity Estimator”, *Review of Economic Studies*, 79, issue 3, p. 933-959.

[11] Levin, Sharon and Stephan, Paula, (1991), “Research Productivity over the Life Cycle: Evidence for Academic Scientists”, *American Economic Review*, 81, issue 1, p. 114-32.

[12] Oster, Sharon M. and Hamermesh, Daniel, (1998), “Aging And Productivity Among Economists”, *The Review of Economics and Statistics*, 80, issue 1, p. 154-156.

[13] Papay, John P., Willett, John B. and Murnane, Richard, (2011), “Extending the Regression-Discontinuity Approach to Multiple Assignment Variables”, *Journal of Econometrics*, 161, issue 2, p. 203-207.

[14] Zinovyeva, Natalia and Bagues, Manuel, (2015), “The Role of Connections in Academic Promotions, American Economic Journal: Applied Economics”, 7, issue 2, p. 264-92.

3.8 Appendix

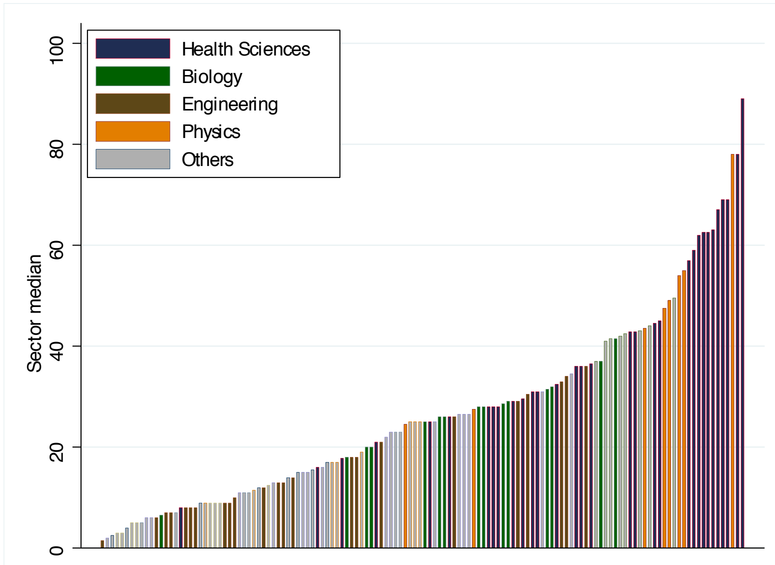

Figure 7: Distribution of cutoff 1, by sector, for applicants for full professors.

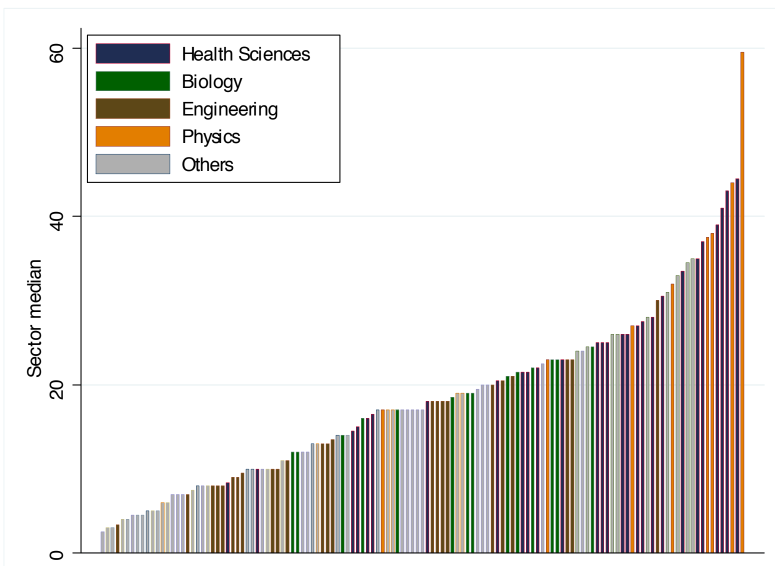

Figure 8: Distribution of cutoff 1, by sector, for applicants for associate professors.

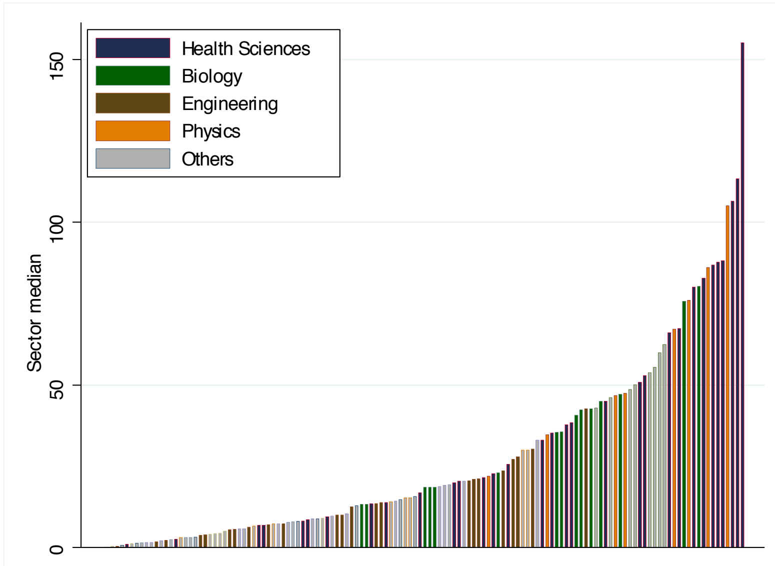

Figure 9: Distribution of cutoff 2, by sector, for applicants for full professors.

