Education and Mobility

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Declaration

I certify that chapter 2 of this thesis, "Studying Abroad and International Labour Market Mobility - Evidence from the introduction of ERASMUS", was coauthored with Matthias Parey. Fabian Waldinger contributed 50 percent to the genesis of the project, 50 percent to the empirical work on the data, and 50 percent to the writing of the text.

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Abstract

This thesis analyses interconnections between educational policies and different aspects of mobility. In the first chapter I use the dismissal of scientists in Nazi Germany to analyse peer effects among university scientists. The usual problems related to estimating peer effects are addressed by using the dismissal of researchers by the Nazi government as a source of exogenous variation in the scientists' peer group. Using a dataset of all physicists, chemists, and mathematicians at all German universities from 1925 until 1938 I investigate spillovers at different levels of peer interactions. There is no evidence for peer effects at the department level or the specialization level. I find, however, that peer quality matters for coauthors. Losing a coauthor of average quality reduces the productivity of a scientist by about 12.5 percent in physics and 16.5 percent in chemistry. The second chapter analyses the effect of studying abroad on international labour market mobility later in life. I have collaborated with Matthias Parey for this research project. We exploit the introduction of the ERASMUS student exchange programme as a source of exogenous variation in student mobility. Our results indicate that student exchange mobility is an important determinant of international labour market mobility: Studying abroad increases an individual's probability of working in a foreign country by about 15 to 20 percentage points. We investigate heterogeneity in returns and find that studying abroad has a stronger effect for credit constrained students. The last chapter of the thesis investigates the effect of school tracking on social mobility of students. In particular I investigate whether ability tracking exacerbates the role of parental background for students' educational achievement. Using microdata from different educational studies I exploit cross-country variation in tracking policies to identify the effect of tracking. Controlling for unobserved country level variables using difference-in-differences, I find no increase in the importance of a student's family background after tracking has taken place. This result runs contrary to the findings of the current literature. I show that the results of the existing literature are not robust to slight changes in specification.

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Preface

This thesis analyses different interlinkages between education and mobility. In this context mobility is broadly defined to encompass physical mobility but also social mobility.
Links between education and mobility have become more salient in the public debate but also in economic research in recent years. These links are important at all educational levels. In this thesis I investigate issues of mobility and education at the university level but also at the school level. At the former, it is widely acknowledged that universities willing to produce first class research and teaching have to attract scholars and students from abroad. Many universities, therefore, benefit from more international mobility. The flip side of increased mobility may be that countries with less attractive universities might lose their brightest students and researchers if they move abroad. Whether the countries which lose students and researchers are truly harmed by the increase in international mobility is, however, not entirely clear. The migrants might return later in life or promote the home country's interests abroad. This thesis analyses a subset of issues linked to international mobility of scientists and students.
In the first chapter I use the forced mobility of scientists in Nazi Germany to identify peer effects among university scientists. The Nazi government dismissed all Jewish and so-called 'politically unreliable' scholars from the German universities as soon as they came into power. The vast majority left Germany and was therefore no longer able to closely collaborate with their former colleagues in Germany. As the number of dismissed scholars varied widely across different universities and departments I can use this dismissal to identify peer effects among scientists. The dismissals not only affected the size of the scientists' peer group but also their average quality. I can therefore separately identify the effect of the number of peers and the effect of average peer quality on a scientist's productivity. I also investigate peer effects at different levels of interactions, namely at the department level (e.g. within the physics department), the specialization level (e.g. among the theoretical physicists in a department), and among coauthors.
Student mobility is another important phenomenon in higher education. Especially since students have become more and mobile in recent years. The second chapter of this thesis analyses how studying abroad affects international labour market mobility later in life. Estimates of the effect of student mobility on later labour market mobility are often contaminated by the fact that individuals who study abroad are probably more mobile than students who do not study abroad. We therefore use the introduction of the ERASMUS student exchange programme as a source of exogenous variation in the probability of studying abroad. We show that the introduction of the ERASMUS programme in a student's department did indeed increase her probability of studying for some time in a foreign country. We can therefore use the ERASMUS programme to identify the causal effect of educational mobility on later labour market mobility.
Mobility or the lack thereof is also an important issue at the school level. Parents move closer to attractive school in order to improve the education of their children. The vast literature on the links between school quality and houseprices analyses some of the consequences of this be-

haviour. Another important aspect of mobility in schools is social mobility. The third chapter of my thesis analyses one potentially important determinant of social mobility within school systems; namely whether students are placed into different school tracks. In particular I investigate whether the tracking regime affects the role of a student's family background for her educational attainment. As countries differ not only in their tracking regime I try to control for the pre-tracking importance of parental background by using a difference-in-differences strategy. Using a test score before tracking has occurred in any of the countries of my sample and one after tracking occurred in some countries I identify the causal effect of tracking.

