RATIONALE

in a world without reason, turn to economics

2

Contents

Preface

The LSE SU Economics Society's Research Division is incredibly proud to present the second volume of Rationale, LSE's first ever student economics journal. Rationale was first established as a working paper series in 2019, by Christopher Dann, who was the previous Head of Research of the Society. This year, we have decided to take the next step and publish Rationale as a student research journal, as this will help ensure an even higher standard of quality. The recent progress of Rationale was made possible only through the foundations laid by Chris and the rest of last year's Research Division, to whom we owe our special gratitude.

This year's volume of Rationale is the result of the great efforts made by the members of the Research Division. We would like to express our immense gratitude to the working group leaders Ola Aboukhsaiwan, Francesco Casalena, Eleanor Jenke and Minsuk Kim for putting many hours of hard work into their projects, for keeping their working groups going in spite of many challenges during their research and for producing truly impressive papers.

The research projects were only enabled by the hard work of the Research Associates –thank you for your commitment and work.

Thank you also to the Editorial Board for your support and advice throughout the year and for providing helpful feedback and comments to the working groups.

The working groups benefited greatly from the support from professors and faculty in the Economics Department, including the Head of the Department Steve Pischke, Rachael Meager, Guy Michaels, Nico Rosetti and Canh Thien Dang, who we thank on behalf of the entire Research Division. We give special thanks to Judith Shapiro for her valuable support and advice throughout the year.

Last but not least, we thank the LSE Economics Society's executive committee members, for their support and help with organising Research Division activities.

We hope that the Research Division will continue to offer students the opportunity to learn about research practice and experience the enjoyment of pursuing your own ideas together with others. We thank the incoming Head of Research, Eddy Zou, for helping to make this possible and wish him the best of luck for the year ahead.

We hope you enjoy this year's edition of Rationale.

Kerry Neitzel
Head of Research and Editor in Chief 2019/20
Xuyi Yang
Deputy Head of Research and Editor 2019/20

Revisiting the Economics of Vocational Education: Estimating the Impact of Vocational Education on Labour Market Outcomes in Germany

Ola Aboukhsaiwan[1], Ju Yeon Kim (Anna)[1], Celine Mano[1], Caroline Maschka[1], and Lim Xiao Rong (Isabelle)[1]

[1]The London School of Economics and Political Science, London, UK

Abstract

This paper investigates the impact of vocational education on labour market outcomes in Germany. The hypothesis rests on the mechanism that educational attainment affects earnings potential, risk of unemployment, length of unemployment, and risk of termination by employer. Results are obtained based on a matching strategy and instrumental variable estimation exploiting the exogenous variation in German states' budgets allocations towards vocational education. We find no effect on wages and negative and significant effects on risk of unemployment and length of unemployment. The results of this paper call for more inventive policy development, particularly in the domain of skill formation in early life.

Acknowledgements

This research paper was the product of a large team effort and we are extremely grateful to each person who helped and encouraged us along the way. In particular: Kerry Neitzel for his continued support and assistance throughout the project; Dr. Canh Thien Dang for his valuable help with the econometrics; and Prof. Steve Pischke for providing us with access to the SOEP dataset.

1 Introduction

What are the economic benefits of investing in education? Who benefits the most and under which circumstances? These questions lie at the heart of the labour economics literature, which has attempted to estimate the returns to education over the past 50 years. Much of this work stems from the work of Becker (1962), which introduced the concept of treating investment in education as a capital investment. Since then, many papers have attempted to estimate the return to educational investment. For instance, Card (1999) investigated the causal effect of education on earnings and Moretti (2004) examined the positive externalities resulting from education.

There are many such externalities: education can grant a "passport" to better paying jobs, contribute towards a higher standard of living, and strengthen the prosperity of nations (Moretti, 2004). This is due to its use as a policy tool to facilitate the development of human capital. The economics of education, while referenced in earlier publications, only saw academic interest begin in around the 1960s. Mincer (1958) was the first to derive an empirical formulation of earnings over the lifecycle. Becker's Human Capital (1964) followed up on Mincer's findings, by discussing the importance of education planning in economics. While the 1970s saw a general decline in investment in education along academic skepticism of the existing education system (Blaug 1985), studies of that period focused on possible alternative education systems, including ones which alternated work and education periods (for instance, the influential Learning to Be; Fauré et al. 1972). Human capital theory gained prominence afterwards, suggesting that education and training are investments that increase the productivity of the learners (Becker, 2002; Mincer, 1958). Further research built on this idea: that labour productivity is one of the key drivers of GDP (Sala and Silva, 2013) in any country and that education also has greater benefits beyond monetary gains. For instance, research indicates that higher levels of schooling have a causal effect on health and life satisfaction (Oreopoulos and Salvanes, 2011), improves patience and other traits (Becker and Mul-

ligan, 1997), lowers crime rates (Lochner, 2004) and increases civic participation (Tenn, 2007).

However, modern governments have been increasingly challenged to develop educational solutions in response to recent technological changes and a persistent decrease in outcomes of non-college-educated workers (Autor, 2019). Governments around the world are becoming more interested in whether different types of secondary education (e.g. vocational) may provide young people with the skills they need to succeed after graduation (European Commission, 2010). Yet, in stark contrast to the growing body of evidence on the impact of various fields of study in higher education (Altonji et al., 2012; Kirkeboen et al., 2016), there exists a paucity of compelling causal evidence on the impact of vocational education on labour market outcomes.

Vocational education and training (VET) is frequently perceived as improving the opportunities of youths who lack the resources, skills or motivation to continue with higher education. Many have argued that VET provides useful skills to prepare young individuals for labour market entry and improve their chances of a successful professional career (Wolter and Ryan, 2011; Hanushek and Wößmann, 2008). Others have argued that VET allows students to develop more flexible skills, preparing them in a job market that may see great change due to automation (Krueger and Kumar, 2004).

The impact of vocational education is an empirical question. Empirical studies have attempted to estimate this, yet identification and measurement are a constant challenge. Little work has been done to address the empirical limitations associated with selection bias and endogeneity in estimating the impact of vocational education on key labour outcomes. As a result, the aim of this study is to investigate the effect of vocational education on various labour outcomes, including earnings, probability of unemployment, probability of being fired by an employer and length of unemployment. This paper employs a unique matching strategy and novel instrument which exploits the exogenous variation in German states' budget allocations towards vocational education over time.

The contribution of this paper is threefold. First, this paper fills the research gap in examining the impact vocational education has on key labour market outcomes in a relatively well-developed institutional context. Second, it uses an identification strategy that overcomes the shortcomings of previous research methods. Last, but definitely not least, the data used in this paper is at the individual-level, allowing for a rich analysis of human decision-making in response to policy decisions.

The paper is structured as follows. Section 2 offers a literature review, while Section 3 presents the empirical motivation. Section 4 describes results, robustness checks, and explores the heterogeneity in the main results. Section 5 concludes.

2 Literature Review

2.1 The German Education System

The paths into vocational or academic education vary widely within different countries, the structures and rigidity of which have large implications on labour market conditions. Germany is a particularly potent case to study given its institutionalised vocational training program, as well as a relatively rigid education system.

As Germany is largely federalised, its education system varies largely from state to state in timeline and flexibility. On a whole, German elementary school (Grundschule) lasts from four to six years, after which most states separate students into three main streams: Gymnasium (resulting in "Abitur," a level that grants a General Maturity certificate which allows entrance to university), Realschule (an upper intermediate maturity certificate) and Hauptschule (a secondary general school, resulting in a Vocational Maturity certificate upon graduation). The segmentation is primarily dependent on academic performance in the last year of Grundschule, although depending on the state, parents may have varying degrees of influence over the decision. Figure 1 provides an overview of the different education streams available in Germany.

Firstly, by going to Gymnasium, students earn a general or university maturity certificate, Abitur, after 12–13 years of education. It is the only certificate with which students can directly enter university and strictly academic tertiary education. This is the path taken by most Abitur graduates.

Secondly, a secondary education in Realschule entails a mix between vocational and academic education. It is of varying lengths, but usually formal academic education ends after the 10th year, from which point students pursue a vocational path through VET programs, or an academic path through Fachhochschule, or a mix through the Dual System. This path has the most variances inter- and intra-state.

Lastly, if entering the path with a Hauptschule education, students finish after 9–10 years of education, culminating in a Vocational Maturity certificate. Different paths are aligned for this, either going directly into apprenticeships, vocational education training, or through the dual system, which binds vocational training with education on

the tradesmanship path. The curricula are developed in close cooperation between employers' associations, trade unions and government institutions. Trainees receive school education at public vocational schools 1–2 days per week and on-the-job training within firms 3–4 days per week (Korpi and Mertens, 2003). All these paths are institutionalised, and in most cases result in a formal and nationally-recognised certification. After this stage, most graduates pursue relatively low-skilled labour, although there is the ability to gain further academic or technical education later on through Fachoberschulen and Berufsoberschulen schooling which is directed towards building upon the dual system.

To this end, many studies have shown that VET is extremely beneficial especially in regards to youth unemployment; for instance, Ryan (2001) suggests that VET programs such as apprenticeships have a significant positive effect on youth employment rates. Additionally, Hanushek et al. (2015) uses German data to note that the primary gains from VET programs are seen early in a individual's career, with greater employment prospects and higher wages compared to general education programs. Cörvers et. al (2010) state that VET graduates earn more in the initial phase of their careers and the life-cycle differences in earnings are more pronounced in Germany than in UK or the Netherlands, suggesting that Germany is a relevant case study to examine. Yet many papers indicate that these benefits are short lived. Hanushek et al. (2015) go on to argue that the positive effects of VET are offset by the lack of adaptability as a consequence of vocational schooling systems, eventually resulting in diminished employability. Schmillen (2018) investigates the relationship between occupational choice and unemployment in Germany, suggesting the rigidity of the VET system could negatively impact long-term employment prospects. Schmillen argues that since VET students are trained to enter only one occupation, they are more greatly impacted by changes in their chosen industry. In contrast, Brunello and Rocco (2017) analyze OECD countries, including Germany, and conclude that while the primary benefits of VET are seen in the beginning of a person's career, wage and employment returns to VET can be relatively high, exccept in nations where the relative supply of VET graduates are lower.

2.2 German Labour Market Conditions

The German labour market has long been of interest to many economists, due to its large reforms towards competitiveness in the last two decades, which have resulted in decreasing unemployment rates, increasing participation rates, and gradually increasing labour compensation. These improvements in conditions are thanks to its "effective

and unique combination of flexibility and rigidity in its labour market" (Schneider and Rinne, 2019).

The Hartz Reforms of the early 2000s arguably played a large role by increasing the monitoring of the unemployed, making flexible forms of employment more attractive, reducing unemployment benefit duration, and abolishing the long-term unemployment assistance (Schneider and Rinne, 2019). The reforms have helped increase the matching efficiency in all occupational labour markets (Stops, 2016).

Beyond low unemployment, Germany has also benefited from an increasing labour market participation rate. This has been especially the case for the older demographic segment, with a 30% rise from 2000 to 2018, significantly more than in comparable countries (Schneider and Rinne, 2019). This is thanks to its Active Labour Market Policy, which directly targets the unemployed and elderly (Boockmann and Brändle, 2018). Yet, this increasing employment of the older population has allegedly not come at the cost of youth employment - Germany has also been very effective in maintaining high youth employment rates, when compared to the rest of Europe - it is in fact one of the only countries in the OECD whose job prospect for the young have improved (OECD, 2019). Schneider and Rinne (2019) attribute this largely to Germany's dual apprenticeship and vocational training system, as it provides an "effective mechanism to provide the skills and qualifications in demand" and helps to reduce the barriers between education and employment through standardised certification of qualification.

Nonetheless, the German labour market still faces issues. Young workers and those without tertiary education face the most significant risks, being at threat for underemployment, unemployment and low paid work, and this is even further exacerbated for women (OECD, 2019). Furthermore, German jobs are at a high risk of automation, relative to the OECD average, with 10% of jobs being at risk - specifically again threatening lower-skilled workers.

Beyond this, Germany is faced with an aging population, with the portion of the population aged 20-64 set to decline 30% by 2060 (OECD, 2016). This will only further exacerbate Germany's existing labour shortages - specifically for low-skilled jobs, likely resulting in a skill shortage (Zöllner et. al, 2018). Recent increases in migration flows to Germany may help to mitigate these shortages, but only if integration policies are effective - as disparities persist with employment of people with migration background remaining 10% lower than people without (German Microcensus, 2013).

To sum up, the German labour market conditions have experienced many favorable

changes in the last two decades. Yet, new challenges exist with automation, an ageing population and low-skilled labour shortages rising to prominence. This indicates losses to the vocationally-educated by automation, but also opportunities given the large shortages.

3 Empirical Motivation

3.1 Data Sources

The dataset used for analysis is the German Socio-Economic Panel (SOEP) data. It is an independent, non-partisan research-driven infrastructure unit that serves the international scientific community by providing nationally-representative longitudinal data and related datasets on private households in Germany (Gerstorf and Schupp, 2016). It contains survey data from approximately 11,000 private households in Germany from 1984 to 2017 (however, eastern German states have data from 1990 to 2017). It is produced by the Deutsches Institut für Wirtschaftsforschung and is hosted by the European University Institute.

The advantages of the SOEP data are numerous. It is unique in that it covers thousands of households in Germany and has been collecting data every year since 1984 on economic and social circumstances, behaviour, and subjective well-being of individuals from an intergenerational life course perspective (Gerstorf and Schupp, 2016). This leads the SOEP data to have strengths in the diversity and number of themes and variables, the long duration of the study and the data format.

Additionally, we merged the SOEP data with data from the Federal Statistical Office of Germany (Statistisches Bundesamt). We leveraged data on the average expenditure per student of either all education types, general education and vocational training by each state on an annual basis. Data collected is from the 2000- 2019 editions.

3.2 Data Characteristics

We pulled individual socioeconomic panel data from the following SOEP files: pgen, bioparen, pbrutto, hbrutto, and ppfad. Using this data, we obtained our control variables (sex, years of work experience, state of origin, and whether or not they were born in Germany), our independent variable (an indicator =1 if they held the Vocational Maturity Certificate, =0 if they held the General Maturity Certificate), and our labour market outcome variables (net income, whether or not they have experienced unemployment,

total length of unemployment, and whether or not they have been fired before). Our process for obtaining our instrumental variables is described below.

In order to obtain our final sample, we performed 3 key steps. First, we retained respondents whose most recently observed highest educational certificate was the General Maturity Certificate or the Vocational Maturity Certificate. Second, we mapped a year of streaming to each unique individual based on their year of birth and their state's general streaming policy. The year of streaming refers to the year in which the individual would have entered either the vocational or general stream, assuming that the annual federal budget allocated to each educational stream would have a direct impact on stream-entry in the same year. We then mapped the corresponding budget data from the Federal Statistical Office of Germany to each unique individual based on their state and stream year. Finally, we dropped respondents whose year of streaming was before 1996 to create our final sample of the so-called "younger generation."

3.3 Identification Strategy

The identification strategy used in this paper relies on a novel way to address the fundamental selection issue that typically poses a threat to causal inference in questions related to the impact of educational choices made by individuals.

The simplest estimation technique, ordinary least squares (OLS), on all individuals in the sample may generate estimates that are upwards biased. This positive relationship may be driven by self-selection into education, i.e. individuals who have the most to gain from more education are more likely to stay. This will be the case, for example, if students with higher ability find studying easier, and would likely receive higher wages anyway. As such, the positive correlation observed between years of education and wages would partially reflect the premium on ability, and could not be interpreted as the returns from an additional year of education, as intended. Takers of vocational education are often considered to have lower ability relative to those who pursue general education in Germany. A raw comparison of labour outcomes across both groups would certainly suffer from bias as ability is unobservable.

Parental education (particularly, father's education) has been used as an instrument in the earnings equation to deal with the endogeneity problem of education. Recently, however, many have found that parental education can be a proxy for unobservable networking, which directly affects wages (Gong, 2018). We circumvent this issue by using a novel matching strategy and new instrument in our instrumental variables es-

timation. We match holders of the Vocational Maturity certificate (treatment group) with holders of the General Maturity certificate holders (control group). We create a synthetic control group composed of individuals who have not pursued further education, despite their capacity to do so as afforded to them by having achieved a General Maturity certificate (more commonly known as Abitur). We presume that those who drop out of the academic pipeline may be similar at an observational level, and ideally, also in terms of unobservable characteristics, such as ability, hence bringing us closer to a causal interpretation.

In our regressions, several control variables have been used to mitigate selection bias and omitted variable bias, including sex, state and work experience. Cho et. al (2015) suggest that women are significantly more constrained in their decision-making in vocational education. At the same time, men and women are largely influenced by the gender division in the labour market (OECD, 2017), supporting that sex is a confounder which must be controlled for. Also, VET is regulated and funded by both the federal government and the German states, and school curricula may vary between the different German states (Spees, 2018) while labour outcome varies between the German states, suggesting that the state variable is a confounder as well. Evidence also suggests that work experience is highly correlated with labour outcomes, as more experienced individuals are more likely to observe better outcomes in their careers. We also add a squared term to account for work experience's probable diminishing returns to learning-by-doing (Harmon, Oosterbeck and Walker, 2000).

Additionally, we exploit temporal and spatial exogenous variation in the federally allocated budget for vocational education across the 16 different states in Germany as an instrument. We posit that using the ratio of vocational education budget to each state's overall education budget could serve as a relevant instrument. A credible instrument must satisfy three conditions:

1- Relevance: the instrument must affect the probability of treatment. That is, in a regression of the treatment on the instrument, also known as the first stage equation, the coefficient on the IV must be sufficiently strong. Conceptually, we presume that the larger a budget is allocated to vocational training in a given state at a given time, the more probable it is that the quality of education will be higher via institutional investments, thereby attracting students into entering vocational education.

2- Instrument as good as randomly assigned: the instrument must be randomly assigned, in the sense that the instrument does not co-vary with the explanatory variables or any

of the unobservables captured in the error term. We have no reason to presume that the ratio of vocational education budget would have a direct relationship with any of the student's observed and unobserved characteristics. Perhaps there is reason to think that poorer states may be more inclined to develop their vocational education system. This is why we control for the state that the individual is from.

3- Exclusion restriction: the instrument affects the outcome exclusively via its effect on the treatment. The ratio of vocational education budget does not have a direct relationship with the labour outcomes of individuals, so we presume the exclusion restriction holds although this cannot be empirically tested.

3.4 Estimation Strategy

We estimate four key specifications using ordinary least squares (OLS) and instrumental variables (IV) regressions.

$$\begin{aligned} Lnwage_i = \beta_0 + \beta_1 Vocational + \beta_2 Yearsof Experience \\ + \beta_3 Yearsof Experience^2 + \beta_4 Sex + \beta_5 GermanBorn + \beta_6 State + \epsilon_i \end{aligned} \tag{1}$$

$$\begin{aligned} Unemployed_i = \beta_0 + \beta_1 Vocational + \beta_2 Yearsof Experience \\ + \beta_3 Yearsof Experience^2 + \beta_4 Sex + \beta_5 GermanBorn + \beta_6 State + \epsilon_i \end{aligned} \tag{2}$$

$$\begin{aligned} Fired_i = \beta_0 + \beta_1 Vocational + \beta_2 Yearsof Experience \\ + \beta_3 Yearsof Experience^2 + \beta_4 Sex + \beta_5 GermanBorn + \beta_6 State + \epsilon_i \end{aligned} \tag{3}$$

$$\begin{aligned} \text{LengthofUnemployment}_i = \beta_0 + \beta_1 Vocational + \beta_2 Yearsof Experience \\ + \beta_3 Yearsof Experience^2 + \beta_4 Sex + \beta_5 GermanBorn \\ + \beta_6 State + \epsilon_i \end{aligned} \tag{4}$$

where *Lnwage* is the logged wage of individual i, *Unemployed* is a binary indicator for whether individual i has ever been unemployed, *Fired* is a binary indicator for whether individual i has ever been fired by their employer, and *Length of unemployment* is the number of years individual i has been unemployed.

Among explanatory variables, *Vocational* is a binary indicator for whether individual i has obtained a Vocational Maturity certificate, *Years of Experience* is the number of years of work

experience that individual i has, *Years of Experience^2* is the number of years of work experience squared that individual i has, *Sex* is a binary indicator for whether individual i is a man or woman, *GermanBorn* is a binary indicator for whether individual i is born in Germany, and *State* is a categorical indicator for the state that individual i lives in.

4 Empirical Results

4.1 Preliminary Findings

Before discussing the analytical results of our specifications, we first discuss some exploratory findings of our sample.

Our final sample consists of 4,053 unique individuals. 3,269 individuals (81%) hold a General Maturity Certificate, and are thus assigned to the control group. Our treatment group constitutes of 784 individuals who hold a Vocational Maturity Certificate. We are missing 49.44% of log net income data from the total sample: 56.40% of the control group is missing this data while the 26.37% of the treatment group is missing this data. We see that within each group, gender is rather evenly balanced, although there is a slightly higher percentage of females in the treatment group as shown in Table 2.

Using a complete case analysis, we provide high-level descriptive statistics in Table 1. We find that both mean, median and minimum log incomes are higher for the treatment group than the control group, but the maximum log income is higher for the control group than the treatment group. Figure 2 clearly illustrates this. By comparing Figure 3 and Figure 4, which show income distribution of the sample, we can see that income is more equally distributed across treatment group while most of the control groups earn less than 1000 per month.

Table 3 shows the differences in probability of unemployment between the treatment and control groups. The treatment group appears to have a significantly lower propensity to experience unemployment than the control group: only 44.90% of the treatment group had experienced unemployment compared to 72.68% of the control group. Despite the lower propensity to experience unemployment, we observe that the treatment group has a higher average length of unemployment of 0.22 months compared to the control group's average of 0.15 months.