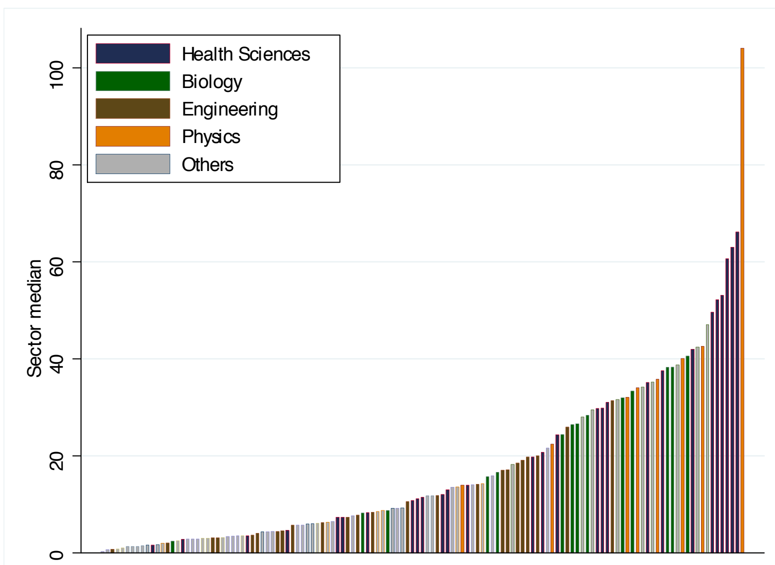

Figure 10: Distribution of cutoff 2, by sector, for applicants for associate professors.

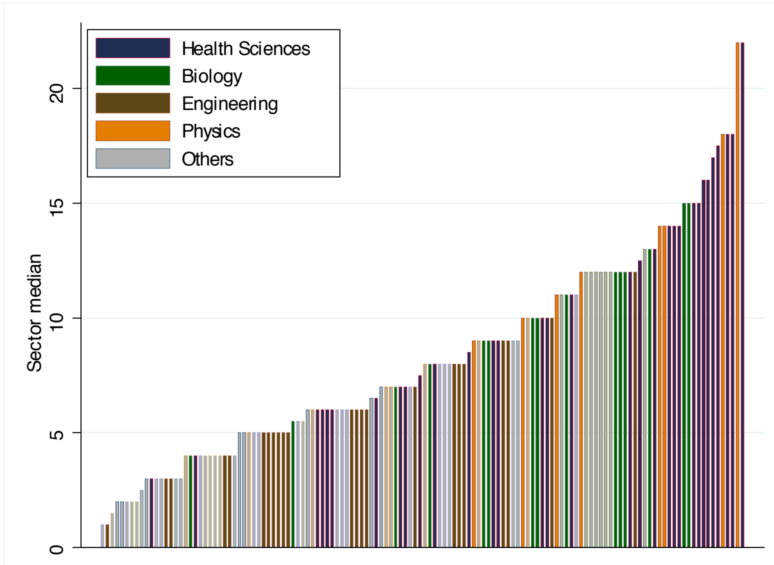

Figure 11: Distribution of cutoff 3, by sector, for applicants for full professors.

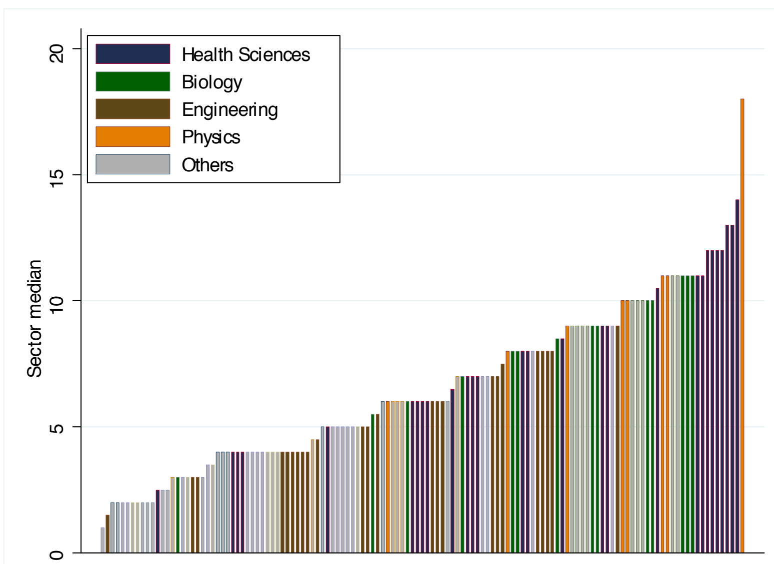

Figure 12: Distribution of cutoff 3, by sector, for applicants for associate professors.