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1.1 Introduction

This chapter analyses peer effects among university scientists. It is widely believed that peer effects are an important element of academic research. Individual researchers, however, may not consider these effects when deciding about their place of employment. This could potentially lead to a misallocation of talent and to underinvestment in academic research. Having a good understanding of peer effects is therefore crucial for researchers and policy makers alike. Despite the widespread belief in the presence of peer effects among scientists there is only limited empirical evidence for these effects.

The main reason for this lack of evidence lies in the fact that obtaining causal estimates of peer effects is very challenging. An important problem for any estimation of peer effects is caused by sorting of scientists. Highly productive scientists often work alongside other productive researchers while less productive researchers often work in universities with less productive colleagues. The key question is whether productive scientists are more productive because they are collaborating with successful peers or because their productivity is higher per se. Estimation techniques which do not address the sorting of researchers will thus overestimate the importance of peer effects. Another problem corroborating the estimation of peer effects is the presence of unobservable factors which affect a researcher's productivity but also the productivity of his peers. For scientists these factors could be the construction of a new laboratory which the econometrician may not observe. These unobserved factors would usually lead to an upward bias of peer effects. Estimates of spill-over effects may also be distorted by measurement problems. The main problem is the correct measurement of a researcher's peer group. It is not only difficult to identify the peers of any given scientist but also to ascertain the quality of these peers. These problems will complicate any attempt to obtain unbiased estimates of peer effects. A promising strategy to estimate peer effects is therefore to analyse a scientist's productivity if his peer group changes due to reasons which are unrelated to his own productivity.

In this chapter I propose the dismissal of scientists by the Nazi government as an exogenous and dramatic change in the peer group of researchers in Germany. Almost immediately after Hitler's National Socialist party secured power in 1933 the Nazi government dismissed all Jewish and so called "politically unreliable" scholars from German universities. Around 13 to 18 percent of all scientists were dismissed between 1933 and 1934 (13.6 percent of physicists, 13.1 of chemists, and 18.3 percent of mathematicians). Many of the dismissed scholars were outstanding members of their profession, among them the famous physicist and Nobel Laureate Albert Einstein, the chemist Georg von Hevesy who would receive the Nobel Prize in 1943, and the Hungarian mathematician Johann von Neumann. Scientists at the affected departments were thus exposed to a dramatic change in their peer group. Researchers in unaffected departments, however, did not lose a single colleague. I use this dramatic change in the peer group of scientists who remain

in Germany to identify peer effects among physicists, chemists, and mathematicians.
I focus on these subjects because advancements in these fields are widely believed to be an important source of technological progress. Other reasons for focusing on science are the following. The productivity of scientists can be well approximated by analysing publications in academic journals. It was part of the scientific culture to publish results in scientific journals already in the 1920s and 1930s, which is the time period studied in this chapter. I also concentrate on the sciences because of the attempt of the Nazi regime to ideologize all parts of society after 1933. These policies also affected university research. The impact on different subjects, however, was very different. Subjects such as economics, psychology, history, or sociology were affected much more than the sciences.[1] The last reason for focusing on physics, chemistry, and mathematics is the fact that researchers at the German universities were in many cases the leading figures in those fields in the early 20th century. Examples for this leading role of German science at the time are the Nobel Prize awards to researchers from German universities. Between 1910 and 1940, 27 percent of Nobel laureates in physics and 42 percent of Nobel prize winners in chemistry were affiliated with a German university; this is a much larger fraction than that of any other country at that time. If peer effects are an important determinant of a researcher's productivity they are likely to be especially important in a flourishing research environment such as Germany in the early 20th century.
In order to investigate peer effects, I construct a new dataset of all physicists, chemists, and mathematicians teaching at all 33 universities and technical universities *(Technische Hochschulen)* in Germany at the time. Using data from historical university calendars I obtain a panel dataset of all scientists at these universities covering the years 1925 until 1938. I do not consider the years after 1938 because of the start of World War II in 1939. In order to assess the extent of the dismissal I compile a list of all dismissed physicists, chemists and mathematicians from a number of different data sources. Finally, I obtain data on publications and citations of these researchers in the leading academic journals of the time. More details on the data sources are given in the data section below.
This dataset allows me to investigate spill-over effects among scientists. The collaboration of researchers can take different levels of intensity. A very direct way of peer interaction is the collaboration on joint research projects involving joint publication of results. There are, however, more subtle interactions of colleagues in universities. Peer effects would also be present if researchers discuss ideas and comment on each other's work but do not copublish. Yet another way in which peers may affect a researcher's productivity is through peer pressure. A scientist's work effort may depend on the effort of his peers because he may want to match or surpass their research output. Having more (less) productive peers would thus increase (reduce) a researcher's productivity. The definition of peer effects in this chapter encompasses any of these different types. In addition to these different levels in the intensity of peer interactions there