Table 4 shows the differences in probability of being fired between the treatment and control groups. The treatment group appears to have a higher propensity to be fired than the control group: 3.83% of the treatment group had experienced unemployment compared to 1.56% of the control group. In addition, as seen in Table 5, the control group has a more dispersed total length of unemployment with a higher maximum value 12 years, while in the treatment group, the total length of unemployment is mostly less than a year and the maximum length is around 8 years.

We performed t-tests to compare the equality of means of the control and outcome variables between the treatment and control group. Comparing the equality of means in Table 6, we

do not find statistically significant differences for the sex and German-born indicators. We do find that the treatment group has 1.78 years more work experience on average compared to the treatment group, significant at the 0.1% level. This is expected, as the nature of their vocational stream entails some work experience as part of their educational training.

We also looked at the composition of each group by states, and find that there are significant differences only for Hesse and Brandenburg at the 5% and 1% level respectively. A higher proportion of General Maturity Certificate holders are from Hesse, whereas a higher proportion of Vocational Maturity Certificate holders are from Brandenburg.

Upon further research, we find that Hesse's sectoral structure contrasts strongly with the rest of Germany. The majority of Hesse's active population is employed in the service sector (75.7%) whilst 23.7% work in the industry and construction sectors. Furthermore, in 2017, Hesse had the second largest share of companies with more than 250 employees in Germany, and one of the lowest shares of SMEs (Statistisches Bundesamt, 2019). This is in part attributed to Frankfurt's position as a key financial centre in Europe. The area around Frankfurt is characterised by a high industrial density for chemical and pharmaceutical industries, mechanical engineering and automotive industry (European Commission, 2019). These differences could create a higher demand for workers with general education, resulting in the higher observed proportion of General Maturity Certificate holders in Hesse.

We also explored the structure of Brandenburg's economy and found that it is relatively structurally weak, with traditional sectors being agriculture and the steel industry. Brandenburg's region tends to be less industry oriented in favour of the farming sector, and has the largest share of SMEs in Germany (Statistisches Bundesamt, 2019). This could point to a lower demand or focus towards general education in Brandenburg.

With this set of t-tests presented in Table 7, we find statistically significant differences between the treatment and control group for all outcome variables of interest. The treatment group has higher log income on average, lower proportion of individuals who have faced unemployment, higher length of unemployment and higher proportion of individuals who have been fired compared to the control group.

4.2 Discussion

In our following specifications, we have controlled for years of experience, years of experience squared, sex, German-born status and state origin. Our simple OLS estimates in Table 8 show that receiving vocational training in place of general training resulted in a 49.9% increase in wages, a 12.8% decrease in probability of unemployment, a 3.45% increase in probability of being terminated by the employer, and a 0.0823 months increase in length of unemployment experience on average, all of which are statistically significant at the 0.1% level.

The earnings specification suffers from a considerably larger number of missing data relative to the other specifications. The sample is missing 49.44% of total log net income data: 56.40% of

the control group is missing this data while 26.37% of the treatment group is missing this data. We suspect that there may have been sample selection issues, in so far as wages are concerned. This missing data might be correlated with employment status as unemployed people may have been more reluctant to report their earnings.

By contrast, our second stage IV estimates in Table 10 instead indicate that receiving vocational training in place of general training had no statistically significant effect on wages and probability of being terminated by employer, but resulted in a 78.8% increase in probability of unemployment and 0.931 months increase in length of unemployment on average. We observe that the OLS specifications are underestimating the effect of vocational education on the unemployment related outcomes, suggesting downward bias. We conjecture that this arises potentially due to differences in the underlying characteristics of both the treatment and control groups.

The raw estimate comparisons suggest that individuals who select into vocational education may be more skilled relative to their counterparts, who only complete Abitur but do not continue on for higher education. Through IV, we only compare individuals who would have a comparable level of skill, and find that the results reverse: vocational education is associated with negative labour market outcomes.

The F-statistic for the vocational budget ratio ranges from 18.922-23.232 for each IV regression and their associated outcome variables in Table 9, where the first stage regression output is presented. Given that the rule of thumb for a relevant instrument is F greater than 10 (Staiger and Stock, 1997), we find evidence that our first stage predictions are reasonably strong, suggesting we do not have a weak instrument problem.

Additionally, it is worth noting the policy relevance of our instrument, amount of budget allocated to vocational education in a given state. Through our instrumentation, we are identifying an effect for the group of compliers: individuals who are induced into taking up vocational education since there is more related funding in their respective states. The causal effect estimated is on precisely those individuals who are induced to change their behavior when the main policy lever of funding is adjusted.

4.3 Heterogeneity of Impacts

In this section, we discuss the empirical results achieved upon stratification. The availability of individual-level data allows us to test whether the effects of vocational education vary across genders, citizenship status (German born vs. non-German born), and financial crisis time window (2000-2008, 2009-2017).

We expect the effects of vocational education to vary across men and women. This is primarily driven by the well-documented female wage penalty across all percentiles of the income distribution (Behr and Theune, 2018). Haasler and Gottschall (2015) attribute these gender differences to the nature of Germany's vocational training system. They argue that the system strengthens gender-segmentation among professions, which perpetuates the gender-pay gap, as typically "fe-

male" professions such as childcare and nursing have noncompetitive pay. The stratified results are presented in Table 11. The hypothesis that women face a larger wage penalty is confirmed. Across unemployment measures, women also face higher risks of unemployment and their length of unemployment is also higher relative to the results found for men. Table 11 results confirm that for women experience almost doubled length of unemployment than men in average and higher probability of getting unemployed.

We expect the effects of vocational education to vary across German born and non-German born individuals. Firstly, non-Germans are more likely to hold jobs sectors such as the service and construction, which receive their labour flow from vocational education (Glitz, 2014). This can be in part be attributed to discriminatory biases, particularly against candidates of Turkish origin (Thijssen, Lancee, Veit, Yemane, 2019). Yet, research finds that the longer non-Germans spend in Germany, the smaller the differences in labour market outcomes become (Glitz, 2014). The stratified results are presented in Table 12. The results for this stratification are less conclusive. We observe that for non-German born individuals, results are insignificant across the board. However, for German-born individuals, we find that the Vocational Maturity certificate has stronger and more positive results on their risk of unemployment and length of unemployment. This could suggest that the vocational education system may be more tailored for German-born individuals, or that perhaps German-born individuals are more informed and familiar with the German labour market, allowing them to utilise their Vocational Maturity certificate more effectively.

Finally, we expect the effects of vocational education to vary both before and after the economic recession of 2007-2008. Well-established vocational education and training systems have become well-known in providing graduates with strongly institutionalised pathways to work. We expect that a byproduct of this institutional setup is that workers are sheltered from exposure to early job and employment insecurities (Helbling, 2016). The stratified results are presented in Table 13. Contrary to our hypothesis, we find that the probability of unemployment and length of unemployment is magnified in the period 2009-2017. This suggests that job security among the treatment group may be a concern. It may be that after a period of pronounced economic deterioration, institutionalised pathways to work along occupational lines may hamper occupational mobility for vocational education certificate holders. This poses risks to the treatment group on two fronts: graduates risk losing occupation-specific educational investment acquired during their training, but they may also be unable to use any of their knowledge and skills in the labour market. This may ultimately constrain upward mobility to limit employment gains.

4.4 Vocational Education: The Magic Bullet?

Across the political spectrum, the concept of vocational education has been supported. In Germany, vocational education has been positioned as a very common pathway to gain skills and embark on successful careers. The vocational education system is also highly integrated with the business sector, as unions and employer associations help develop curriculum and provide

apprenticeships for students (Ebner, 2015). However, with increasingly changing work environments, middle-class workers pursuing vocational education may be struggling to pivot as industries evolve.

Studies comparing the impact of vocational education typically suffer from selection issues. After controlling for this selection, we observe that even when being compared to individuals who only obtain a General Maturity certificate, individuals who take up vocational education are at an employment disadvantage, particularly from the perspective of job security. Our results indicate the vocational education increases the probability of unemployment.

There could be many reasons driving these results. We hypothesize that individuals with a General Maturity certificate are competing with individuals who have a Vocational Maturity certificate for similar jobs. Results are inconclusive due to over 50% of missing data in occupational breakdown in the SOEP data. With that said, top occupations across both vocational and general groups are health and social work and the retail sector, including repairs of motor vehicles and motorcycles. This may simply due to the size of the sector, and as such, we caution against hasty inference. However, this may also suggest that both our treatment and control individuals are working in similar sectors but garnering different employment advantages during their career.

Although we do not observe firm behavior in our dataset, it is entirely plausible that firms would rather let go of vocational workers rather than re-train them on the job. Our results are in line with evidence found by Lauer (2006), which in a French-German comparison, suggest that the Vocational Maturity certificate may be less closely connected to the labour market compared to the General Maturity certificate. This may be due to the fact that General Maturity certificate holders may have more general cognitive skills that give workers the ability to adapt to new circumstances and new jobs.

5 Conclusions and Implications

This study illustrates that vocational education does not have the employment advantages it has often been touted to have. This is potentially driven by systematic differences in unobservable traits, such as ability and motivation, between the treatment and control groups, even after matching to address some of the selection bias issues at hand. We observe gender differentials in the labour experience of the vocational certificate holder. In addition, we also find that the treatment group experiences job increased job insecurity in terms of probability of unemployment and length of unemployment in the time period after the 2007-2008 economic recession.

This paper takes on a reduced form approach, producing empirical results about the impact of vocational education on key labour market outcomes. We remain wary, however, of outstanding threats to causal inference. For example, it is plausible that federal budgets allocated towards vocational education may be responding to labour market conditions. There is also a need for developing structural theory to explain the underlying mechanisms driving the results

observed.

The streaming of students at a relatively young age is at the core of the pedagogical strategy and administrative organization of the German education system. There is a growing body of research suggesting that premature and excessive streaming may undermine equality of opportunity (Meier and Schütz, 2007). With that said, the notion that path dependency may have already taken its course is worth considering. We know that children's early experiences lay critically important groundwork for the rest of their lives, and that young children's health development depends on the quality of their earliest environments.

Early childhood policy is often viewed through the lens of "school reform" that manifests as a list of initiatives to fix schools. While there is nothing wrong with adapting policy to accommodate new information or to improve on past policies, there may be a greater need for root cause analysis policy that addresses the structural reasons for the observed heterogeneity across children's life outcomes. There is growing evidence that points to the positive life outcomes accrued from early childhood interventions, as early as birth. High-quality birth-to-five programs for disadvantaged children can deliver a 13% per year return on investment—a rate substantially higher than the 7-10% return previously established for preschool programs serving 3- to 4-year-olds (Luis et al., 2016).

Over the last decade, vocational training has gained prominence in policy discourse and is seen as a facilitator of employment and social mobility. This paper offers many policy implications for the great gains to be made by investing in the early and equal development of human potential. Potential extensions of our research work could critical explore whether vocational education further entrenches inequalities rather than alleviates it, and the extent to which it is able or unable to undo years of early disadvantage that students may experience as they go through the education pipeline.

References

Altonji, J., Blom, E., Meghir, C. (2012). Heterogeneity in Human Capital Investments: High School Curriculum, College Major, and Careers. *Annual Review Of Economics*, 4(1), 185-223.

Autor, D. H., (2019). "Work of the Past, Work of the Future." *AEA Papers and Proceedings*, 109: 1-32.

Baltes, B. B., Rudolph, C. W. (2010). Examining the effect of negative turkish stereotypes on evaluative workplace outcomes in Germany. *Journal of Managerial Psychology*, 25(2), 148-158.

Becker, F. (2002). Improving organisational performance by exploiting workplace flexibility. *Journal Of Facilities Management*, 1(2), 154-162.

Becker, G. (1962). Investment in Human Capital: A Theoretical Analysis. *Journal Of Political Economy*, 70(5, Part 2), 9-49.

Becker, G. (1964). Human capital. National Bureau of Economic Research.

Becker, G., Mulligan, C. (1997). The Endogenous Determination of Time Preference. *The Quarterly Journal Of Economics*, 112(3), 729-758.

Blaug, M. (1985). Where are we now in the economics of education?. *Economics Of Education Review*, 4(1), 17-28.

Boockmann, B., Brändle, T. (2018). Coaching, Counseling, Case-Working: Do They Help the Older Unemployed Out of Benefit Receipt and Back Into the Labor Market?. *German Economic Review*, 20(4).

Brandenburg. European Commission: Internal Market, Industry, Entrepreneurship And SMEs. (2020).

Brunello, G., Rocco, L. (2017). The effects of vocational education on adult skills, employment and wages: What can we learn from PIAAC?. Series, 8(4), 315-343.

Cho, Yoon Y., Kalomba, D., Mobarak, A. M., Orozco, V. (2013). Gender Differences in the Effects of Vocational Training: Constraints on Women and Drop-Out Behavior. IZA Discussion Papers 7408, Institute of Labor Economics (IZA).

Cornwell, C., Rupert, P. (1988). Efficient estimation with panel data: An empirical comparison of instrumental variables estimators. *Journal Of Applied Econometrics*, 3(2), 149-155.

Cörvers, F., Heijke, H., Kriechel, B., Pfeifer, H. (2011). High and Steady or Low and Rising? Life-Cycle Earnings Patterns in Vocational and General Education. *SSRN Electronic Journal*.

Card, D. (1999). The Causal Effect Of Education On Earnings. Handbook Of Labor Economics. Vol 3. Amsterdam: Elsevier Science.

Deutsches Institut für Wirtschaftsforschung (DIW), Berlin. (2016) SOEP Wave Report 2015.

Ebner, C. (2015). Labour market developments and their significance for VET in Germany: An overview. *Research In Comparative And International Education*, 10(4), 576-592.

Faure, E., Herrera, F., Kaddoura, A., Lopes, H., Petrovsky, A., Rahnema, M., Ward, F. (2020). Learning To Be: The World Of Education Today And Tomorrow. London: Harrap Co.

Federal Statistical Office of Germany (Statistiches Bundesamt). Statistisches Jahrbuch. Years 1999-2018.

García, J. L., Heckman, J., Leaf, D. E., Prados, M. J. (2016). The Life-cycle Benefits of an Influential Early Childhood Program. IZA Institute of Labor Economics Discussion Paper Series.

German Vocational Education. Vocational Education. (2020). Retrieved 11 March 2020, from https://vocationaleducation.weebly.com/german-vocational-education.html.

Gong, B. (2018). Like father like son? Revisiting the role of parental education in estimating returns to education in China. *Review of Development Economics*, 23(1), 275–292.

Hanushek, E., Schwerdt, G., Wiederhold, S., Woessmann, L. (2015). Returns to skills around the world: Evidence from PIAAC. *European Economic Review*, 73, 103-130.

Hanushek, E., Schwerdt, G., Woessmann, L., Zhang, L. (2016). General Education, Vocational Education, and Labor-Market Outcomes over the Lifecycle. *Journal Of Human Resources*, 52(1), 48-87.

Harmon, Colm., Oosterbeek, H., Walker, I. (2002). The Returns To Education. Dublin: Institute for Study of Social Change.

Helbling, L. A. (2016). Job and employment insecurity in early careers : investigating the vocational education premium. Doctoral Thesis, University of Basel, Faculty of Humanities and Social Sciences.

Hesse. European Commission: Internal Market, Industry, Entrepreneurship And SMEs. (2020).

Kirkeboen, L., Leuven, E., Mogstad, M. (2016). Field of Study, Earnings, and Self-Selection*. *The Quarterly Journal Of Economics*, 131(3), 1057-1111.

Korpi, T., Mertens, A. (2003). Training Systems and Labor Mobility: A Comparison between Germany and Sweden*. *Scandinavian Journal Of Economics*, 105(4), 597-617.

Krueger, D., Kumar, K. (2004). Skill-Specific rather than General Education: A Reason for US–Europe Growth Differences?. *Journal Of Economic Growth*, 9(2), 167-207.

Leibniz Institute for the Social Sciences. (2013). Metadata for the Social Sciences: Microcensus.

Lochner, L. (2004). EDUCATION, WORK, AND CRIME: A HUMAN CAPITAL APPROACH*. *International Economic Review*, 45(3), 811-843.

Meier , V., Schütz, G. (2007). The Economics of Tracking and Non-Tracking . No 50, Ifo Working Paper Series, Ifo Institute - Leibniz Institute for Economic Research at the University of Munich.

Mincer, J. (1958). Investment in Human Capital and Personal Income Distribution. *Journal Of Political Economy*, 66(4), 281-302.

Moretti, E. (2004). Workers' Education, Spillovers, and Productivity: Evidence from Plant-Level Production Functions. *American Economic Review*, 94(3), 656-690.

Organisation for Economic Co-operation and Development (OECD). (2016). Historical population data and projections: 1950 - 2050.

Organisation for Economic Co-operation and Development (OECD). (2017). Education Indicators In Focus.

Organisation for Economic Co-operation and Development (OECD). (2019). Jobs: OECD Data (Germany)

Oreopoulos, P., Salvanes, K. (2011). Priceless: The Nonpecuniary Benefits of Schooling. *Journal Of Economic Perspectives*, 25(1), 159-184.

Rinne, U., Schneider, H. (2019). The labor market in Germany, 2000–2018. IZA World Of Labor.

Ryan, P. (2001). The School-to-Work Transition: A Cross-National Perspective. *Journal Of Economic Literature*, 39(1), 34-92.

Sala, H., Silva, J. (2012). Labor productivity and vocational training: evidence from Europe. *Journal Of Productivity Analysis*, 40(1), 31-41.

Schmillen, A. (2018). Vocational education, occupational choice and unemployment over the professional career. *Empirical Economics*, 57(3), 805-838.

Spees, A. (2018). Could Germany's Vocational Education Training System Be a Model for the U.S.?. WENR.

Staiger, D., Stock, J. (1997). Instrumental Variables Regression with Weak Instruments. Econometrica, 65(3), 557-586.

Stops, M. (2016). Revisiting German labour market reform effects—a panel data analysis for occupational labour markets. *IZA Journal Of European Labor Studies*, 5(1).

Tenn, S. (2007). The Effect of Education on Voter Turnout. *Political Analysis*, 15(4), 446-464.

Wolter, S., Ryan, P. (2011). Apprenticeship. In E. Hanushek, S. Machin L. Wössman, Handbook of the Economics of Education, Vol. 3 (pp. 521-576). Elsevier.

Zoellner, M., Fritsch, M., Wyrwich, M. (2018). An evaluation of German active labour market policies: a review of the empirical evidence. *Journal Of Entrepreneurship And Public Policy*, 7(4), 377-410.

Figure 1: Overview of German Education Pipeline

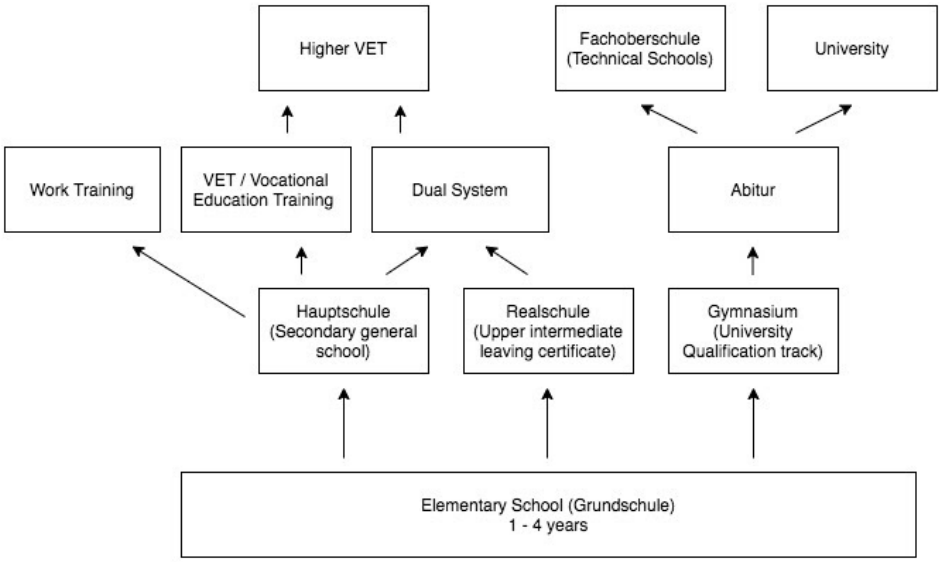
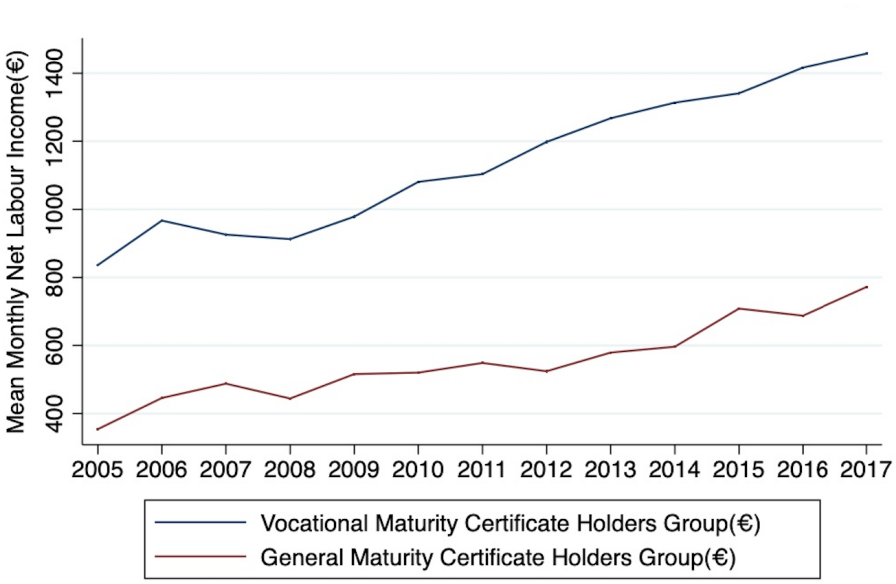
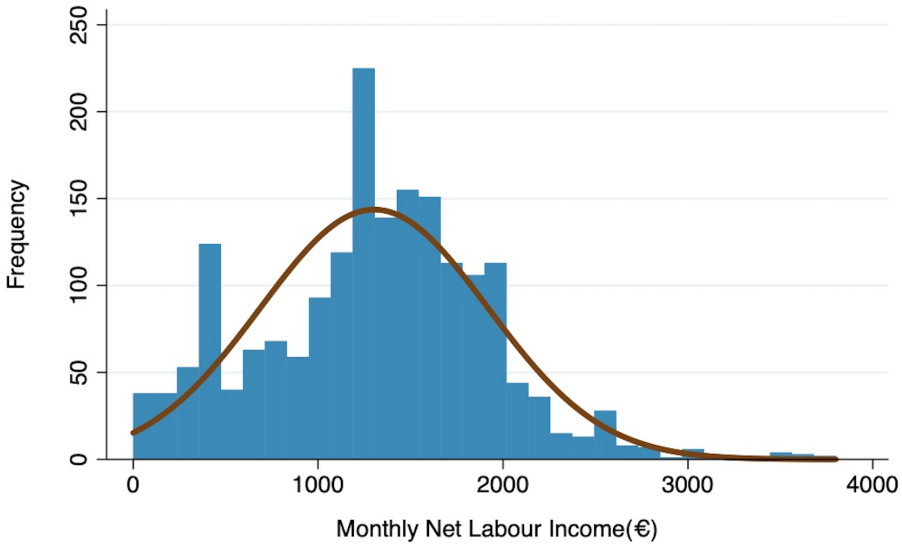


Figure 2: Income Over Time Across Treatment and Control Groups



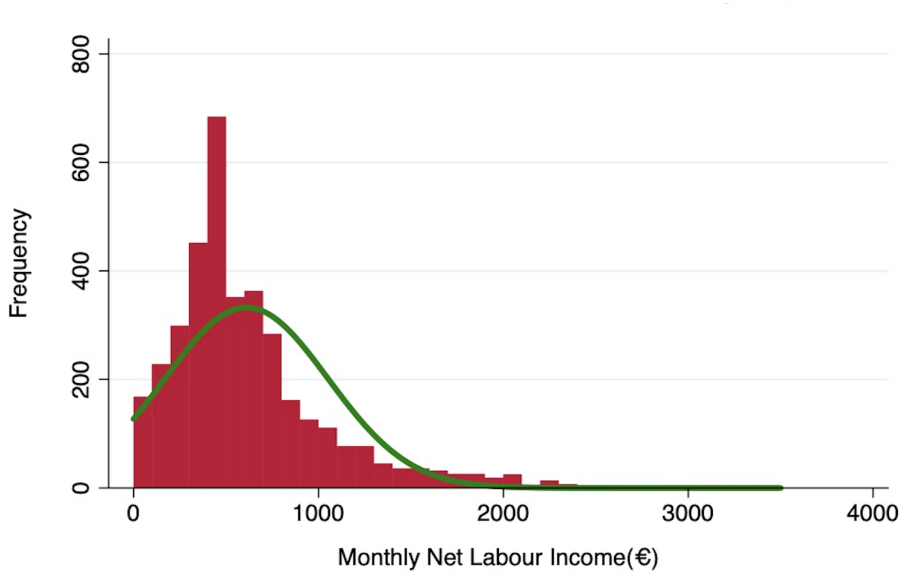
Notes: Dropped values in years below 2004 due to high volume of missing data among the treatment group.

Figure 3: Income Distribution of Treatment Group



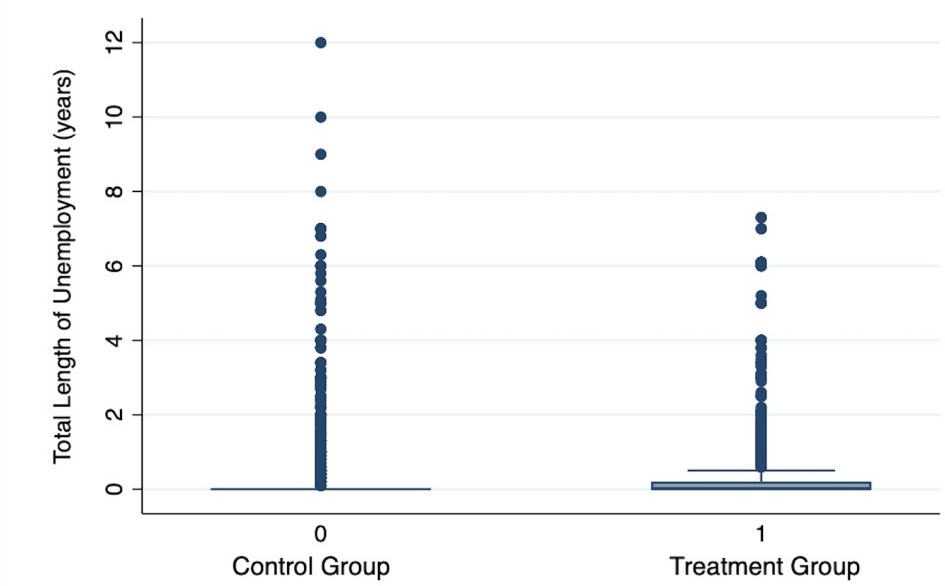
Notes: Dropped values over 4000 due to high volume of missing data.

Figure 4: Income Distribution of Control Group



Notes: Dropped values over 4000 due to high volume of missing data.

Figure 5: Length of Unemployment Distribution Across Treatment and Control Groups



Notes: Dropped missing values due to high volume of missing data.

Table 1: High-level descriptive statistics on treatment and control groups

	Count	Min	Max	Mean	Median	SD
General	3,639	2.708	9.105	6.207	6.215	0.723
Vocational	1,854	3.367	8.517	7.031	7.208	0.643
N	5,493					

Table 2: Differences in gender between treatment and control groups

	Male		Female		Total
	N	%	N	%	N
General	1,562	47.78	1,707	52.22	3,269
Vocational	345	44.01	439	55.99	784
Total	1,907	47.05	2,146	52.95	4,053

Table 3: Differences in probability of unemployment between treatment and control groups

	Never Unemployed		Experienced Unemployment		
	N	%	N	%	N
General	893	27.32	2,376	72.68	3,269
Vocational	432	55.10	352	44.90	784
Total	1,325	32.69	2,728	67.31	4,053

Table 4: Differences in probability of being fired between treatment and control groups

	Never Fired		Have Been Fired		
	N	%	N	%	N
General	3,215	98.47	50	1.53	3,265
Vocational	757	96.07	31	3.93	788
Total	3,972	98.00	81	2.00	4,053

Table 5: Differences in length of unemployment between treatment and control groups

	Count	Min	Max	Mean	SD
General	3,264	0	12	0.153	0.650
Vocational	789	0	7.3	0.222	0.660
N	4,053				

Table 6: T-test differences in means across control variables

	diff.	
Sex	-0.0374	(-1.89)
German-born	0.000922	(0.05)
Years of experience	-1.781***	(-24.59)
State		
Schleswig-Holstein	0.00722	(0.97)
Hamburg	0.00345	(0.65)
Lower Saxony	-0.000757	(-0.06)
Bremen	0.00446	(0.89)
North Rhine-Westphalia	-0.0185	(-1.08)
Hesse	0.0243*	(2.18)
Rhineland-Palatinate	0.00516	(0.66)
Baden-Wuerttemberg	0.00314	(0.25)
Bavaria	0.000903	(0.07)
Saarland	0.00245	(0.60)
Berlin	0.001000	(0.11)
Brandenburg	-0.0224**	(-3.01)
Mecklenburg-West Pomerania	0.0000416	(0.01)
Saxony	-0.00876	(-1.09)
Saxony-Anhalt	-0.00315	(-0.49)
Thuringia	0.00144	(0.21)
N	4,053	

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: T-test differences in means across outcome variables

	diff.	
LnWage	-0.825***	(-41.45)
Unemployed	0.276***	(15.24)
Fired	-0.0240***	(-4.33)
Length of Unemployment	-0.0690**	(-2.76)

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Key OLS regressions - Full sample

	(1)	(2)	(3)	(4)
	LnWage	Unemployed	Fired	Length Unemp.
Vocational	0.499***	-0.128***	0.0345***	0.0823***
	(0.0276)	(0.0204)	(0.0118)	(0.0164)
Years of Experience	0.245***	-0.0749***	0.0105*	0.0618***
	(0.0145)	(0.0116)	(0.00588)	(0.0230)
Years of Experienceˆ2	-0.0170***	0.00456***	-0.00155***	-0.00565***
	(0.00177)	(0.00132)	(0.000594)	(0.00163)
Sex	-0.122***	0.0220	-0.00272	-0.0113
	(0.0252)	(0.0157)	(0.0102)	(0.0189)
German born	0.0801**	-0.0224	0.0104	-0.299***
	(0.0314)	(0.0184)	(0.0104)	(0.0334)
Observations	5,493	10,869	10,869	10,869

Standard errors (clustered at the individual-level) in brackets

State included as control; Sample includes survey participants during years 2000-2017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: First stage IV regressions - Full sample

	(1) LnWage	(2) Unemployed	(3) Fired	(4) Length Unemp.
Vocational Budget Ratio	0.404***	0.399***	0.399***	0.399***
	(0.093)	(0.083)	(0.083)	(0.083)
Years of Experience	0.262***	0.267***	0.267***	0.267***
	(0.011)	(0.010)	(0.010)	(0.010)
Years of Experienceˆ2	-0.023***	-0.024***	-0.024***	-0.024***
	(0.002)	(0.001)	(0.001)	(0.001)
Sex	0.064***	0.052***	0.052***	0.052***
	(0.014)	(0.012)	(0.012)	(0.012)
German born	0.096***	0.088***	0.088***	0.088***
	(0.020)	(0.015)	(0.015)	(0.015)
First stage F-statistic	18.922	23.232	23.232	23.232
Observations	5,493	10,869	10,869	10,869

Standard errors (clustered at the individual-level) in brackets

State included as control; Sample includes survey participants during years 2000-2017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Second stage IV regressions - Full sample

	(1)	(2)	(3)	(4)
	LnWage	Unemployed	Fired	Length Unemp.
Vocational	-0.468	0.788***	0.150	0.931***
	(0.426)	(0.288)	(0.154)	(0.328)
Years of Experience	0.503***	-0.323***	-0.0209	-0.192**
	(0.114)	(0.0788)	(0.0426)	(0.0906)
Years of Experienceˆ2	-0.0391***	0.0265***	0.00123	0.0143*
	(0.00992)	(0.00713)	(0.00379)	(0.00805)
Sex	-0.0590	-0.0277	-0.00901	-0.0558*
	(0.0388)	(0.0244)	(0.0129)	(0.0298)
German born	0.169***	-0.102***	0.000257	-0.383***
	(0.0537)	(0.0347)	(0.0155)	(0.0527)
Observations	5,493	10,869	10,869	10,869

Standard errors (clustered at the individual-level) in brackets

State included as control; Sample includes survey participants during years 2000-2017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Second stage IV regressions - Stratified by gender

	Women				Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LnWage	Unemp.	Fired	Len. Unemp.	LnWage	Unemp.	Fired	Len. Unemp.
Vocational	-0.757	0.923**	0.125	0.963**	-0.092	0.710	0.279	0.540
	(0.676)	(0.399)	(0.219)	(0.465)	(0.459)	(0.537)	(0.280)	(0.944)
Years of Exp.	0.612**	-0.371***	-0.027	-0.255*	0.448***	-0.309**	-0.037	-0.013
	(0.204)	(0.128)	(0.071)	(0.147)	(0.116)	(0.127)	(0.076)	(0.226)
Years of Exp.ˆ2	-0.050***	0.033**	0.002	0.022	-0.034**	0.024**	0.002	0.000
	(0.00334)	(0.00153)	(0.000466)	(0.00186)	(0.00334)	(0.00153)	(0.000466)	(0.00186)
German Born	0.132**	-0.081**	0.010	-0.312***	0.163*	-0.133	-0.024	-0.370**
	(0.061)	(0.036)	(0.014)	(0.063)	(0.088)	(0.084)	(0.040)	(0.169)
Observations	3,038	5,948	5,948	5,948	2,455	4,921	4,921	4,921

Standard errors (clustered at the individual-level) in brackets

State included as control; Sample includes survey participants during years 2000-2017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Second stage IV regressions - Stratified by German-born status

	German born				Non-German Born			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LnWage	Unemp.	Fired	Len. Unemp.	LnWage	Unemp.	Fired	Len. Unemp.
Vocational	-0.783	1.277***	0.290	0.873***	0.488	-0.059	-0.09	1.599
	(0.518)	(0.482)	(0.250)	(0.298)	(0.424)	(0.287)	(0.134)	(1.214)
Years of Exp.	0.697***	-0.555***	-0.069	-0.203**	0.200*	-0.070	0.019	-0.162
	(0.0.163)	(0.167)	(0.087)	(0.100)	(0.074)	(0.044)	(0.021)	(0.147)
Years of Exp.ˆ2	-0.057***	0.047***	0.005	0.016*	-0.014**	0.004	-0.002	0.100
	(0.015)	(0.016)	(0.008)	(0.009)	(0.006)	(0.004)	(0.002)	(0.011)
Sex	-0.062*	-0.027	-0.007	-0.018	-0.208*	0.071*	-0.002	-0.197
	(0.036)	(0.028)	(0.014)	(0.016)	(0.078)	(0.042)	(0.020)	(0.124)
Observations	4,389	8,400	8,400	8,400	1,104	2,469	2,469	2,469

Standard errors (clustered at the individual-level) in brackets

State included as control; Sample includes survey participants during years 2000-2017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Second stage IV regressions - Stratified by time period (2000-2008, 2009-2017)

	2000-2008				2009-2017			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LnWage	Unemp.	Fired	Len. Unemp.	LnWage	Unemp.	Fired	Len. Unemp.
Vocational	-0.858	0.382	-0.0154	0.745***	0.152	0.436***	0.125	0.774***
	(0.840)	(0.490)	(0.264)	(0.361)	(0.225)	(0.167)	(0.0952)	(0.235)
Years of Exp.	1.506***	-0.462*	-0.001	-0.258	0.377***	-0.221***	-0.0136	-0.148**
	(0.673)	(0.248)	(0.153)	(0.216)	(0.0615)	(0.0454)	(0.0264)	(0.0647)
Years of Exp.ˆ2	-0.346**	0.0725	0.025	0.047	-0.0292***	0.0174***	0.000611	0.0103*
	(0.161)	(0.060)	(0.044)	(0.048)	(0.00561)	(0.00418)	(0.00235)	(0.00573)
Sex	-0.233***	-0.0779**	-0.00850	-0.0363	-0.0793***	-0.00126	-0.00702	-0.0452
	(0.089)	(0.039)	(0.030)	(0.025)	(0.0287)	(0.0201)	(0.0108)	(0.0284)
German-born	0.270	-0.0875	0.0492**	-0.0851	0.118***	-0.0755***	-0.000907	-0.379***
	(0.165)	(0.0556)	(0.0206)	(0.0568)	(0.0400)	(0.0267)	(0.0128)	(0.0503)
Observations	450	1,093	1,093	1,093	5,043	9,776	9,776	9,776

Standard errors (clustered at the individual-level) in brackets

State included as control; Sample includes survey participants during years 2000-2017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The Impact of Immigration on Average Income in Sweden

Minsuk Kim[1], Mikael Johansson[1], and Sherman Khoo[1]

[1]The London School of Economics and Political Science, London, UK

Abstract

The economic impact of immigration has been one of the most salient issues for many countries in Europe. Does the increase of immigration lead to negative economic outcomes for the local population? We study the effect of the rise in immigration density on the local income levels in all 290 municipalities in Sweden. We utilise the panel data of the average income in the region as well as the average income of identified ethnic groups to study the heterogeneous impact of immigration on the labour market. By using the internal instrument of immigration density level in 2011, we address the issue of reverse causality present between immigration density and local income levels. Moreover, we exploit the variation in immigration policy between Denmark and Sweden to conduct a Difference in Differences estimation on the impact of immigration. Our study shows that there was a negative impact of immigration on the average income level in Sweden. However, marginal income estimates provided evidence that immigrant labour acts as a complement to the native population, displaying a positive impact of 0.8% on the income of Swedish citizens for every 1% increase in immigration density in our two stage least squares estimation. Despite this positive economic impact on the native population, we find that having a 1% increase in immigration density in the previous year correlates to 2-3% increase in the vote of the far-right political party, Sweden Democrats.

1 Introduction

Immigration has been increasing in Sweden since the 1940s, particularly after 2014 and the onset of the European refugee crisis, the question of migrant economic impact rose to salience. Sweden took in far more refugees per capita in Europe during this period.[1] Sweden has had an open and humanitarian approach to immigration throughout recent history. From the 1940s to the 1970s, Sweden welcomed significant labour immigration from Eastern and Southern Europe, filling a domestic labour shortage in heavy industry and natural resources extraction sectors. The country's economy has been expanding rapidly as a result of a high demand for raw materials, ships and other goods, from the war-torn powers of Europe. The export-focused growth started to slow in the 1970s. From the 1980s a different type of migrant, most notably refugees and tied-workers, settled in Sweden in large numbers taking up low-skilled jobs.

At the beginning of the migration wave in 1980, Sweden implemented comprehensive integration schemes for migrants, offering housing, benefits and Swedish language classes. It was said to be one of the best cultural assimilation schemes provided in Europe, but it was also very costly. This level of service proved difficult to provide during the European refugee crisis: the influx of immigrants was too large to manage effectively. At its peak in 2016 Sweden took in 163,000 immigrants [2] most of whom were Syrian refugees. This considerable for a country with 9.8 million citizens. In addition, there was no shortage in low-skilled labour, leaving many refugees struggling to integrate and find employment. The Swedish labour market had changed dramatically, from one focused on heavy industry and shipping to a service-sector focused high-skilled economy, with an increased emphasis on Swedish language proficiency (Schön 2000).

This failure in assimilation and integration has led to an increase in anti-immigrant sentiment in Sweden, most clearly seen in the rise of the Sweden Democrat (SD) party, Sweden's most extreme anti-immigration party. In Sweden's long history of accepting refugees this is the first time there has been significant political backlash. Even in the Balkan crisis of the 1990s, Sweden took in numbers refugees fleeing the remnants of Yugoslavia. However, there was comparatively little opposition, despite the large cultural and ethnic differences. This break in trend suggests a probable change in voter sentiment on the impact of immigration. It is hypothesised that voters expect immigrants to have a negative economic impact on the incomes of domestic Swedes and also that they will

[1] https://foreignpolicy.com/2016/02/10/the-death-of-the-most-generous-nation-on-earth-sweden-syria-refugee-europe/

[2] https://www.migrationsverket.se/English/About-the-Migration-Agency/Statistics/Asylum.html

increase the burden on the Swedish welfare system. This view is supported by a report by the Institute of Futures Studies [3] which found that 31-37% of SD voters believe that immigrants take jobs from the natives. The study also found that SD voters also tend to worry about the cultural impact of the largely Muslim refugees on the Christian/Atheist Swedish culture and heritage. This may point to the possibility that the change in the opinion on migrants is not entirely due to economic reasons, but instead a part of the overall shift in European politics prevalent since the start of the refugee crisis. These hypotheses are often made without backing of empirical research, it can be hard for politicians to accurately assess their validity.

The primary objective of this paper is to assess the impact of immigration density on local income levels. Specifically, the paper focuses on the impact of immigration on the median income at the kommun (municipality) level, the most localised government body in Sweden. The rest of the paper is organised in the following way: the next section reviews the long-standing debate on immigrants and their impact on the labour market followed by the descriptive statistics. We next present the panel data analysis. To explore further, we carry out a Differences-in-Differences (DiD) estimation using Denmark as a counterfactual to Sweden. We use the immigration density in 2011 as the internal instrument for future immigration and carry out an Instrumental Variables Two Stage Least Squares (IV-2SLS) estimation. We then present an analysis of Gini coefficients, income percentiles and voting behaviour. The final section concludes.

1.1 Previous Relevant Literature

Considerable relevant literature today remains divided on the impact of immigration on the labour market. Borjas (1987) and Grossman (1982) for the United States found that an increase in the number of immigrants did not impact the wage levels of the native population. Pischke and Velling (1997) also found little to no evidence of immigrants displacing ethnically German labour in late 1980s. Altonji and Card (1991), using the 1970 share of foreigners as an instrument for 1970-1980 period, found that there was only a small displacement of local workers from increased supply of immigrant labour. More recent study by Manacorda et al (2010) found that despite significant rise of immigration in the UK, there was no appreciable effect on the average wages. They attributed this to the imperfect substitution of natives and immigrants in production.

However, the impact of immigration appears to be heterogeneous according to skill level. Zimmermann and De New (1994) found evidence that blue-collar German workers

[3] https://www.iffs.se/media/22618/swedendemocrats˙eng.pdf

in West Germany were negatively impacted by migration whilst white-collar German workers had benefited from increased wages. However, the effects did not offset each other and the overall impact of immigration on wages was negative. Similar results were found by Ruhs and Vargas-Silva (2020) in the UK, with low-waged workers more likely to lose out and medium and high-paid workers more likely to gain.

2 Descriptive Data

We constructed a panel data set[4] from 2008 to 2018 on 290 Swedish municipalities through Statistics Sweden[5]. However, the marginal income data for individual ethnic groups starts from 2011 rather than 2008. Our main covariates include average years of education and population density.