[1] The sciences were not completely unaffected by the Nazi regime. The most famous example is the "German Physics" movement by a small group of physicists which tried to ideologize physical research. The consensus among historians of science, however, is that the movement never mangaged to have a strong impact on the physics community as a whole. See Beyerchen (1977) for details.

are two main dimensions of peer groups which may matter for academic research. The first dimension is the *number* of peers a researcher can interact with. Another important dimension of a scientist's peer group is the *quality* of his colleagues. The work presented in this chapter is the first to separately identify the importance of these two aspects of peer interactions.

This thesis is also the first to analyse three different geographic dimensions of spill-over effects, namely at the department level, at the specialization level, and at the level of coauthors. Many researchers believe that peer interactions occur at the level of academic departments. The first part of the analysis therefore investigates spill-over effects at the department level. The dismissal is a very strong and precise predictor of changes in the number and the average quality of peers. I find, however, that neither the number of dismissed colleagues nor the dismissal induced change in average department quality significantly affects the productivity of physicists, chemists or mathematicians. I also estimate a more structural model of peer effects instrumenting the peer group variables with the dismissal. I do not find any significant effects of the number of peers or their average quality at the *department* level.

Using the same methodology I can also analyse peer effects at the level of a researcher's specialization within the department. It is therefore possible to investigate the presence of peer effects among all theoretical physicists in a department for example. The dismissal is a very strong predictor for the number and the quality of peers at the specialization level. Neither the number of dismissed peers in a researcher's specialization nor their average quality have a significant impact on a researcher's productivity over time. When instrumenting a researcher's peer group with the dismissal I do not find evidence for peer effects at the *specialization* level.

In addition to that I investigate an even narrower definition of a researcher's peer group by estimating peer effects among *coauthors* for physics and chemistry. Due to the very low level of coauthorships in mathematics I cannot analyses spill-over effects for coauthors in mathematics. I find that losing a coauthor of average quality reduces the average researcher's productivity by about 12.5 percent in physics and 16.5 percent in chemistry. Losing coauthors of higher than average quality leads to an even larger productivity loss. Furthermore, I show that the effect is solely driven by recent collaborations. The productivity of scientists who lose a colleague with whom they did not coauthor in the last four years before the dismissal does not fall due to the dismissal. It is not entirely clear whether one would like to call the joint publication of papers a real spill-over effect. I therefore investigate whether authors who lose a coauthor also publish less if one focuses on the publications which were not coauthored with the dismissed coauthor. Finding a drop in these publications after the dismissal would suggest classic spill-over effects between coauthors. I find a negative and significant effect from losing a high quality coauthor even on the publications which were published without the dismissed coauthor. This is evidence for peer effects among coauthors.

Understanding the effects of the dismissal of a large number of scientists during the Nazi period is interesting in its own right. The findings of this thesis may also lead to a better understanding of similar events which occurred in other countries. One example is the purge of thousands of scientists who did not adhere to the communist ideology in the Soviet Union under Stalin.

The scope of this work, however, goes beyond the understanding of historical events, because it allows the identification of peer effects using an exogenous variation in a researcher's peer group. The question remains whether evidence on peer effects in Germany in the 1920s and 1930s can be used to understand peer interactions today. A number of reasons suggest that the findings of this study may be relevant for understanding spillovers among present-day researchers. The three subjects studied in this chapter were already well established at that time; especially in Germany. Scientific research followed practices and conventions which were very similar to current research methods. Researchers were publishing their results in refereed academic journals, conferences were common, and researchers were surprisingly mobile. Unlike today, they could not communicate via E-mail. They did, however, vividly discuss research questions in letters. Given the dramatic fall in communication and transportation costs it is quite likely that localized peer interactions are even less important today than in the 1920s and 1930s. The increased specialization in scientific research makes it harder to find researchers working on similar topics in the same department. This will further contribute to the fact that today's department level and within department specialization level peer effects are less important than in the past.

As described before I do find that peer effects among *coauthors* are important. The reduction in transportation and communication costs would suggest that potential benefits from collaborating with researchers who are located in a different university may be even more important today. The increased importance of teams in the production of scientific research and increased cooperation between researchers from different universities and even countries may be a result of peer effects among coauthors.[2] Thus my results are likely to provide a lower bound for peer effects among coauthors.