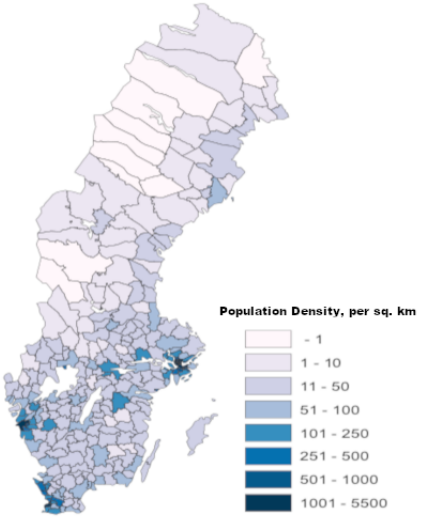


Figure 1: Population density of Swedish municipalities (per km^2)[6]

Immigration density is calculated by the share of foreign-born population in the municipality. From the above table it is evident that immigration density has been increasing steadily in Sweden since 2008, with a noticeable surge in 2014 and 2015 (an increase of 5.6 percent and 7.6 percent). This period of increased immigration corresponds to the period of Syrian refugee crisis, hence the increase can be attributed to Sweden's intake

[4]available upon request
[5]https://www.scb.se/en/
[6]2017. Image from Wikimedia Commons, Moralist / CC0. (Modified)

Table 1: Descriptive Statistics

	Mean Values		
	Average Income	Immigration Density	Population Density
2008	239701.9	.1024255	132.6497
	(21505.78)	(.0531566)	(451.3722)
2009	244201	.1059295	135.0141
	(22386.72)	(.0539617)	(464.3212)
2010	242049	.1090714	136.8514
	(23424.15)	(.0546902)	(473.5512)
2011	241247.2	.1119452	139.1772
	(24065.84)	(.055373)	(485.1847)
2012	246575.7	.1153815	141.0414
	(24651.37)	(.0559015)	(495.6904)
2013	252500.1	.1205177	143.4152
	(25283.86)	(.0567022)	(508.674)
2014	256334.7	.1269357	145.6669
	(26793.5)	(.0577307)	(519.4594)
2015	263839.7	.13405	148.2934
	(27830.26)	(.0588567)	(533.3965)
2016	270870.7	.1442842	151.0838
	(28271.28)	(.0601666)	(545.9557)
2017	274990.5	.1498849	153.7338
	(28711.08)	(.0612609)	(558.4769)
2018	275904	.1539196	156.1121
	(29234.71)	(.062707)	(568.313)

of immigrants. The variance of immigration density is also increasing over time, which most likely indicates that immigration is becoming more region-specific in Sweden.

A notable feature in Swedish population density is that the variation in density across the municipalities is very high. This is largely due to the fact that the majority of Sweden's land, especially the northern region is sparsely populated as it can be seen in Figure 1. Most of Sweden's population is aggregated in the southern, urban municipalities, creating the high variance in population density. Later we explore the potential heterogeneous effect from the urban-rural divide in Sweden.

Due to the 2008 recession, median income per capita is increasing overall due to the economic recovery. This time-specific trend is visible in the data and we use time fixed effects and first differences to tackle this issue.

3 Panel data analysis

We first run an Ordinary Least Squares (OLS) regression on our data.

The regression equation is:

$$\log\left(AverageIncome\right)_i = \alpha_i + \gamma \log\left(ImmDensity\right)_i + \delta \mathbf{X} + \epsilon_i \tag{1}$$

$\log(AverageIncome)$ is the logarithmised median income, $log(ImmDensity)$ is the logarithmised immigration density and X includes all the controls. It is important to note that due to the changes in immigration density being very large, we have logarithmised it in order to facilitate interpretation. For instance, if the immigrant density in the region increased from 25% to 27.5%, this would be a 10% increase rather than a 2.5 percentage point increase. We believe that smoothing out the function is more representative of shocks to the labour market. We control for years of education in the municipality since it could indicate a concentration of high-skilled labourers, which will affect the general wage level in the municipality. For example, a university town like Uppsala that has a higher education level affects both the immigration density due to international population as well as high average income due to high levels of education.

The above plot shows the cross-sectional estimate of immigration density on income over the time period available on our data. With reference to figure 2, we see that there is a general negative trend, indicating that the effect becomes stronger over the years. When the data is pooled, the effect of immigration density on average income is positive

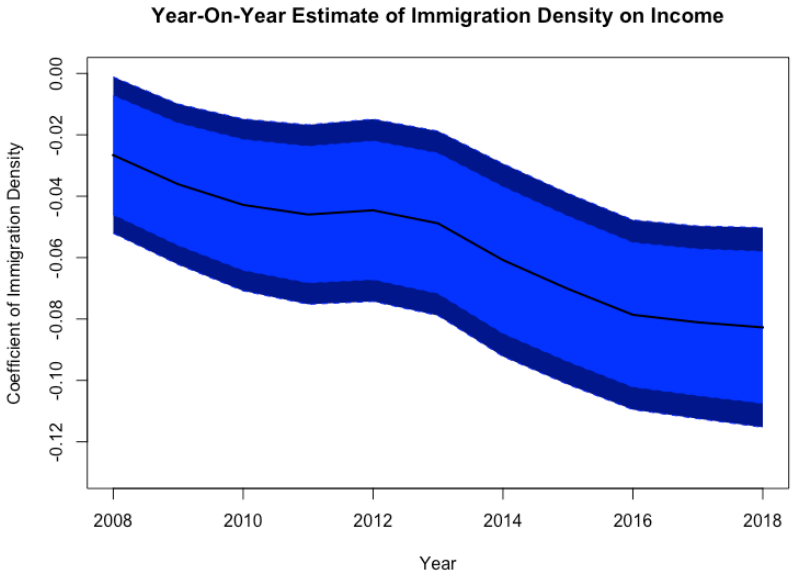


Figure 2: Coefficient against year

and insignificant. However, this estimate evidently ignores endogeneity problems that are present in the data such as the serial correlation in states and variation across time. Thus, the pooled OLS model does not reflect the causal relationship between immigration density and income.

We then use the fixed effects panel data model. Our main specification is as follows:

$$\log\left(AverageIncome\right)_{it} = \alpha_t + \beta_i + \gamma \log\left(ImmDensity\right)_{it} + \delta_1 \log\left(PopDensity\right)_{it} + \delta_2 YearsOfEducation_{it} + \epsilon_{it} \tag{2}$$

Fixed effects remove the state-specific income level present in the regression. This would be the case where a municipality has inherent differences that is uncaptured by our specification. These involve municipalities' geographical location and infrastructure, which are important in Sweden because of its polarized population distribution. As it can be seen in Figure 1, southern municipalities are much more populated and offer better infrastructure compared to sparsely population northern municipalities.

The results of the first panel data regression show a statistically significant positive effect, with a 1% increase in immigration density increasing average income by 10.4% (Table 2). This is likely due to the economic recovery post 2008 financial crisis. We introduce the

year fixed effects in the following regression and the coefficient on $log(ImmDensity)_{it}$ changes to negative 4%, which is highly significant. If this is a causal estimate, it is likely to be aligned to the hypothesis that migrant labour is a substitute for local labour. These results mean that observables included in the year fixed effects, such as a positive financial shock recovery, are correlated to both income and immigration density in the first panel data regression.

To control for heterogeneous effects of financial shocks over time in each state, we included the state-specific time trends, presented in the third column. These are largely accounting for time trends within each municipality, which may occur when financial recovery is faster in one state than the other due to having a different economic structure. The coefficient on $log(ImmDensity)_{it}$ is still negative and significant, with a 1% increase in immigration density leading to a decrease of 3.735% in average income. We used clustered standard errors to robustify our results. Though the standard error nearly doubles, the coefficient of negative 3.7% remains highly significant.

However, average income level alone does not prove the detailed labour mechanism that drives the relationship with immigration density. Hence, we present marginal incomes of each ethnic groups to evaluate those most affected by the negative shocks.

From table 3, it can be seen that in fact, Swedish income is positively affected by the increase in immigration density. Contrastingly, we see that immigrant income plummets, providing evidence for our alternative hypothesis that native workers and migrant workers are complements, not substitutes. Nordic immigrant's income only decreases by a small amount compared to other immigrants' income, showing that the negative shock to income is perhaps a function of how assimilated workers are to the Swedish population. Intuitively, discrimination in the labour market should be lower for immigrants who come from countries which are culturally similar to Sweden. Notably, Nordic citizens have a permanent residency in Sweden, allowing complete freedom of labour across these countries, giving little room for income discrimination.

More importantly, the negative effect is pronounced for the income of Asian immigrants, where one percent increase in immigrant density leads to a drop in income by more than 150%. It is evident that the negative 4% drop in overall income that we saw above is largely due to the Asian population absorbing this effect. On the contrary, we see insignificant results for EU citizens. This is likely due to the great heterogeneity among EU migrants, ranging from immigrants with an educational and cultural background

Table 2: Panel Data Regressions

	Dependent Variable: log Average Income				
	(1)	(2)	(3)	(4)	(5)
	OLS	FE regression	FE regression	FE regression	FE regression
log(ImmDensity)	0.00251	0.10395***	-0.04087***	-0.03735***	-0.03735***
	(0.00359)	(0.00384)	(0.00294)	(0.00436)	(0.00774)
log(PopDensity)	0.00953***	0.18526***	-0.02158***	0.01132	0.01132
	(0.00118)	(0.01492)	(0.00828)	(0.01129)	(0.01490)
YearsOfEducation	0.16827***	0.36397***	0.20481***	0.02033*	0.02033
	(0.00463)	(0.00861)	(0.00973)	(0.01222)	(0.02000)
Observations	3,190	3,190	3,190	3,190	3,190
R-squared	0.48972	0.85157	0.96150	0.98449	0.98449
State FE	NO	YES	YES	YES	YES
Year FE	NO	NO	YES	YES	YES
State-specific Time-Trend	NO	NO	NO	YES	YES
Cluster	NO	NO	NO	NO	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

similar to the Swedes to those from less developed countries who have lower education levels and are culturally further away. The overall impact is negative on average.

We then explore the urban rural heterogeneity in Sweden. As we have seen from Figure 1, Sweden is sparsely populated and the labour market may behave very differently according to the population density. To assess this potential heterogeneity of states, we created an indicator variable $PopDense$ to show the effect of being in an urban area. Then, we interact this term with the measure of immigration density $log(ImmDensity)_{it}$. Equation 2 shows this regression:

$$\log\left(AverageIncome\right)_{it} = \alpha_t + \beta_i + \gamma \log\left(ImmDensity\right)_{it} + \delta_1 \log\left(PopDensity\right)_{it} + \delta_2 YearsOfEducation_{it} + \delta_3 PopDense + \delta_4 PopDense \times \log\left(ImmDensity\right) + \epsilon_{it} \tag{3}$$

$\gamma+\delta_1+\delta_4$ shows the impact of immigrant density in cities and $\delta_1+\delta_4$ is the total additional impact from being in an urban area rather than a rural area. As seen from table 4, all columns show that immigration density has a negative impact on income in urban regions, presumably due to less skilled migrants going to cities where they will have better economic prospects in the low-skill service sector. EU citizens display a clear positive coefficient in rural areas and a negative coefficient in urban areas. It is plausible to think that this is indicative of a selection bias of EU citizens going to different municipalities in Sweden. Furthermore, the native Swedish population is affected negatively in urban areas, perhaps indicating the results from Zimmerman that low skilled local population are affected by the competition from immigration. The Asian immigrant population is affected significantly more when they are in an urban area (-130% as opposed to -380%). This is likely due to the labour market supply increase being larger in cities, though the more than double the amount of drop in income is notable. It is evident that immigrants are the ones mainly affected by the surplus labour with the effect being especially large in cities.

However, there are several issues with our identification. The main one is endogeneity. To be specific, our estimates are likely to be affected by reverse causality. Immigrants tend to go to economically prosperous regions to find work and hence the local income levels will be correlated regardless of the effect from migration. This would mean that our estimate also includes the effect of local wage levels as well as the effect of migrants on income. This would certainly create a bias for our estimates. Furthermore, whilst

Table 3: Marginal Income FE models

	Dependent Variable: log Average Income					
	(1)	(2)	(3)	(4)	(5)	(6)
	Native Swedish	All immigrants	Scandinavian	EU	Asian	Refugees
log(ImmDensity)	0.01247***	-0.55149***	-0.04901	-0.04821	-1.65779***	-1.52349*
	(0.00467)	(0.08352)	(0.03691)	(0.09991)	(0.24308)	(0.78159)
log(PopDensity)	0.02123**	0.22054	0.07425	0.30643*	0.33120	-0.24722
	(0.01045)	(0.22081)	(0.11669)	(0.16019)	(0.36199)	(1.27390)
YearsOfEducation	0.01401	0.14352	0.05231	-0.19738	-0.71734	2.81428
	(0.01275)	(0.10494)	(0.08082)	(0.18430)	(0.56031)	(2.31864)
Observations	2,320	2,320	2,320	2,320	2,320	2,153
R-squared	0.99472	0.85499	0.86746	0.82379	0.62623	0.25971
Number of code	290	290	290	290	290	290
state FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
State-specific Time-Trend	YES	YES	YES	YES	YES	YES
Cluster	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Marginal Impact in cities

	Dependent Variable: log Average Income					
	(1)	(2)	(3)	(4)	(5)	(6)
	Native Swedish	All immigrants	Scandinavian	EU	Asian	Refugees
log(ImmDensity)	-0.04145***	0.01586***	-0.48030***	-0.01603	0.13691***	-1.27913***
	(0.00330)	(0.00291)	(0.01872)	(0.01272)	(0.02827)	(0.07734)
log(PopDensity)	-0.04187***	-0.04227***	-0.10529	-0.05504	-0.32002**	-1.80002***
	(0.01582)	(0.01393)	(0.08968)	(0.06094)	(0.13547)	(0.37059)
PopDense× log $(ImmDensity)$	-0.01813***	-0.01967***	-0.03742	-0.02757	-0.12858**	-0.76862***
	(0.00680)	(0.00599)	(0.03855)	(0.02620)	(0.05824)	(0.15932)
YearsOfEducation	0.15394***	0.10060***	0.17219***	-0.00220	-0.01463	-0.40918
	(0.01076)	(0.00947)	(0.06098)	(0.04144)	(0.09212)	(0.25199)
Observations	2,320	2,320	2,320	2,320	2,320	2,320
R-squared	0.97097	0.98869	0.67457	0.72946	0.66325	0.29878
Number of code	290	290	290	290	290	290
State FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

the use of clustered standard errors gets rid of serial correlation, we are not exploiting the additional information available.

Our first robustness check is using clustered standard errors. Though the coefficients remained significant, we saw that clustered standard errors were much larger than the original standard errors (0.00436 compared to 0.00774). This suggests the presence of serial correlation. To take this into account, we provide the First Differences estimates below.

The first-differences specification shows a dramatic change in estimate sizes for the average income. This is likely to mean that the two-way fixed effects model is not efficient compared to our first differences model due to time series issues. If we apply the first-differences specification to the subgroups of immigrants, we see that for all types of immigrants the coefficients change.

In table 6, Coefficients reduce by a noticeable margin for the First Differences estimate, providing further evidence of the presence of serial correlation. Swedish citizens are still unaffected by the increased immigration. The coefficients are both negative for Nordic and EU citizens. However, they are insignificant and relatively small. Most notably, the Asian population that have just arrived (Asian having been in Sweden for one to two years), which comprised of mostly refugees, receive a large hit in their income, with additional immigrant population in the region.

Overall, from the panel data analysis we see consistent results that the native Swedish population were not affected by the increase in immigration density. Rather, this negative income shock was absorbed by immigrants, especially by the Asian population, who have little bargaining power nor the cultural assimilation required to compete on the same wage level. However, in urban areas where low-skilled workers are concentrated, all groups were negatively affected by the additional immigration.

Table 5: Robustness check

	Dependent variable: log Average Income	
	Twoways FE vs First Differences	
	(1)	(2)
log(ImmDensity)	-0.038***	0.018***
	(0.003)	(0.006)
YearsOfEducation	0.139***	0.155***
	(0.011)	(0.016)
log(PopDensity)	0.00003***	0.00004**
	(0.00001)	(0.00002)
Observations	2,320	2,030
R^2	0.210	0.051
Adjusted R^2	0.094	0.049
F Statistic	179.402***	36.013***

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Marginal FD

	Dependent variable: log Average Income					
	(1)	(2)	(3)	(4)	(5)	(6)
	Native Swedish	All immigrants	Scandinavian	EU	Asian	Refugees
log(ImmDensity)	0.01397^***	-0.38451***	-0.02487	-0.00230	-1.04057***	-1.09082**
	(0.00510)	(0.02700)	(0.01988)	(0.04391)	(0.11560)	(0.48329)
log(PopDensity)	0.00005***	0.00011	0.00005	0.00006	0.00001	0.00046
	(0.00002)	(0.00008)	(0.00006)	(0.00014)	(0.00036)	(0.00173)
YearsOfEducation	-0.09042***	-0.26628***	-0.09097*	-0.44450***	-1.47839***	-0.37186
	(0.01327)	(0.07027)	(0.05175)	(0.11430)	(0.30094)	(1.33492)
Observations	2,030	2,030	2,030	2,030	2,030	1,863
R^2	0.03305	0.09308	0.00219	0.00776	0.04243	0.00286
Adjusted R^2	0.03162	0.09174	0.00071	0.00629	0.04101	0.00125
F Statistic	23.08180***	69.31258***	1.48369	5.28367***	29.92614***	1.77573

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4 Difference in Differences

Sweden and Denmark both follow the Nordic model, which is characterised in Alestalo et al. (2009) as having three qualities; a strong state, a commitment to equality and universal social rights. This, along with the similarities in GDP per capita, has led us to assess Denmark as a possible counterfactual to Sweden. The European refugee crisis occurred in 2014, where a large number of refugees arrived in various EU countries. However, the refugee influx did not affect every EU country symmetrically. In particular, Denmark was noticeably less affected by the inflow of refugees during this period of time compared to Sweden. Sweden accepted the largest number of asylum seekers per capita in the EU whilst Denmark on the other hand was significantly less liberal in its refugee policy. We believe that this could be the exogenous variation in refugees necessary to assess the impact of immigration. In Hernes et al. (2019), three hypotheses for cross-country differences in labour market outcomes of refugees are posited: differences in refugee population characteristics, integration programme measures and refugee settlement patterns. Sweden adopted an integration policy that focused more on educated refugees, as opposed to Denmark's approach of focusing on less educated refugees. Secondly, with regards to settlement models, Denmark adopted a "steered" settlement model, limiting possibilities of refugees to self-settle, which allowed a more dispersed settlement of refugees compared to Sweden. Hence, we seek to exploit the exogenous variation of refugee policy between Sweden and Denmark to compare the changes over time of local income levels between these two countries.

To ensure that Denmark is a valid counterfactual in our model, we ran a panel regression on the Danish data similar to the one done in the earlier panel data analysis.

$$\begin{aligned}\log\left(AverageIncome\right)_{it} = \alpha_t + \beta_i \log\left(ImmDensity\right)_{it} + \delta_1 \log\left(PopDensity\right)_{it} \\ +\delta_2 YearsOfEducation_{it} + \mu_{it}\end{aligned} \tag{4}$$

We can see from table 7 that as opposed to the significant results obtained in our Swedish panel data regression, the results from the Danish panel data are not statistically significant. From this, we can infer that Denmark had different labour market outcomes from the influx of refugees. This implies that Denmark is a possible valid counterfactual for a Difference-in-Differences model.

We run a regression of logarithmised average income levels, $\log\left(AverageIncome\right)_{it}$, against the treatment, a dummy variable with value 1 if the municipality is in Swe-

Table 7: Denmark

Dependent variable: log Average Income	
	(1)
	FE regression
log(ImmDensity)	-0.04688
	(0.02843)
log(PopDensity)	0.18969
	(0.19421)
YearsOfEducation	0.01716
	(0.04688)
Observations	784
R-squared	0.98842
State FE	YES
Year FE	YES
State specific Time-Trend	YES
Cluster	YES
Robust standard errors in parentheses	

den, and the post, another dummy variable with 1 if the observation was in post-2014, an interaction term between treatment and post and finally, two controls: population density and education levels. As differences between municipalities act as the source of variation, we use clustered standard errors at the municipality level.

$$\log\left(AverageIncome\right)_{it} = \alpha_t + \beta_1 Sweden_i + \beta_2 Post2014_i + \beta_3 Post2014 \times Sweden_i + \delta_1 \log\left(PopDensity\right)_i + \delta_2 YearsOfEducation_i + \mu_{it} \tag{5}$$

As seen from Table 8, we find a negative effect on Sweden on the average income levels. The magnitude of the impact, at -1.3%, is in line with the estimate which we have obtained in prior models to obtain the impact of immigration on average income levels. It remains significant when clustered standard errors are used and with all other controls in place.

In order to ensure the credibility of our difference-in-difference model, an important assumption is that the trends are parallel. With reference to Figure 3, we see that this assumption is largely satisfied in our comparison of the income growth between Sweden and Denmark. However, if the policy responses also varied over time, where policy was changed in the years following the refugee crisis, this would be a violation of our assumption. Further, labour markets are prone to idiosyncratic economic shocks, which may differ greatly over time and across countries. Hence, this would also make our estimate inconsistent.

The most important limitation to our estimation is that due to the absence of available data, it was not possible to compare marginal income of different ethnic groups. This means that though we may see the negative impact of the differences in immigration policy, we do not know which groups are impacted the most. Hence, it is difficult to deduce any theoretical mechanism in the labour markets from these results.

Whilst we can see that Denmark and Sweden are valid counterfactuals to each other, we require more micro-level data in order to assess the importance of this result. Furthermore, simple DiD estimation may undermine the endogenous policy making of each country. In other words, there are numerous adhoc adaptations of policies to the labour market situations, which would violate parallel trends across the time period. Hence, it is difficult to justify that the estimates are causal.

Table 8: Differnece-in-Differences

Dependent variable: log Average Income	
	(1)
	DiD
Sweden	-0.12829***
	(0.02950)
Post2014	0.13991***
	(0.00608)
Post2014×$Sweden$	-0.01319**
	(0.00610)
log ($PopDensity$)	-0.04406**
	(0.02049)
YearsOfEducation	0.24351***
	(0.02816)
Constant	31.43959***
	(4.33219)
Observations	3,974
R-squared	0.97979
State FE	YES
Year FE	YES
State specific Time-Trend	YES
Cluster	YES
Robust standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

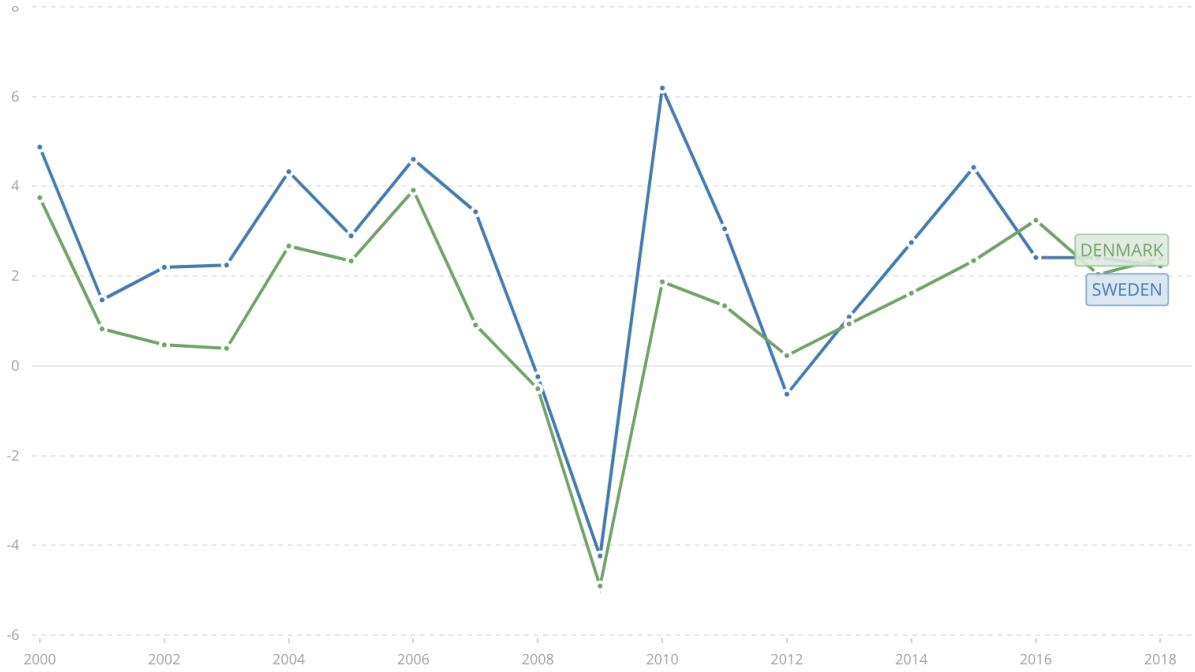


Figure 3: Plot of Economic growth against year, for Sweden and Denmark

5 IV-2SLS

The key limitation behind our DiD and First Differences estimates are that they are prone to reverse causality issues. Since Economic incentives primarily determine where migrants choose to settle, it is important to tackle this problem. Thus, we carry out an instrumental variable regression with 2011 municipal immigration density as an instrument. In a German paper concerning the moving patterns of immigrants (Tanis, 2018), it was found that immigrants are more likely to move to areas where there is already a significant co-ethnic immigrant presence. The purpose of this IV regression is to utilize the change in immigration patterns induced by this and capture any associated changes to average income in the municipality.

Our specifications are:

$$\log\left(ImmDensity\right)_{it} = \alpha_t + \gamma \log\left(InitialImmDen\right)_i + \delta_1 \log\left(PopDensity\right)_{it} + \delta_2 YearsOfEducation_{it} + \mu_{it} \tag{6}$$

$$\log\left(AverageIncome\right)_{it} = \alpha_t + \gamma \log\left(Immdensity\right)_{it} + \delta_1 \log\left(PopDensity\right)_{it} + \delta_2 YearsOfEducation_{it} + \epsilon_{it} \tag{7}$$

	(1)
	log Immigration Density
log(InitialImmDen)	0.928***
	(0.0121)
YearsOfEducation	-0.0732***
	(0.0145)
log (*PopDensity*)	-0.00473
	(0.00448)
Observations	2320

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

Running the above first stage regression (Equation 6) shows that the coefficient is signifi-

cant at 0.93. The large correlation provides evidence for our hypothesis that immigrants move to municipalities with higher pre-existing levels of immigrants. It provides a strong first stage satisfying our relevance condition.

As seen from the IV-2SLS estimation below, the estimate is significant at negative 5%. However, it is crucial that we see the impact on differing geographical origin of the immigrant as well. For Nordics, there is a 3 to 4 percent decrease in average income when immigrant density increases by 1%. For other immigrants, the coefficient is insignificant at -0.3%. For Swedes, the coefficient value is significant at positive 0.8%. Most importantly, an increase of 1 percent in immigration density leads to 13% decrease in the income of recent settlers, who we identified as refugees. The overall impact of immigration density on municipal median income is negative, but the results suggest that the negative effect is captured by immigrants rather than by the Swedes. This is indicative of the theory that immigrants affect other immigrants' income rather than the natives' income.

In accordance with the Swedish law, immigrants able to arrange accommodation on their own are allowed to do so, whereas the rest are allocated quasi-randomly to municipalities across Sweden. The allocation of immigrants is based on the ability and to some degree the willingness of municipalities to accommodate them. This allocation process then raises three main concerns about the "as good as random" assignment of pre-existing levels of immigrants per municipality. Firstly, it is likely that richer immigrants, for example immigrants from Western countries, are more likely to arrange accommodation on their own as opposed to refugees and people from non-Western countries, which creates a selection bias in treatment status of the municipality. In order to address this, we control for education as well as providing the treatment effect on migrants of differing origins, which would address the heterogeneous group of immigrants.

Secondly, municipalities have some say in whether or not to accommodate refugees. Hence it is possible that there is a correlation between municipalities with higher economic growth and the change in immigration density. This is potentially true if larger right wing, anti-immigration voting behaviour developed over time and thus remain uncaptured by the initial levels of immigration density. This would create a bias in our estimates and we try to robustify our results by including year fixed effects in our regression to tackle the annual change independent of our internal instrument. Finally, a study on the movement patterns of immigrants in Sweden (Bevelander 2019), found that immigrants are more likely to settle in big cities like Malmo, Gothenburg and Stockholm, due to improved economic prospects. This could lead to inconsistent results if immi-

Table 9: IV-2SLS

	Dependent variable: log Average Income						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Overall Income	Native Swedish	All immigrants	Scandinavian	EU	Asian	Refugees
log(ImmDensity)	-0.01107***	0.04480***	0.06156***	-0.00489	0.06532***	0.02064	-0.13011**
	(0.00399)	(0.00372)	(0.00924)	(0.00783)	(0.01094)	(0.02336)	(0.05214)
log (*PopDensity*)	0.00000	0.00001***	-0.00001	0.00001	-0.00005***	0.00002	0.00027***
	(0.00000)	(0.00000)	(0.00001)	(0.00001)	(0.00001)	(0.00002)	(0.00004)
YearsOfEducation	0.18151***	0.17788***	0.26170***	0.22813***	0.32859***	0.56167***	0.08041
	(0.00431)	(0.00401)	(0.00996)	(0.00844)	(0.01179)	(0.02519)	(0.05663)
Observations	2,320	2,320	2,320	2,320	2,320	2,320	2,153
R-squared	0.59499	0.70250	0.33595	0.36246	0.38950	0.25931	0.05583
Year FE	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

grants display different "preference" for cities over time. In order to address this issue, the regression includes a control for municipal population density.

To satisfy the validity condition of our instrument, immigration density should affect average income only through immigration density induced by the initial immigration density. We address three possible alternative channels through which pre-existing immigration density can affect the average income. Firstly, it is possible that high levels of initial immigration density could indicate the municipal industry mixture, inducing a shift towards a larger low skill sector, negatively impacting the average income. However, it is difficult to think that past industrial composition independently impacts the future labour market outcomes since this cannot change in the period of 8 years. Secondly, unobservables underlying economic growth trends in municipalities might be correlated with initial levels of immigration density. This would be true if it is not accounted for by the controls and sequential exogeneity.

Finally, the increase in the supply of labour should be exogenously determined and the local labour force does not relocate due to the changed labour market circumstances (Borjas 1987). This is the stable unit treatment value assumption of our regression. That is, immigration does not adversely affect other municipalities. The plausible mechanism in which this may be the case is if the local population, who may be more mobile than the immigrants, relocate to another municipality where they can get the same wage level as their previous municipality. This would evidently bias our results on local income levels towards zero since this would essentially cause the wage level to fall into an equilibrium on the national level as opposed to the local level. However, there was no sign of large scale relocation of the native labour force relative to the size of the immigration. Furthermore, immigration rose in almost all municipalities during this time period hence it is unlikely that the effect of immigration is lost in the internal migration of the local population. Hence, we believe that the displacement effect is unlikely to be taking place in Sweden.

However according to Anderson-Hsiao (1982), the instrument of the first lag may result in weak instruments if it follows a random walk model. In other words, if the coefficient of the lag variable is close to 1, it is likely to cause inconsistency. Our model does not directly use the first lag as an instrument as it displays an average of the lags with 2011 as its base. However, 0.93 is close to violating this assumption. We would need to assume that this process is stationary for the IV-2SLS estimate to be a consistent estimator. It is likely to be satisfied since it is unlikely that immigrants do not discount past information over time when they are selecting future settlement locations. This

would guarantee that our process is not a random walk model and hence allow our estimate to be consistent.

$$\begin{aligned}\log\left(Immdensity\right)_{it} = \alpha_t + \beta_1 \log\left(initialimden\right)_i + \delta_1 \log\left(PopDensity\right)_{it} \\ +\delta_2 YearsOfEducation_{it} + \mu_{it}\end{aligned} \tag{8}$$

Assuming that our process of using an internal instrument is a stationary process (rather than a random walk model), there are a few issues in interpreting these estimates as causal estimates. Firstly, we are building upon the idea that immigrants are likely to settle where co-ethnic groups are located. Since we use the foreign share of population to measure immigration density, we cannot strongly establish causality in the first stage. We assume that the co-ethnic population movement is homogeneous throughout the country. This is a strong assumption to make and would require micro-level data to explore the impact of different immigrant groups. Secondly, since Tanis (2018) finds that the immigration behaviour is first determined most heavily by the economic incentives and their second relocation being more affected by the co-ethnic population, it is difficult to estimate the size of the lag present between the two processes. If the process of relocation from the initial settlement takes longer than 5 to 6 years, our "average" effect from the first stage is likely to have measurement error. Thirdly, our instrument only accounts for those that relocate themselves due to the co-ethnic preference. This perhaps limits our analysis to non-refugees since this was evidently not the case for the refugees that were received in Sweden, where refugees were allocated quasi-randomly according to the availability in each municipality. Due to this quasi-randomness of refugees, our first stage estimation may have a lot of "noise".

Whilst we recognise that the use of internal instruments could induce potential biases in our estimates, the theoretical issues have been addressed through appropriate controls and sufficiently explained in the context of Sweden. Furthermore, the results are largely consistent with our previous methodologies though differing in magnitude. Hence, we think that IV-2SLS estimates provide plausible evidence that migrant labour acts as a complement to native labour, whilst creating competition for pre-existing foreign labour.

6 Inequality and Political shift

We have so far explored how immigration density registers an overall negative impact on wages in Sweden and that the effect is largely concentrated on Asian, especially recently

arrived immigrants. In this section, we would like to explore the heterogeneity of this negative wage impact based on income groups.

In table 10, it is evident that there is a positive correlation between the Gini coefficient and immigration density. This means that as immigration rises, inequality increases. However, the outcome variable of the Gini coefficient does not display the overall picture of who is affected by the divergence of income.

Table 11 displays various income percentile ratios. We note that firstly, P50 (median income) denominated ratios are all negative and decrease as numerator percentile increases. Secondly, P20 denominated ratios are all highly significant and positive. This suggests that much of the increase in the Gini coefficient is the result of an increase in the number of people in the lower income percentiles, where immigrants are mostly situated, thus decreasing income ratio. In fact, the former results show that the upper/middle class gap in Sweden is unaffected, or even positively affected by the influx of immigrants. This mechanism would be consistent with the results from the previous sections if we assume that the native Swedish population in the middle class receive the positive complementary effect of immigrant labour on their income. However, we are limited in our analysis of inequality since the data set we use is aggregated at the municipal level. As such, we are unable to identify the exact interactive effect between ethnic groups and income groups.

To continue the societal analysis, we looked into the political aspect of immigration in Sweden. Sweden is a representative democracy and a constitutional monarchy, where seats in Parliament are assigned proportionally to the number of votes in the election, occurring every 4 years. We focus our analysis on the votes on Sweden Democrats, who are the furthest right leaning party in Sweden's Riksdag. The party became popular among those with anti-immigration sentiments and quickly rose to prominence, with support increasing from 5.7% in 2010 to roughly 24% in 2020 (the latter of which is an average of several opinion polls conducted in 2020).

Before 2010, Swedish politics had been characterized by "the left block": the Left Party, the Social Democrats and the Green Party, in contrast to the "the right block": the Center Party, the Liberal Party, the Moderates and the Christian Democrats. When the Sweden Democrats entered the Riksdag, it was to the dismay of all other parties. Due to the various issues surrounding the Sweden Democrats, in particular, the problematic origins, anti-immigrant rhetoric and multiple scandals of its party members, other political parties denounced and politically ostracized the Sweden Democrats.

Table 10: Inequality

	Dependent Variable: Gini coefficient				
	(1)	(2)	(3)	(4)	(5)
	OLS	FE regression	FE regression	FE regression	FE regression
log (*ImmDensity*)	0.03058***	0.05160***	0.02289***	0.00926	0.00926
	(0.00197)	(0.00299)	(0.00431)	(0.00945)	(0.01273)
log (*PopDensity*)	-0.00620***	0.04088***	0.02076	-0.02747	-0.02747*
	(0.00063)	(0.01273)	(0.01291)	(0.01782)	(0.01643)
YearsOfEducation	0.00244	0.08653***	-0.00633	-0.00436	-0.00436
	(0.00249)	(0.00815)	(0.01566)	(0.02461)	(0.02782)
Observations	2,030	2,030	2,030	2,030	2,030
R-squared	0.12252	0.53011	0.56258	0.71378	0.71378
State FE	NO	YES	YES	YES	YES
Year FE	NO	NO	YES	YES	YES
State-specific Time-Trend	NO	NO	NO	YES	YES
Cluster	NO	NO	NO	NO	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Inequality

	Dependent Variable: Marginal Inequality				
	(1)	(2)	(3)	(4)	(5)
	P80/P50	P90/P50	P95/P50	P80/P20	P50/P20
log (*ImmDensity*)	-0.04223	-0.10461**	-0.13382**	54.45424**	38.36791***
	(0.03187)	(0.04311)	(0.05933)	(24.91202)	(10.57695)
log (*PopDensity*)	-0.01357	0.00614	-0.07887	84.50105	12.51452
	(0.05373)	(0.05886)	(0.08107)	(52.45110)	(8.55628)
YearsOfEducation	-0.10829	-0.14615	-0.20969	136.71878	-26.73655
	(0.09545)	(0.14053)	(0.19375)	(150.21869)	(22.67291)
Observations	2,030	2,030	2,030	2,030	2,022
R-squared	0.71839	0.77274	0.79755	0.79510	0.80935
State FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-specific Time-Trend	YES	YES	YES	YES	YES
Cluster	YES	YES	YES	YES	YES

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

However, restricting the Sweden Democrats' political influence proved difficult due to the parliamentary situation. For more than 50 years, elections tended to end with one block obtaining a majority seat in Parliament. However, during the 2010 and 2014 elections, due to the entry of the Sweden Democrats, neither block gained more than 50% of votes in Parliament, giving Sweden Democrats the balance of power in many votes. In the aftermath of the 2018 election, the rise of the Sweden Democrats led to the breakup of the historic Swedish "right" and "left" blocks and a center government was created, consisting of the Sweden Democrats and the Green Party, with support of the Left Party, The Center Party, and the Liberals. The center right parties supported the center left government, since they refused to be part of a right block government which would require active support of the Sweden Democrats. This clearly indicates that the traditional political parties failed to address the issue of immigration that rose up during this time.

Our analysis suggests that the economic impact of immigration on income is minimal to the Swedes and we continue to investigate the causes underlying the impact at the societal level, assessing the change in Sweden Democrats' voting share per municipality as the immigration density in that region changes. Our analysis suggests that there is a strong correlation between Sweden Democrats' voting share and immigration density, with high immigration being more likely to vote for Sweden Democrats. This is consistent with other empirical data and with the fact that Southern municipalities with high levels of immigration tend towards higher Sweden Democrats' voting share. Since the election cycle is 4 years, we use constant values in each election cycle. We include a two-year lag in the regression, giving voters an appropriate time lag to form opinions around this issue. The regression specification is as follows:

$$\begin{aligned} \text{VoteSD}_{it} = \beta \log\left(ImmDensity\right)_i + \beta_1 \log\left(ImmDensity\right)_{it-1} \\ +\beta_2 \log\left(ImmDensity\right)_{it-2} + \delta X_{it} + \epsilon_{it} \end{aligned} \tag{9}$$

Although fixed effects estimates are more efficient when we assume no serial correlation, from our panel data analysis it is clear that our dataset has time series issues. Hence we think that the using the First Differences estimation would display a more representative behaviour. From the table below, we see insignificant results for the immigration density during the year of voting, but highly significant results from the first and second lag: A one percent increase in immigration density in the previous year raises the vote of Sweden democrats in that region by 2 to 3 percent. This shows that the exposure to

immigrants builds anti-immigration sentiment over time, where it "trickles down" to the voting behaviour.

As seen from our previous results, Swedish citizens on average do not receive a negative income shock. Thus, it would be difficult to say that this voting behaviour shift is motivated by an economic incentive. In other words, the phrase "immigrants are taking our jobs" is likely not the only motive behind the Sweden Democrats' recent success. This is reinforced by the fact that the inequality of the upper income levels has not deteriorated and hence the middle-class community is unlikely to be economically damaged. Rather, we would need to delve deeper into voting behavior to identify the causes shifting voters away and creating the anti-immigrant sentiment, which is an area that needs further analysis.

Table 12: Sweden Democrats votes

	Dependent variable: SD Votes	
	Two ways FE	First differences
	(1)	(2)
$\log(ImmDensity)$	5.366***	-2.386
	(0.531)	(1.969)
$\log(ImmDensity)t-1$	0.018	2.470***
	(0.252)	(0.360)
$\log(ImmDensity)t-2$	0.215	2.609***
	(0.194)	(0.358)
$\log(PopDensity)$	-5.519***	-9.461***
	(1.501)	(3.669)
YearsOfEducation	-9.570***	12.282**
	(1.769)	(5.228)
Observations	2,316	2,026
R^2	0.105	0.048
Adjusted R^2	-0.028	0.045
F Statistic	47.399***	20.256***

Standard errors in brackets
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7 Conclusion

There is much social and economic debate on the impact of immigration. Here, we present evidence that suggests immigrants are not substitutes for local labour but for other immigrants. This implies that the often-used rhetoric, that immigrants are taking local jobs, seems implausible. This is in line with the results found from Altonji, Borjas and Card that saw no effect of foreign share of population on local labour income levels. Hence, our findings contribute to the external validity of the results found in America on the immigrant and the local population.

Our first differences estimates and the IV-2SLS estimates provide consistent results that the impact of immigration density on local income is positive whilst the impact on immigrants, especially for the Asian population who have just come to Sweden, is negative. This supports the theoretical framework of complementary nature of immigrant labour to the local labour market. The magnitude of this impact is difficult to estimate due to the relatively short panel timeline from 2011 to 2018. Collecting from the evidence shown above, the impact of a 1 percentage increase in the immigration density is likely to lead to about 1% increase in Swedish income levels. Whilst the impact on Nordic income and the EU income is not clear, impact on Asian population in Sweden is significantly negative for those who have only been in the country for 1 to 2 years, where the 2SLS estimate predicts a roughly 13% decrease in income.

Analyses based on the shares of foreigners in either a locality or an industry are also subject to the criticism that natives may react to an increased inflow of foreigners by locating elsewhere, thus dissipating the adverse effects of increased migration (Chiswick (1992, 1993)). This will bias the estimated effects of immigration toward zero. Filer (1992) and Card (1991) find conflicting results in the US of this effect. In Sweden, whilst we have no quantitative data on internal migration, qualitative data suggests that this is unlikely to be the case.

However, there is room for further research on who is receiving the changes in income levels. Due to the limitations of the municipal level of the data, we are unable to identify the exact composition of those affected by immigration. For instance, the skill bias in the potential effect, as seen from the results of Zimmerman, would provide further categorisation that would allow us to distinguish the different levels of immigration. Whilst we predict from the inequality data as well as urban/rural analysis that low

skilled service sector workers suffer from the inflow of less educated immigrants, we have no quantitative data to back this hypothesis. Such data would unveil the causal mechanism behind the rise in right wing votes correlated to the increase in immigration density.

Whilst there are various limitations to assuming causality from our results, they provide important implications on the impact of immigration on the labour market. From our findings, it is difficult to pin-point the root of the anti-immigration sentiment prevalent in Europe. However, perhaps the most important evaluation from our paper is that the direct economic impact of income levels are on average rather insignificant on the local population. This reveals that there may be more sophisticated mechanisms behind such political direction. Furthermore, the negative impact on income that is entirely absorbed by the immigrants, especially those who have only recently arrived in the country, shows that concentrating immigrant population into one area could impoverish them further due to the oversupply of labour into certain sectors of the economy.

References

L. Abend, "An Island for 'Unwanted' Migrants Is Denmark's Latest Aggressive Anti-Immigrant Policy" (2019), *https://time.com/5504331/denmark-migrants-lindholm-island/, accessed 26/02/2020.*

M. Alestalo, S.E. Hort, S. Kuhnle, The Nordic model: Conditions, origins, outcomes, lessons (2009), HSoG Working Paper Series.

J. Altonji, D. Card, "The Effects Of Immigration on the Labour Market Outcomes of Natives" (1989), *NBER Working Paper Series.*

S. Andersen, J. Dølvik, C. Ibsen, "Nordic labour market models in open markets" (2014), European Trade Union Institute.

P. Bevelander, "The immigration and integration experience : the case of Sweden." (2010), (Uma A. Segal, Doreen Elliott, and Nazneen S. Mayadas), *Immigration Worldwide, Chapter 19 Sweden, p. 286*, Oxford University Press.

P. Bevelander, C. Lundh, "Employment Integration of Refugees: The Influence of Local Factors on Refugee Job Opportunities in Sweden" (2007), IZA Discussion Paper No. 2551.

G. Borjas, "Immigrants, Minorities, and Labor Market Competition" (1987), *ILR Review, Vol. 40, No. 3 (Apr., 1987), pp. 382-392.*

J. De New , K. Zimmermann, (1994). Native wage impacts of foreign labor: a random effects panel analysis. *Journal of population economics, 7(2), 177-192.*

C. Green-Pedersen, J. Krogstrup (2008) "Immigration as a political issue in Denmark and Sweden" *European Journal of Political Research* Volume *47, p.610-634.*

J. Grossman, The Substitutability of Natives and Immigrants in Production (1982), *The Review of Economics and Statistics, Vol. 64, No. 4 (Nov., 1982)*

V. Hernes, J. Arendt, P. Joona, K. Tronstad, "Nordic integration and settlement policies for refugees" (2019), Nordic Council of Ministers.

V. Hernes, J. Arendt, P. Joona, K. Tronstad, "Nordic integration and settlement policies for refugees" (2019), Nordic Council of Ministers.

K. Jylhä, J. Rydgren, P. Strimling, "Sweden Democrat voters. Who are they, where do they come from, and where are they headed?" (2019), Institute for Futures Studies 2019/1, Stockholm 2019

A. Kelly, "FACT CHECK: Have Immigrants Lowered Wages For Blue-Collar American Workers?" (2017), *https://www.npr.org/2017/08/04/541321716/fact-check-have-low-skilled-immigrants-taken-american-jobs*, *accessed 2/03/2020*

S. Lennart. En modern svensk ekonomisk historia: Tillväxt och omvandling under två sekel (A Modern Swedish Economic History: Growth and Transformation in Two Centuries). Stockholm: SNS (2000).

E. Lewis, "Immigartion, Skill Mix, and Capital Skill Complimentarity" (2011), *The Quarterly Journal of Economics*, Vol. 126, No. 2 (May 2011), pp. 1029-1069.

M. Manacorda, A. Manning, J. Wadsworth, "The Impact of Immigration on the Structure of Wages: THeory and Evidence from Britain" (2010), *Journal of the European Economic Association*

Ministry of Justice, "Sweden's migration and asylum policy" (2019), *https://www.government.se/4adac4/contentassets/183ca2f36f1c49f3b7d1b5724a5753ce/swedens-migration-and-asylum-policy–fact-sheet-2019.pdf*, *Eaccessed 25/02/2020.*

A. Otto, M. Steinhardt, "Immigration and election outcomes — Evidence from city districts in Hamburg" (2014), *Regional Science and Urban Economics Volume* 45, p.67-79.

K. Patel F. Vella, "Immigrant Networks and Their Implications for Occupational Choice and Wages" (2007), IZA Discussion Paper No. 3217.

J. Pischke, J. Velling, "Employment Effects of Immigration to Germany, An Analysis Based on Local Labour Markets" (1997), *The Review of Economics and Statistics*, Vol. 79, No. 4 (Nov., 1997), pp. 594-604.

M. Ruhs, C. Vargas-Silva, "Briefing: The Labour Market Effects of Immigration" (2020), *The Migration Observatory*

C. Schmidt, K. Zimmermann, (1992). Migration pressure in Germany: past and future. *In Migration and economic development* (pp. 201-230). Springer, Berlin, Heidelberg.

H. Thomassen, "The integration of refugees in Denmark" (2019), Policy Department for Economic, Scientific and Quality of Life Policies Directorate-General for Internal Policies, Study requested by the EMPL committee.

Investigating Monetary and Macroeconomic Policy in the Bretton Woods Era: Evidence From France

Utkarsh Aggarwal[1], Francesco Casalena[1], Patrick Leitloff[1], Ciaran Marshall[1], and Tomáš Tapak[1]

[1]The London School of Economics and Political Science, London, UK

Abstract

We attempt to develop a new methodology of inquiry in order to assess the effects of monetary policy during the Bretton Woods Era. Building on the data and the findings of Monnet (2014) on monetary policy in France in the period 1946-1972, we adopt the Local Projections Model (LPM) developed by Jorda (2005) in order to obtain multiple impulse-response functions of endogenous real and monetary macroeconomic variables to domestic and foreign monetary policy shocks. Our results are broadly consistent with a standard open-economy Mundell-Fleming model and confirm the consensus view on monetary policy implementation in that period.

JEL Codes: E52, E58, N14

1 Introduction

1.1 Inquiry

During the time-honoured Bretton Woods Era, many countries (e.g. France, Germany, UK and Italy) started experimenting with a new method of implementing monetary policy. Central Banks started leveraging direct controls on credit aggregates and the overall money-market degree of liquidity as primary tools in setting out the monetary policy stance. At the same time, the official discount rate was sidelined to a secondary role. The consensus view at the time was that Central Bank interest rates had somehow lost their meaning, as they were not particularly efficient in regulating the flow of credit to the real economy (Monnet, 2014). This exegesis was embedded in the wider political framework of the time, which took the name of Keynesian Consensus. The majority of policymakers agreed on the fact that demand-side policies were crucial in order to stir the economy towards full-employment. This goal was to be achieved by fiscal policy, while monetary policy would assume a more restrictive countercyclical role. Exchange rates were fixed and international capital flows were heavily regulated, therefore allowing for a spread between domestic and foreign interest rates. Simultaneously, restrictive credit controls employed to dampen domestic demand allowed for a disconnect between the Central Bank's rate and the monetary stance. Therefore, the domestic interest rate structure was allowed to vary freely, easing the burden of Government and private debt during the post-war reconstruction.

While the literature at the end of that era almost immediately (e.g. Hodgman, 1973) started reporting and studying monetary policy implementation during this period, only very recently have systematic quantitative analyses been carried out. Most notably Monnet (2014) and Aikman et al. (2016) use VAR models to investigate the quantitative effects of monetary policy on output and other endogenous variables. While the former adopts a pure narrative approach in the fashion of Romer and Romer (1989) for identifying episodes of monetary restriction, the latter attempts to build a quantitative index representing the monetary stance, which is then used complementarily to the discount rate in estimating the dynamic multipliers associated with the impulse-response functions. We consider the narrative approach superior. In fact, building a dummy variable taking the value one in case of a restrictive episode is the most effective way of identifying the contractionary policy actions carried out through credit controls. This is due to the multiplicity of instruments used over time, as well as the relative binding capacity of each instrument, depending on the state of the economy.

We build on the identification made by Monnet (2014) as a starting point for our analysis. The main contribution here is to develop a new methodology for investigating the effects of monetary policy on real and monetary variables during the Bretton Woods Era. In doing so, we estimate impulse-response functions (IRFs) of narrative shocks by employing the Local Projections method set forth by Jorda (2005). This allows us to adopt a richer and more flexible specification, which accomplishes the task of generalising the inference in a general equilibrium Mundell-Fleming framework. In fact, without the nuisance of the imposition of a priori theory-based constraints, we can indeed experiment by including more variables contributing to the impulse-response functions, as well as test for an asymmetric state-dependent effect of quantitative restrictions on output and prices. All in all, our results are able to identify short and long-run relations with real and monetary endogenous variables that are broadly consistent with the consensus view on monetary and macroeconomic policy implementation during the Bretton Woods Era. Hence, our findings significantly strengthen the current mainstream view, while also shedding light on some peculiarities of macroeconomic policy implementation in France.

The remainder of this paper is divided in the following way: after a brief evaluation of monetary policy during our period of interest, the first section briefly outlines the issue we are investigating and its importance in relation to common topics in open-economy macroeconomics . Specific emphasis is given to a description of the international monetary system in order to stress the ties between monetary policy implementation during the Bretton Woods Era and the so-called "trilemma". The second section describes our econometric model of inquiry and its underlying methodology. The third section displays the data and our findings. The fourth section concludes.

1.2 The Bretton Woods Era: a historical appraisal

Towards the end of WWII, there was broad consensus amongst policymakers that a new monetary system would be necessary in the post-war period. It was generally agreed that past monetary systems had proved suboptimal. Theoretical discourse on the framework of such a system was heavily influenced by two very contrasting past experiences (Bordo, 1993). These were the pre-WWI gold standard and the interwar period of macroeconomic disorder (Obstfeld et al, 2004). According to Eichengreen and Flandreau (1997),

the defining characteristic of the era of the classical gold standard (ca.1870-1910) was the stable exchange rate between most significant currencies, as the money supply of a country was directly linked to its gold reserves. While causing great certainty regarding exchange rates and intensive foreign lending, the international gold standard implied a significant interconnectedness of national economies: Its binding nature prevented any kind of domestic monetary policy to mitigate adverse effects of supply and demand shocks, given that each country's external balance had to be strictly maintained (Bordo and Redish, 2003). The reestablishment and subsequent incremental abandonment of the gold standard during the interwar period enabled governments to pursue an independent monetary policy in pursuit of domestic objectives. This, however, did not result in the desired stable economic conditions and full employment. To improve their external balance, many countries devalued their currencies, effectively engaging in beggar-thy-neighbour monetary policy. Furthermore, the interwar period was a time of amplified currency speculation.

As stated by Gorski (1945), the groundwork for the Bretton Woods System was the Atlantic Charter of 1941. Its resolutions in clauses 4 and 5 called for a lowering of international trade barriers and increased global economic cooperation and advancement of social welfare. The Bretton Woods Agreements are comprised of three separate treaties, including the agreement of the International Monetary Fund (IMF agreement) and the agreement of the International Bank of Reconstruction and Development (IBRD agreement). Together, these agreements were thought to ensure a stable and less speculative monetary system. Exchange rates in the Bretton Woods system were fixed, but adjustable. The member states committed to maintain their exchange rates within 1% of parity. IMF approval was necessary for significant exchange rate changes (rule of fundamental disequilibrium). In addition, the IMF was meant to oversee the exchange rates as an independent body. The primary goal was to accompany the process of reestablishing currency convertibility and to help member countries overcome temporary liquidity problems. Its general scarcity of currency clause ensured that the IMF had the option to penalise countries which adopted destabilising monetary policy stances. The main role of the IBRD was to provide the loans necessary for a quick rebuilding of the economic system (Gorski, 1945).

The envisioned quick recovery towards current account convertibility took longer than anticipated. It was accompanied by persistent deficits against the dollar by most West-

ern European economies and several balance-of-payment crises, which led to temporary additional exchange and capital controls. After reinstating current account convertibility in 1958, international trade (especially between the Bretton Woods member states) rose dramatically, and consequently many of the controls in place could no longer be enforced effectively (Bordo, 2017). According to Bordo the Bretton Woods System of the 1960s had evolved into a "gold dollar standard", crucially dependent on US monetary policy. This asymmetry resulted in conditions similar to those of the interwar period. One crucial implication of this asymmetry was the fact that the US was not required to adjust its balance of payments and could run a deficit while implementing expansionary fiscal policy, thereby causing inflation in other countries. This prompted confidence considerations, as at some point the participating countries could all at once demand to exchange their dollar reserves into gold. In 1964 the dollar amounts held by foreign central banks increased US gold reserves for the first time. This development also evoked concerns about the ability of the US to create sufficient liquidity, since overcoming the US deficit would mean less provision of liquidity for the other countries of the Bretton Woods system (Eichengreen, 2008).

To overcome this issue, the IMF issued "Special Drawing Rights", designed as an international reserve to complement the dollar. Various measures aimed at reducing the risk of a "gold run" were implemented. While they were able to postpone the collapse of the Bretton Woods System, the existing imbalances were amplified by US inflation. After France and Britain announced that they would convert dollars into gold in the near future, the US suspended gold convertibility in 1971. Ultimately, in 1973, the adjustable peg system was abandoned, thereby concluding the Bretton Woods experience (Bordo, 2017).

2 The Issue

2.1 Monetary policy with quantitative controls

As a matter of fact, the desire of governments to regulate interest rates and plan the economy during reconstruction arguably ended up creating a situation where there was little transmission between interest rates and credit quantities. In such an environment, manipulating interest rates to conduct monetary policy could lead to suboptimal outcomes. Hence, monetary policy during that period was conducted using credit controls, meaning that, rather than adjusting interest rates, the central bank directly manipu-

lated the quantity of credit (Monnet, 2014). Nevertheless, credit controls were used for a much wider agenda than simply smoothing out macroeconomic fluctuations. In his review of the implementation of credit controls in Western Europe, Hodgman (1973) lists the following reasons:

1. To finance government debt at lower interest rates than market preferences would permit
2. To check the flow of credit to the private sector without raising domestic interest rates, thus attracting foreign funds through the balance of payments
3. To influence the allocation of real resources towards priority uses
4. To block the channels of financial intermediation and therefore to assist a restrictive general monetary policy by impeding a rise in velocity
5. To strengthen popular acceptance of price-wage controls by holding down interest income to credit-granting institutions and private investors

In some countries, especially the United Kingdom, policymakers viewed interest rates as ineffective for the conduct of macroeconomic control (e.g. in the Radcliffe Report, 1959). It was thought that large movements in interest rates would be required to have an impact on output (Aikman et. al., 2016). Nowadays the prevailing view has shifted towards a more balanced judgment: recent assessment of the use of credit controls instead of interest rates suggests that, if the money market is dysfunctional, as was the case of France, then credit rationing may be superior to credit allocation through interest rates (Monnet, 2014).

The main tools that central banks used to employ to directly control credit were **rediscount ceilings**, **liquidity ratios**, **hire-purchase controls** and **special deposits**. Rediscount ceilings were caps on the total value of loans discounted by the central bank for each financial intermediary (Monnet, 2014). These caps were implemented as a percentage of total deposits for each institution, implemented in an entirely discretionary manner according to the bank's asset quality and individual risk exposure.

Penalty rates could be placed on discounts in excess of the ceilings. Strictly speaking, they constituted a form of microprudential regulation, but used for macro objectives. Credit Ceilings consisted in direct limits on the expansion of credit. Their nature was intrinsically temporary and they were used to mitigate the risk of asset bubbles forming

(e.g. the Banque de France regularly set rediscount quotas for agricultural products to prevent a large surge in the prices of agricultural goods). However, this was only an efficient mechanism for stabilising inflation when banks held a substantial amount of central bank debt. For this reason, direct limits on credit expansion were also deployed since the 1950s by many European monetary policymakers (Monnet, 2014). Credit expansion limits were also used for credit allocation as a form of industrial policy, and also for macroprudential purposes. Such usage of selective credit policy was particularly common in postwar Europe and East Asia in order to solve coordination failures within the loan market (Johnson 1974, Rodrik 1994). In addition, credit controls were deployed as a means to maintain the banking sector in an equilibrium that does not contradict wider monetary and fiscal policy stances. For instance, in order to avoid speculation on public debt or the national currency, credit controls were implemented as a complement to international capital controls: in the standard IS-LM model, credit controls reduce the interest rate on government debt (Monnet, 2014). Other motivations behind their employment include the possibility of reducing the interest rate on loans below the market rate and increased bank access to the central bank discount window during periods of monetary contraction. For all these reasons, monetary policy becomes more powerful in these circumstances (Hodgman, 1973).

Liquidity ratios were used by the central bank when lending to other financial intermediaries in order to determine a debtor's ability to pay current debt obligations without raising external capital. They were used for countercyclical stabilisation of the money supply, and hence output and inflation. Often, they tended to be considered a vital complement to credit controls for preventing banks substituting their assets for those with greater liquidity. Reserve Requirements, instead, had the scope to set the minimum amount of reserves required to be held by each financial institution, as a percentage of total deposits held by that bank. Their usage was mostly aimed at microprudential and macroprudential regulation and countercyclical monetary policy: in theory, a fall in the reserve requirements stimulates credit expansion. Hire-purchase controls were instead a type of asset financing method that allowed firms or individuals to possess and control an asset during an agreed term, whilst paying instalments covering the depreciation of the asset and interest to cover the capital cost. Hire-purchase controls were used to set limits and regulate the degree to which financial institutions could lend assets involving such purchases. Finally, special deposits were an instrument of monetary policy involving the placement of a specified proportion of the banking sector's liquid assets with the central bank as a means of controlling the aggregate money supply. Special deposits were

excluded from liquidity ratios and reserve requirements. Hence, if monetary authorities sought to reduce the money supply, they could call for a special deposit to lower the liquidity base of the banking sector. This reduced bank lending and credit expansion, hence leading to a fall in the money supply.

noindent The impact of credit controls on output and inflation chiefly depends on the channels through which credit supply shocks are transmitted to the economy. These channels can be broadly categorized as demand and supply channels. The former category includes, most importantly, the **bank lending** channel, the **household demand** channel and **business investment**. The latter mainly consists of the **working capital** channel. By restricting the supply of credit, quantity-based monetary policy tools reduce demand by households, as households find it more difficult to borrow for consumption and residential investment. Lower household demand results in decreased output and prices (Barth and Ramey, 2002). Business investment reacts negatively to credit supply reduction. As raising external finance is difficult, firms cancel or postpone their investments. This reduces demand, pushing down prices and output (Mishkin, 1995; Bernanke and Gertler, 1995; Ciccarelli et. al., 2010). Via the demand channel, restrictive policy leads to lower output and prices. Via the supply channel, restrictive policy reduces output, but unlike the case of the demand channel, the effect on prices can be positive, depending on whether the tradable or non-tradable goods sector is affected more strongly. The overall effect of restrictive policy should therefore be negative in terms of output. However, in terms of prices, the overall effect depends on which one of the channels dominates and which sector is the most affected. In the case of the UK, Aikman et. al. (2016) suggest that the channels had been relatively equivalent, while in the case of France, Monnet (2014) found that the restrictive policy had a negative effect on both output and inflation, thus possibly suggesting dominance of the demand channel.

2.2 Capital controls and the "trilemma"

The Mundell-Fleming trilemma, also known as the impossible trinity, postulates that a country needs to select at most two among the threefold choice of free capital mobility, fixed exchange rate management and monetary autonomy. The challenge for international monetary policymakers comes in choosing which of these choices are best to pursue and how they can be managed, since only two of the three options can be exercised simultaneously. The trilemma is widely used to discuss capital controls and monetary

policy autonomy under the Bretton Woods system and it has long been recognized as a benchmark theoretical model for positive and normative policy analysis. In practice, its importance rests on the possibility of analysing different stylised facts that lie in between the different options of the choice: often the divergence between fixed and floating exchange rates is not dichotomous and capital controls are never fully binding (Obstfeld and Taylor 2004; Bordo and James 2015).

The Mundell-Fleming trilemma often becomes a useful point of view for the analysis of credit controls. In this respect, Monnet (2014) highlights two facts that are broadly at odds with the classical assumptions underlying the trilemma argument during the Bretton woods Era. First of all, he notes that conflicts between internal and external objectives were uncommon. Second, the use of quantitative credit controls allowed central banks to disconnect their interest rate from their monetary policy stance. Indeed, the use of monetary policy instruments also needs to conform to the trilemma. Therefore, the question that arises is if the capital controls offer an attractive policy option: often, they can supply a certain degree of policy autonomy. Nevertheless, middle-ground policies should be implemented very cautiously, as soft interest rate pegs allow for a greater scope over autonomy of monetary policy. Finally, although broad capital controls allow for monetary policy autonomy, they introduce costly distortions in the economy (Klein and Shambaugh, 2015).

Therefore, in the Bretton Woods framework credit controls attained a twofold objective. Internally, quantitative controls were already the main instruments of the central bank. Externally, the discount rate could be, for example, adjusted in line in France during the Bretton Woods regime. Since the channel of interest rates was heavily constrained and almost shut down, the interpretation of capital controls is very different from the one given by the Mundell–Fleming model. In fact, such controls were not primarily used to prevent investors from benefiting from the differential between home and foreign interest rates. They were instead used to make credit ceilings and liquidity requirements more effective.

An alternative, possibly complementary, view to the aforementioned hypothesis is that capital controls were a necessary piece of highly regulated and segmented financial systems, where credit was supposed to be directed by the state to priority sectors and where the central bank relied primarily on direct credit controls for monetary policy. The dis-

appearance of capital controls was a correlation of the decrease of state intervention in domestic credit markets and the increasing role of interest rates and open market operations as monetary policy tools. One possible way to effectively make use of the instruments available to the central bank is to use interest rates to achieve the external equilibrium and credit controls to manage inflation. The management of aggregate demand and the way in which monetary policy is applied in prominent economies has important consequences for economic activities globally.

3 The model

3.1 Methodology

Our aim is to propose a new generalised methodology of inquiry into the effects of monetary policy as it was usually implemented during the Bretton Woods Era, namely through direct controls on the outstanding stocks of credit and by directly altering the balance-sheet requirements of commercial banks. The idea is to reconstruct a generalised aggregate demand-aggregate supply model implied by the data. Indeed, while the quantitative SVAR (Structural Vector Autoregression) approach adopted by Monnet (2014) more closely resembles more a partial equilibrium exercise, we attempt to develop our framework in a general-equilibrium perspective. Our ability to include numerous controls for the behaviour of the public and the private sectors allows us to include in the model all the factors that influence (or are influenced by) monetary policy, and thus are feeding back into the impulse response function. Therefore, our methodology may also prove helpful for investigating the effects of monetary policy in other countries during the same time period.

In order to achieve our goal, we adopt the generalised local projections model, as set forth by Jorda (2005). It consists in the simple OLS regression:

$$y_t = \alpha_{t+h} + \beta_{t+h}\varepsilon_t + \gamma_t' x_t + u_{t+h}$$

where y_t is the dependent variable, ε_t is the monetary shock and x is a vector of controls. The equation is estimated with the endogenous variable lagged up to h periods ahead, with $h \in \{1, 2, ..., H\}$. The estimated coefficients β_{t+h} represent the dynamic multipliers associated to the monetary shock. This specification retains numerous advantages. First

and foremost, the researcher does not need to impose any *a priori* theoretical structure on the model as it is sufficient to correctly identify the shocks. The vector of controls in the regressions includes lagged endogenous variables and any other variable that could be dynamically affected by the shock and could feedback to dependent variables.In other words, all controls are accounted for by information related to the true impulse responses, but no theoretical structure is imposed on the specification.

Another advantage of this Local Projections Model (LPM) is that it can account for state-dependence in the impulse response functions. There are many reasons to believe *a priori* that monetary shocks may have differing effects conditional on the state of the economy, i.e. expansion and recession. For this purpose, it is possible to adopt the smooth transition local projections (STLP) model, by combining the contribution of Jorda (2005) with the smooth transition model by Granger and Terasvirta (1993). Formally:

$$y_t = F(z_t)(\alpha^e_{t+h} + \beta^e_{t+h}\varepsilon_{t+h} + \gamma'_t x_{t+h}) + (1 - F(z_t))(\alpha^r_{t+h} + \beta^r_{t+h}\varepsilon_{t+h} + \gamma'_t x_{t+h}) + u_{t+h}$$

Variables are defined below

The coefficients are allowed to vary depending on the state of the business cycle: β^e and β^r denote the response in expansions and recessions, respectively. The state variable F indicates the probability that the economy is undergoing an expansion and, therefore, it is bounded between 0 and 1. It is defined as the cumulative density function of the Logistic random variable:

$$F(z_t) := (\frac{exp(\theta z_t)}{1 + exp(\theta z_t)})$$

The variable z is a lagging-behind moving average of quarterly growth, minus a parameter equal to the portion spent in recessions (we set it to 20%, following the trend in the literature) and divided by its standard deviation. Finally, the parameter θ indicates the speed by which the economy swings from one state to the other. We follow the consensus by calibrating it as equal to 3.

For VARs impulse response functions are calculated with respect to a baseline scenario of the economy. As long as the process by which the economy switches from expansions to recessions is not explicitly modelled, state-dependent dynamic multipliers are likely to be biased. In fact, a shock might well make the state of the economy change, and if a simple VAR is not able to capture this switch, then it will return distorted impulse response functions. The STLP model overcomes this issue. For all these reasons, local projection allows the researcher to use a far more flexible specification. In contrast to standard VARs, we can test for state-dependence and include more variables in order to account for many more factors, namely the term-structure of interest rates and the relevant stocks of debt and money, which are often excluded from the usual VAR specifications due to parsimony.

As a matter of fact, this is indeed the goal of our analysis: we aim to establish a new, standardised, theory-based methodology of inquiry for monetary policy during the Bretton Woods Era that thoroughly stresses the ties between free capital mobility, fixed exchange rates, interest-rate policies and direct credit aggregates targeting. The LPM grants us the possibility of simultaneously studying the interactions between monetary policy stance, short and long-run interest rates, output and prices, the stocks of money and credit to the government and to the private nonfinancial sector, the foreign exchange rate and interest rate and the trade balance. We are able to study the interactions between these endogenous variables, also allowing for nonlinearities, as well as to gain an understanding of how policymakers used to behave in response to endogenous developments in the economy and what tools they decided to employ to achieve specific targets.

For the sake of illustration we chose France as a case study, since narrative restrictive monetary shocks have already been identified by Monnet (2014). Indeed, since many different tools, ranging from credit ceilings to liquidity controls, were used at the time, and it is not clear how binding they were, the only way to gauge the effectiveness of the policy stance is to build a dummy variable taking the value 1 whenever a restrictive episode occurred. Hence, we are not able to associate a precise impulse response function to each variable, but we still can understand whether monetary policy had some effects or not. Monnet (2014) employs a Cholesky VAR in his analysis, including up to four variables in the system. This procedure allows him to capture the effects of monetary shocks on selected variables precisely, but it falls short of analysing macroeconomic policy in a general-equilibrium open-economy AD/AS framework. For this reason,

starting from his contributions, we develop a generalised methodology that, given some narratively-identified monetary shocks, can effectively uncover the differing nuances of macroeconomic policy. Future research will be able to improve the analysis of monetary policy as it was implemented in other countries, such as the United Kingdom (cf. Aikman et al, 2016), Germany or Italy, during the Bretton Woods Era.

3.2 Econometric specification

For our purposes we estimate three different models. The first calculates the Impulse-Response Functions (IRFs) of prices, output, money and credit stocks, interest rates, the trade balance and the exchange rate in response to a narrative monetary shock. The second model analyses possible asymmetric IRFs of prices and output conditional on the state of the business cycle. The third, finally, seeks to understand how the economy reacted to changes in the other variables of interest, particularly the domestic policy rate, the foreign interest rate (i.e. the Federal Funds rate, given the natural dominance of the US economy in the Bretton Woods framework) and the exchange rate.

impulse response functions are robust to the number of lags chosen of the control variables, as well as to the inclusion or omission of irrelevant variables. We have chosen to include four lags of each variable in the baseline specification of each equation. It is assumed that four lags are sufficient to absorb all the relevant information that might feed back into the dependent variable. This assumption is fairly arbitrary; nevertheless including more lags does not alter significantly the IRFs obtained. Every equation includes the current and lagged values of every variable as regressors. Omitting variables that might be deemed "irrelevant," i.e. theoretically not directly strongly related to one another. For example long-run interest rates and prices does not change our IRFs in an appreciable manner. Therefore, we do not have problems of overfitting.

As far as the diagnostics are concerned, we are not estimating the equations using the seemingly unrelated regressions estimator (SUR), which is in this case the most efficient one. Similarly, we are not using Driscoll-Kraay corrected standard errors, which would account for correlation across equations. As a result, the standard errors of our IRFs are not minimised and possibly biased. This has been a deliberate choice, made for the sake of easing the computation, which, however, does not affect the nature of our findings. Even by assuming a wider uncertainty about the dynamic multipliers, the point estimates remain unbiased.

4 Data and Results

4.1 Data

Our dataset spans the years 1947 to 1972 inclusive. All variables are reported monthly, except for the trade balance in proportion of GDP reported quarterly. The data we employed have been obtained from heterogeneous sources. Specifically, the identification of restrictive monetary shocks following a narrative approach 'a la Romer and Romer (1989) is identical to the one performed by Monnet (2014) based on minutes from the Banque de France. Indexes of consumption, prices, and production of total industry have been retrieved from the yearly reports of the National Credit Council. The source for the seasonally-adjusted series for the M2 money stock and the stock of credit to the private non-financial and the public sectors is Patat and Lutfalla (1990, while the source of all the financial variables, the social discount, money market and long-run interest rates, as well as the Franc-US Dollar Exchange rate and the trade balance as a ratio to GDP is the database: "Global Financial Data" http://www.globalfinancialdata.com/.

4.2 Results

4.2.1 Model 1: Narrative restrictive shocks

As just stated, our first specification simply runs parallel to the VAR regressions implemented by Monnet (2014). The only difference is the greater number of control variables and lags, which allows us to account for more information and does not require a priori reasoning based on economic theory. Unsurprisingly, the effects we find on output and prices are very similar to those in the aforementioned study. On average, a restrictive monetary shock induces a 2.5% fall in the price level and a circa 5% fall in industrial production after one year. It is interesting to notice how these proportions are roughly similar to those implied by the Taylor Principle. In other words, a monetary shock has on average twice the effect on output than it has on prices.

The impact on the M2 aggregate is fully consistent with the correspondent decrease in nominal and real output. Broadly speaking, this entails money velocity (and thus demand for money) remaining approximately stable in recessions.

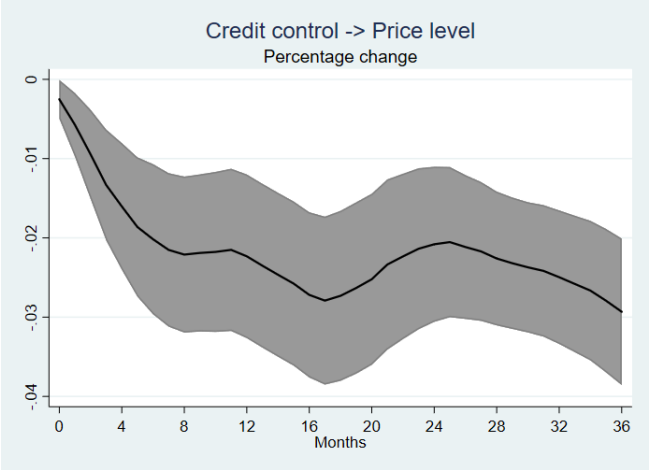


Figure 1: The impact of a restrictive credit control shock on the aggregate price level

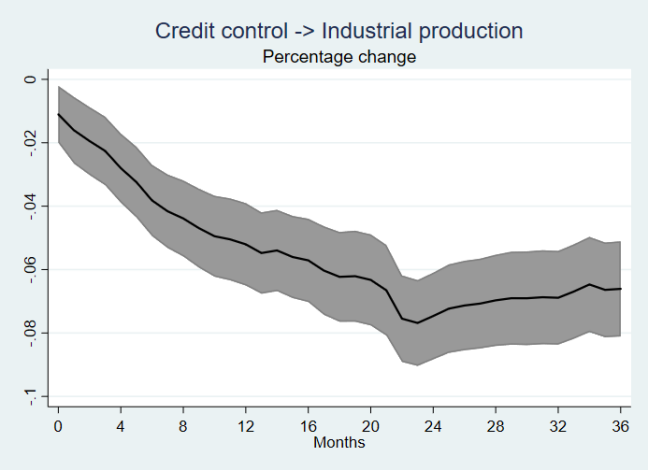


Figure 2: The impact of a restrictive credit control shock on the industrial production index including construction

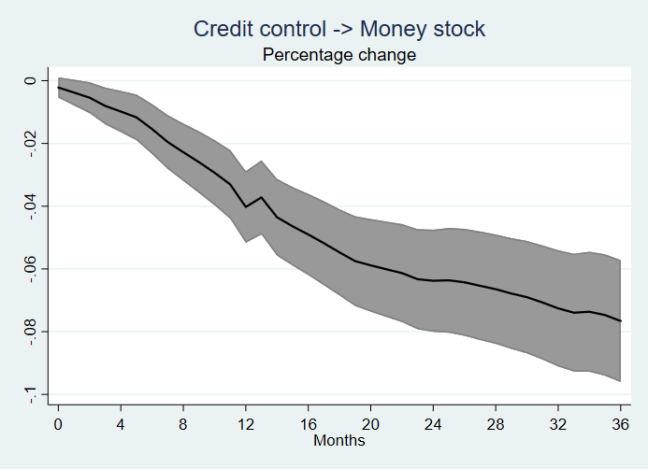


Figure 3: The impact of a restrictive credit control shock on the M2 monetary aggregate

The effect of a quantitative monetary restriction on the total outstanding stocks of credit to the private and public sectors entails interestingly different time paths. For what concerns the former, it attains its maximum trough of slightly less than 10% after one year and half following the shock. Afterwards, it slowly starts recovering. There are many possible interpretations of this phenomenon. We could assume that frictions in the bank lending channel and credit rationing tend to worsen the downswing. In addition, we could also consider that two years after the monetary restrictions, firms start again to demand some credit for covering short-run liabilities or because they plan to investment

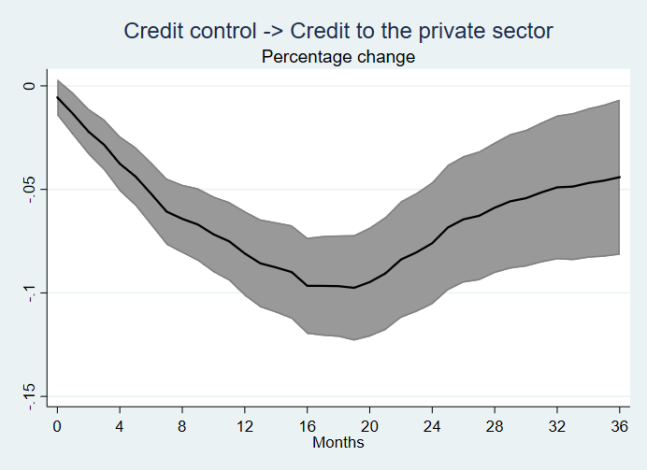


Figure 4: The impact of a restrictive credit control shock on the total outstanding stock of credit to the private nonfinancial sector

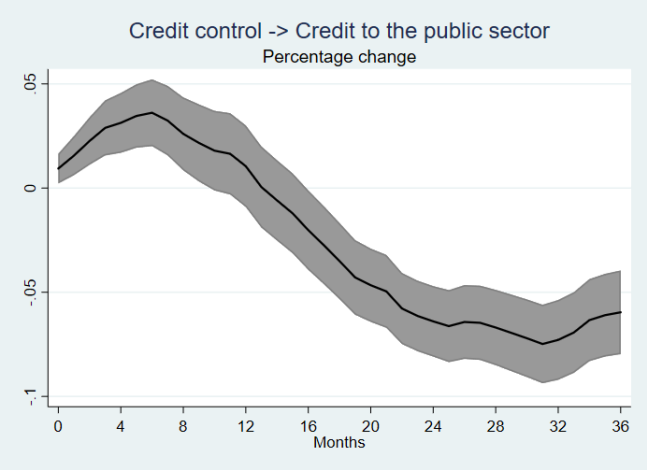


Figure 5: The impact of a restrictive credit control shock on the total outstanding stock of credit to the public sector

not immediately but in the foreseeable future.

As far as credit to the general government is concerned, we should first of all stress the fact that the initial surge (of about 3% after 5 months from the restriction) remains significant and robust to the number of lags included in the specification, as well as to the omission of any variable. It is reasonable to think that, immediately after credit controls enter into force, banks might prefer to shift towards lending to the government or buying short-term bills. Afterwards, credit to the public sector may diminish due to the portfolio preferences of the banking sector.

The initial surge in credit to the government is directly matched by an initial moderate decrease in both short and long-run public bond yields. Although this effect is not significant, it remains consistent with the increase in credit. The movement of short and long-run rates is consistent with standard economic theory: three-months yields initially surge due to the tightening of the liquidity level on the money market, while 10-year yields remain less affected.

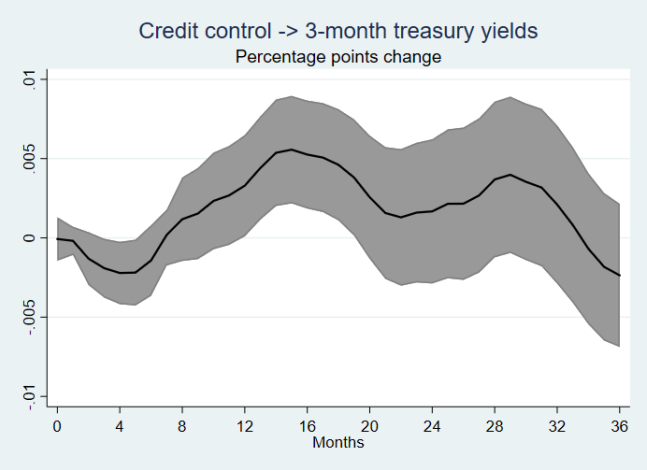


Figure 6: The impact of a restrictive credit control shock on French three-month treasury bills yields

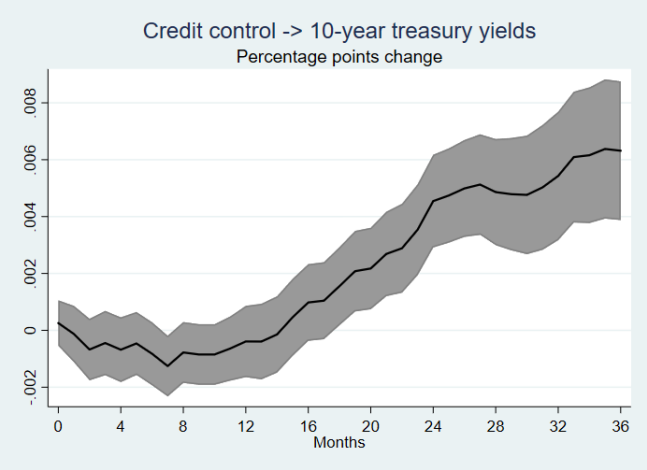


Figure 7: The impact of a restrictive credit control shock on French ten-year treasury bonds yields

Overall, this corresponds to a flattening of the yield curve, as investors prefer to shift to long-run securities for hedging against uncertainty in a recession. Two years after the shock, this trend is reversed, as the economy starts recovering and a steepening of the yield curve occurs. Finally, we can see that, as expected, a credit control implementation leads to an average improvement of the trade balance to GDP by 3% after one year and half. Taking the exports as exogenous, the reduction in income and hence imports entails an improvement of the balance-of-payments. Symmetrically, the increase in foreign currency reserves induced by the balance of payments surplus brings forth downward pressures on the exchange rate, i.e. market forces push for the exchange rate to appreciate. Notice that the FRF/USD exchange rate is price quoted, thus a decrease entails an appreciation. Clearly, in nearly the entire sample the exchange rate was fixed. Therefore, policymakers had to to take into account that a restrictive shock had to bring forth revaluation pressures, rather than a simple exchange rate appreciation.

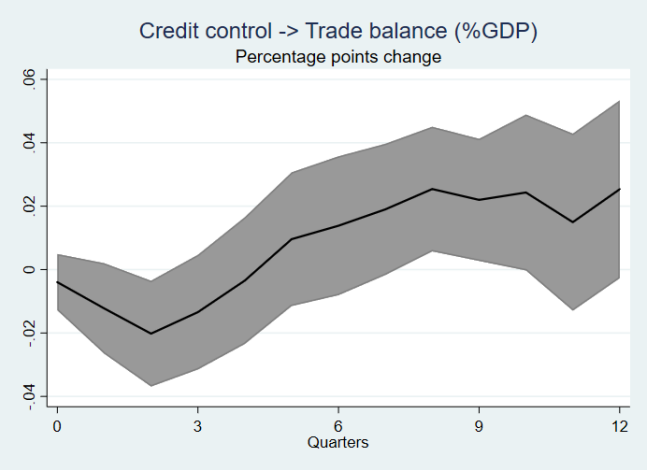


Figure 8: The impact of a restrictive credit control shock on the trade balance (net exports) in percentage of GDP

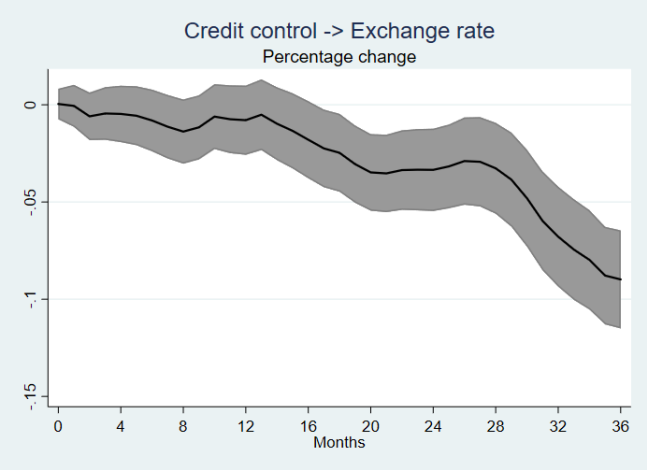


Figure 9: The impact of a restrictive credit control shock on the Franc/Dollar exchange rate (direct quotation)

Overall, the predictions of our first model do not come as a surprise. The standard Mundell-Fleming mechanisms come to the surface. All equations are complete. The only potential threat to internal validity of the trade balance equation is the omitted variable of foreign GDP. Nevertheless, this exclusion does not make the overall assumptions crumble and the results remain perfectly in line with economic theory.

4.2.2 Model 2: Asymmetric IRFs

Recent literature has thoroughly made the case for impulse response functions to shocks that vary conditional on the state of the business cycle. Our specification gave us the chance to model explicitly this phenomenon, and we attempted it for the sake of completeness. Notwithstanding this, the results below need to be taken with a grain of salt. In either case, the effects of monetary shocks prove to be stronger in recessions than in expansions. Clearly, this goes against any economic theory and possibly against common sense. The main problem here is that most shocks occur during expansions, as they represent a standard countercyclical policy. For this reason, IRFs in recessions are biased due to insucient variance and are not meaningful for the sake of the analysis. For completeness we also have reported the chi-square statistic of the Wald test of equality of the coecients, although clearly a model of state-dependent IRFs cannot be estimated conditional on the information available.

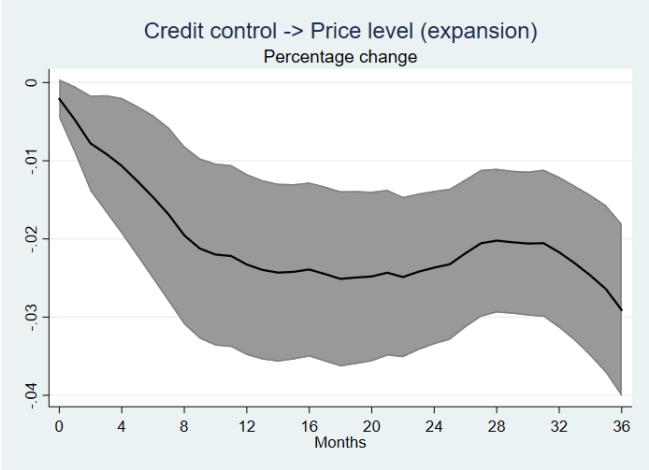


Figure 10: The impact of a restrictive credit control shock on the price level during an expansion

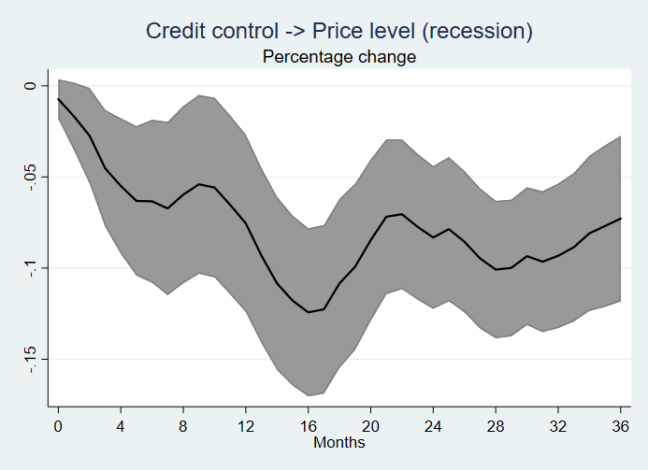


Figure 11: The impact of a restrictive credit control shock on the price level during a recession

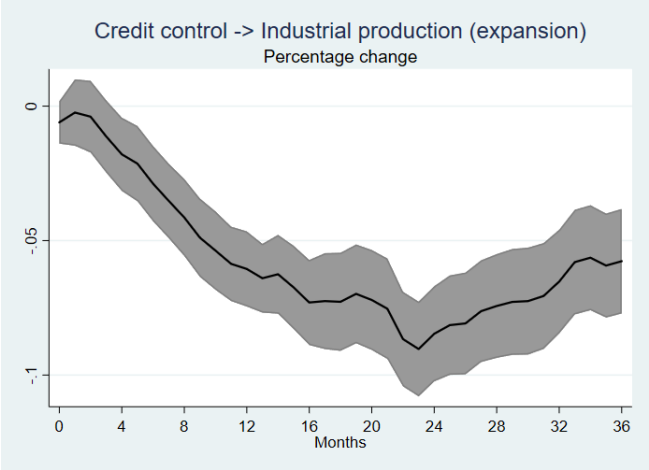


Figure 12: The impact of a restrictive credit control shock on the industrial production index during an expansion

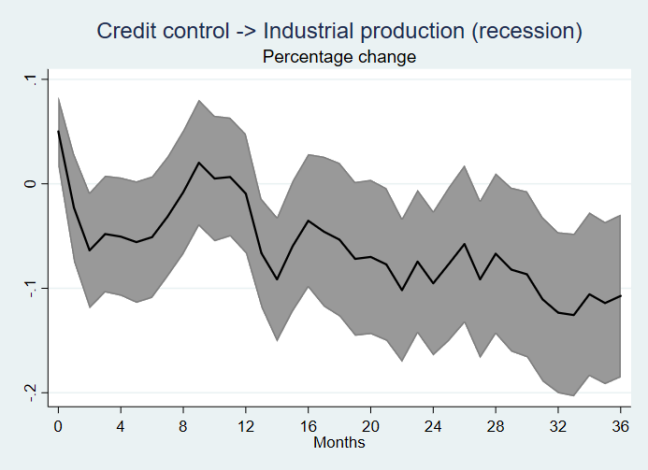


Figure 13: The impact of a restrictive credit control shock on the industrial production index during a recession

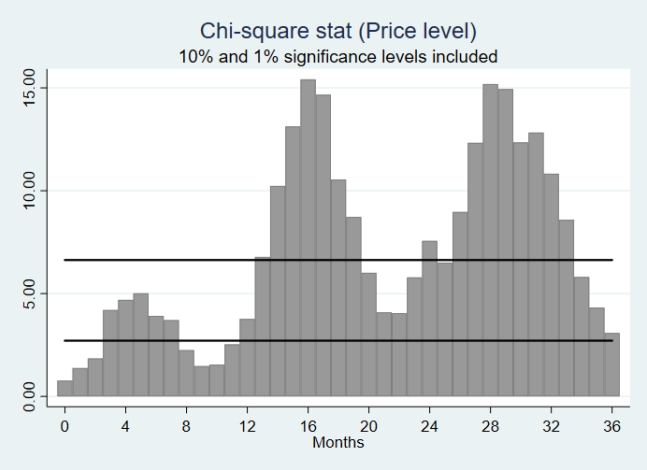


Figure 14: Wald chi-square test statistic of equality between the coefficients of price level impulse response function in both regimes. Horizontal lines denote 10% and 1% significance levels.

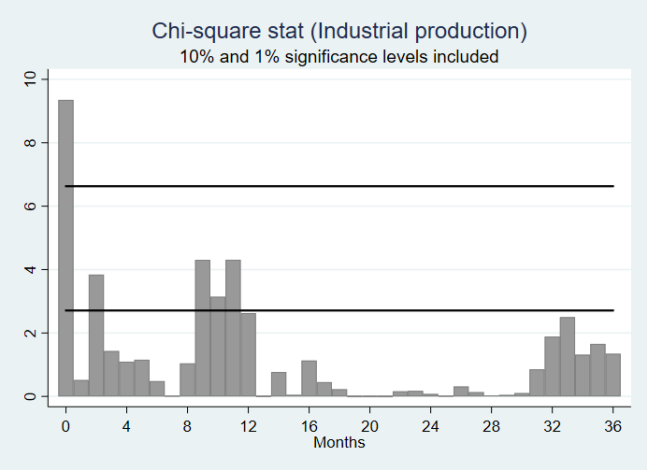


Figure 15: Wald chi-square test statistic of equality between the coefficients of industrial production impulse response function in both regimes. Horizontal lines denote 10% and 1% significance levels.

4.2.3 Model 3: Exchange and interest rates

Our last model has the purpose of analysing the response of endogenous variables to other candidate instruments for macroeconomic policy. First of all, we estimate the IRFs to the central bank's discount rate, which has in other periods has been a core tool for monetary policymakers to adjust in order to achieve targets. Our findings show a significant long-run price and production puzzle, i.e. increasing the discount rate by 1% has positive long-run effects on output and prices. These findings resemble those by Monnet (2014) and they show that the bank rate was not employed as a countercyclical measure.

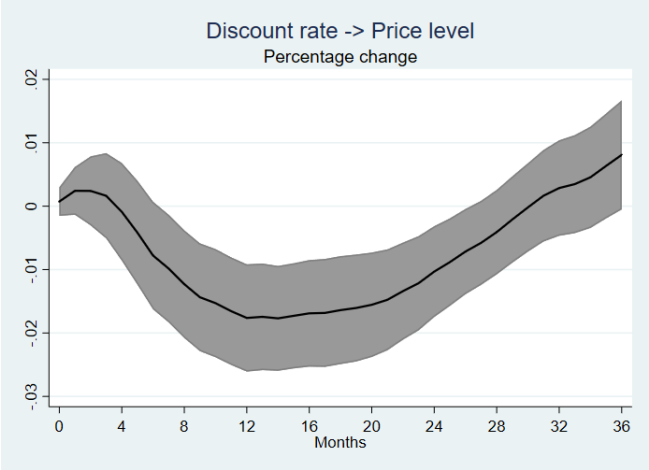


Figure 16: The impact of a shock in the Banque de France's discount rate on the price level.

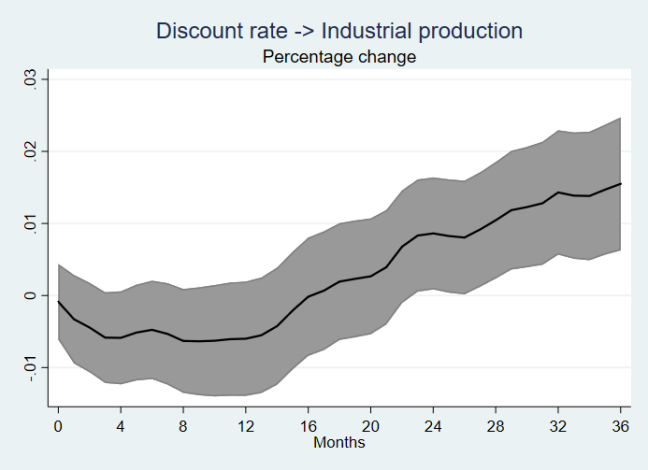


Figure 17: The impact of a shock in the Banque de France's discount rate on the industrial production index.

Additionally, it is also interesting to notice that a shock in the discount rate had a very limited impact on short-run liquidity and virtually no impact on long-run bond yields. Hence, again, it seems likely that a more effective way of impacting the yield curve was the use of direct credit controls.

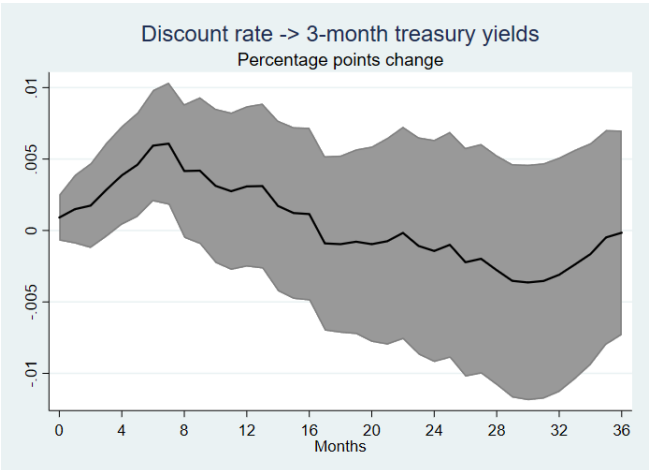


Figure 18: The impact of a shock in the Banque de France's discount rate on French three-month treasury bill yields.

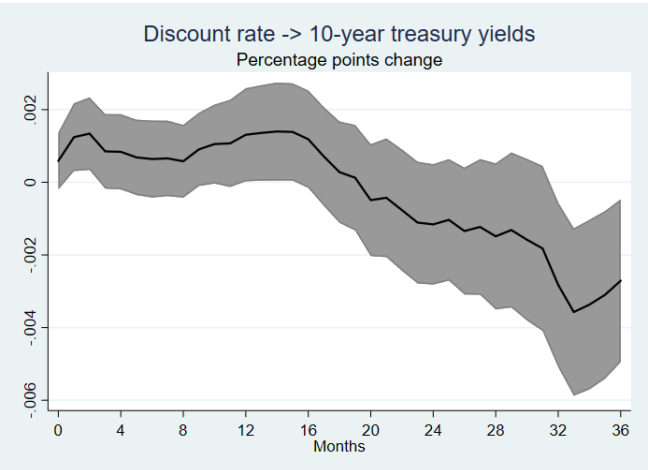


Figure 19: The impact of a shock in the Banque de France's discount rate on French ten-year treasury bond yields.

Clearly, in the period the exchange rate was fixed its rare fluctuations could not have an appreciable impact on the trade balance. Therefore, the only tool for fixing the external

deficit was again monetary policy with quantitative controls. This finding is also in line with the consensus view: the IMF agreement considered the exchange rate devaluation as a tool of last resort only for overcoming balance-of-payments deficits.

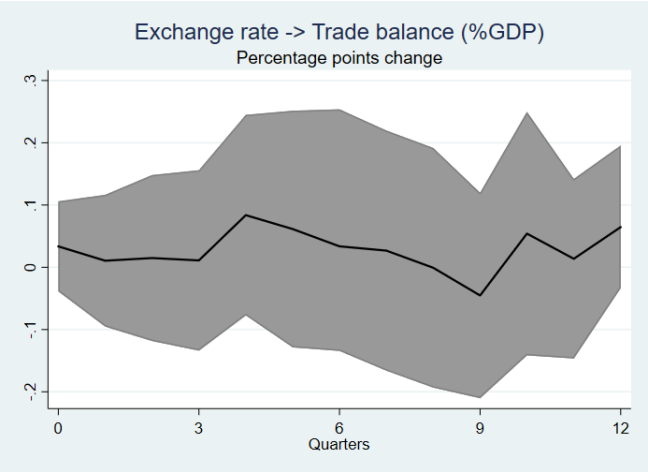


Figure 20: The impact of a 1% devaluation of the Franc/Dollar exchange rate on the trade balance in percentage of GDP

Finally, except for an initial short period of about eight months, shocks in the foreign interest rate did not affect the domestic interest rate, nor the exchange rate. This indicates that international capital flow controls were in place and were binding. Effectively. a consistent spread between the domestic rate and the one implied by uncovered parity persisted. This, therefore, gave the policymaker an additional degree of freedom, while direct credit controls and international capital controls were used complementarily, as in fact argued by Kelber and Monnet (2014).

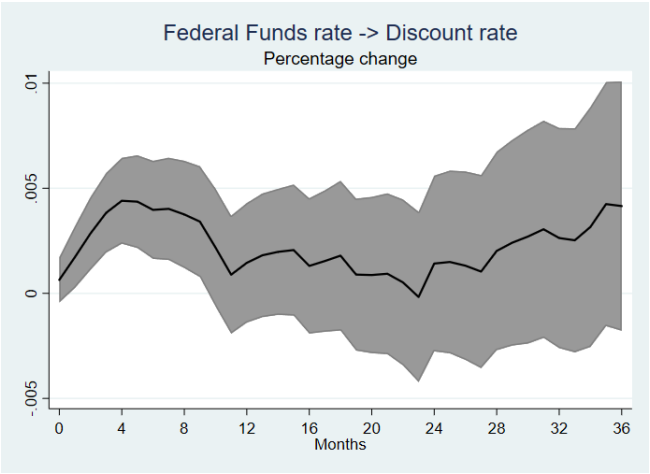


Figure 21: The impact of a shock in the federal funds rate on Banque de France's discount rate.

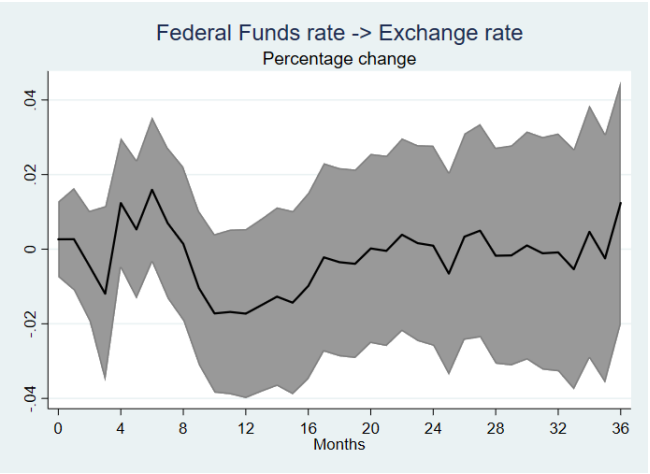


Figure 22: The impact of a shock in the federal funds rate on the Franc/Dollar exchange rate.

5 Concluding remarks

Our results strongly confirm the evidence found by Monnet (2014). Furthermore, they are consistent overall with a generalised open-economy Mundell-Fleming environment where there are strict controls on cross-border financial capital flows. Specifically, we are able to generalise the previous inquiry to a general equilibrium setting. This allows us to understand which tools where used to address which targets, as well as the effects they had on endogenous macroeconomic variables. These results are encouraging for building up a new methodology for studying the implementation issues and the effects of monetary policy during the Bretton Woods Era. There is indeed room for promising future research to apply this methodology to other countries that notoriously experienced direct credit controls as a primary tool of monetary policy in those decades.

In detail, our case study helps to confirm empirically many aspects of the consensus view of monetary policy implementation during the aforementioned period. Most notably, we strongly confirm the irrelevance of the discount rate for the monetary policy stance. Effectively, as Monnet (2014) records, many policymakers agreed at the time that central bank rates had lost their conventional meaning for monetary policy. Our results strongly support this view, which was dominant in France during the thirty postwar years of the *Trente Gloriuses*. At the same time, the negligible impact of movements in the foreign interest rate signals the presence of non-negligible hurdles to cross-border capital

flows.

Another important feature our analysis is able to underscore is the fact that were reluctant to use the exchange rate for adjusting the external deficit. Rather, they seemed to assign credit controls to the double task of achieving both the internal and the external balance. This confirms the philosophy underlying the core IMF agreement: exchange rates were fixed and capital flows were controlled. Adjustments from parity were small and often discouraged, therefore preferring the latter tool for achieving a trade balance equilibrium.

These results also help shed more light on some details that have been overlooked, possibly due to the lack of in-depth empirical analyses. For instance, the stabilisation of the foreign exchange rate in France has been more effectively implemented through credit controls rather than through the interest rate, as common wisdom has always suggested. In a similar fashion, the pass-through of shocks from the central bank policy rate appears to have been very contained, therefore undermining the traditional view that conventional monetary policy was used to control the yield curve, whereas the overall degree of liquidity in the money market was mainly affected through credit controls.

References

Aikman, D., Bush, O., Taylor, A., M., 2016, "Monetary versus macroprudential policies: causal impacts of interest rates and credit controls in the era of the UK Radcliffe Report", NBER working paper 22380.

Bahadir, B., Gumus, I., 2016, "Credit decomposition and business cycles in emerging market economies", *Journal of International Economics*, 103 (4), 250-262.

Barth, M. J., Ramey, V., 2002, "The Cost Channel of Monetary Transmission", NBER Macroeconomic Annual 2001.

Bernanke, B. S., Gertler, M., 1995, "Inside the black box: the credit channel of monetary policy transmission", *Journal of Economic Perspectives*, 9 (4), 27-48.

Bordo, M. D., 2017, "The Operation and Demise of the Bretton Woods System; 1958 to 1971", NBER working paper 23189.

Bordo, M. D., Redish, A., 2003, "Is deflation depressing? Evidence from the classical gold standard", NBER working paper 9520.

Bordo, M. D., James, H., 2015, "Capital flows and domestic and international order: Trilemmas from macroeconomics to political economy and international relations", NBER working paper 21017.

Ciccarelli, M., Mojon, B., 2010, "Global Inflation", *The Review of Economics and Statistics*, 92 (3), 524-535.

Eichengreen, B., 2008, "Globalizing capital: a history of the international monetary system", Princeton University Press.

Eichengreen, B., Flandreau, M. (eds.), 1997, "The gold standard in theory and history" (Vol. 2). London: Routledge.

Gilchrist, S., Schoenle, R., Sim, J., 2017, "Inflation dynamics during the financial crisis", *American Economic Review*, 107 (3), 785-823.

Gorski, M., 1945, "Summary report on Bretton Woods monetary conference", William McChesney Martin Jr. Collection, Federal Reserve Bank of St. Louis, FRASER Website.

Granger, C. W. J., Terasvirta, T., 1993, "Modelling non-linear economic relationships", OUP Catalogue, Oxford University Press, number 9780198773207.

Hodgman, D., 1973, "Credit controls in Western Europe: An evaluative review", The Federal Reserve Bank of Boston, mimeo.

Issing, O., 2010, "The development of monetary policy in the 20th century–some reflections", in *Der Euro in stürmischen Zeiten: Beiträge von Otmar Issing zur Wirtschafts- und Währungspolitik am Center for Financial Studies 2006-2015*, 103.

Johnson, O. E. G., 1974, "Credit controls as instruments of development policy in the light of economic theory", *Journal of Money, Credit and Banking*, 6 (1), 85-99.

Jorda, O., 2005, "Estimation and inference of impulse responses by local projections", *American Economic Review*, 95 (1), 161-182.

Kelber, A., Monnet, E. 2014. Macroprudential policy and quantitative instruments: a European historical perspective. *Financial Stability Review*, 18, 151-160.

Klein, M. W., Shambaugh, J. C., 2015, "Rounding the corners of the policy trilemma: sources of monetary policy autonomy", *American Economic Journal: Macroeconomics*, 7 (4), 33-66.

Lutfalla, M., Patat, J. P., 1986, "Histoire monétaire de la France au XXème siècle", Paris, Economica.

Mishkin, F. S., 1995, "Symposium on the monetary transmission mechanism", *Journal of Economic Perspectives*, 9 (4), 2-10.

Monnet, E., 2014, "Monetary policy without interest rates: evidence from France golden age (1948 to 1973) using a narrative approach", *American Economic Journal: Macroeconomics*, 6 (4), 137-169.

Obstfeld, M., Shambaugh, J. C., Taylor, A. M., 2004, "Monetary sovereignty, exchange rates, and capital controls: the trilemma in the interwar period", *IMF staff papers*, 51 (1), 75-108.

Radcliffe, C. J., 1959, "Report: Committee on the Working of the Monetary System", HM Stationery Office, London.

Rodrik, D., 1994, "King Kong meets Godzilla: The World Bank and The East Asian Miracle", CEPR discussion papers, 944.

Contacts

Labour Economics Working Group

Ola Aboukhsaiwan (o.aboukhsaiwan@lse.ac.uk)
Ju Yeon Kim (Anna) (J.Kim57@lse.ac.uk)
Celine Mano (c.s.mano@lse.ac.uk)
Caroline Maschka (c.l.maschka@lse.ac.uk)
Lim Xiao Rong (Isabelle) (x.r.lim@lse.ac.uk)

Labour and Demographic Economics Working Group

Minsuk Kim (M.Kim16@lse.ac.uk)
Mikael Johansson (m.johansson@lse.ac.uk)
Sherman Khoo (j.khoo3@lse.ac.uk)

Monetary Economics Working Group

Utkarsh Aggarwal (u.aggarwal@lse.ac.uk)
Francesco Casalena (f.casalena@lse.ac.uk)
Ciaran Marshall (C.J.Marshall1@lse.ac.uk)
Patrick Leitloff (p.m.leitloff@lse.ac.uk)
Tomáš Tapak (t.tapak@lse.ac.uk)