This study contributes to a growing literature on peer effects among university researchers. It is, however, one of the first to analyse peer effects among scientists using credibly exogenous variation in peer quality. To my knowledge it is the first study which is able to separate the effects from the number of colleagues and the average quality of those peers. Furthermore, it is the first work to directly analyse peer effects a three levels of peer interactions: at the level of academic departments, at the level of specializations within those departments, and among coauthors.

Azoulay, Wang and Zivin (2007) investigate peer effects among coauthors in the life sciences. Using the death of a prolific researcher as an exogenous source of variation in a scientist's peer group they find that deaths of coauthors lead to a decline in a researcher's productivity. They find stronger effects for more prolific coauthors. Furthermore, they find that co-location of scientists does not increase the effect of a dead coauthor. Surprisingly, they do not find a stronger decline for recent coauthors compared to coauthors who coauthored with the dead scientist long before he died. As they only observe coauthors but not the universe of peers at the university of a dying researcher they cannot directly investigate department level or specialization level

[2] Wuchty, Jones, and Uzzi (2007) show that the number of co-authors in science research increased dramatically since 1955. Furthermore, Adams et al. (2005) show an increase in the geographic dispersion of research teams in the US.

peer effects. A recent study by Weinberg (2007) analyses peer effects among Nobel Prize winners in physics. He finds evidence for mild peer effects among physics Nobel laureates. Using the timing of starting Nobel Prize winning work he tries to establish causality. It is quite likely, however, that this does not fully address the endogeneity problem which may affect his results on spillovers. Kim, Morse, and Zingales (2006) estimate peer effects in economics and finance faculties and find positive peer effects for the 1970s, and 1980s, but negative peer effects for the 1990s. They show some evidence that their results are not contaminated by endogeneity problems. The regression specifically analysing peer effects, however, does not control for endogenous selection of peers.[3]

The remainder of the chapter is organized as follows: the next section gives a brief description of historical details. A particular focus is given to the description of the quantitative and qualitative loss to German science. Section 1.3 gives a more detailed description of the data sources used in the analysis. Section 1.4 describes the identification strategy in further detail. The effect of the dismissal on the productivity of department level and specialization level peers remaining in Germany is analysed in section 1.5. Using the dismissal as an exogenous source of variation in peer quality I then present instrumental variable results of department level and specialization level peer effects in section 1.6. Regressions presented in Section 1.7 probe the robustness of these findings. In section 1.8 I then present evidence on peer effects between coauthors. Section 1.9 concludes.

1.2 The Expulsion of Jewish and 'Politically Unreliable' Scholars from German Universities

Shortly after the National Socialist Party seized power in 1933 the Nazi government implemented the "Law for the Restoration of the Professional Civil Service" on the 7th of April of 1933. Despite this misleading title the law was used to expel all Jewish and "politically unreliable" persons from civil service in Germany. At that time most German university professors were civil servants. Therefore the law was directly applicable to them. Via additional ordinances the law was also applied to university employees who were not civil servants. Thus the law affected all researchers at the German universities. The main parts of the law read:

> *Paragraph 3:* Civil servants who are not of Aryan descent are to be placed in retirement... (this) does not apply to officials who had already been in the service since the 1st of August, 1914, or who had fought in the World War at the front for the German Reich or for its allies, or whose fathers or sons had been casualties in the World War.

[3] Another related strand of the literature focuses on regional spill-over effects of patent citations. Jaffe, Trajtenberg, and Henderson (1993) use an ingenious method to control for pre-existing regional concentration of patent citations. They find that citations of patents are more geographically clustered than one would expect if there were no regional spill-over effects. Thompson and Fox-Keane (2005) challenge those findings in a later paper.

> *Paragraph 4:* Civil servants who, based on their previous political activities, cannot guarantee that they have always unreservedly supported the national state, can be dismissed from service.
>
> ["Law for the Restoration of the Professional Civil Service", quoted after Hentschel (1996)]

In an implementation decree it was further specified that all members of the Communist Party were to be expelled. The decree also specified "Aryan decent" in further detail as: "Anyone descended from Non-Aryan, and in particular Jewish, parents or grandparents, is considered non-Aryan. It is sufficient that one parent or one grandparent be non-Aryan." The law was immediately implemented and resulted in a wave of dismissals and early retirement from the German universities. A careful early study by Harthorne published in 1937 counts 1111 researchers from German universities and technical universities[4] who were dismissed between 1933 and 1934. This amounts to about 15 percent of the 7266 university researchers present at the beginning of 1933. Most dismissals occurred in 1933 immediately after the law was implemented. Not everybody was dismissed as soon as 1933 because the law allowed Jewish scholars to remain in office if they had been in office since 1914 or if they had fought in the First World War or had lost a father or son in the War. Nonetheless, many of the scholars who could stay according to this exception decided to leave voluntarily; for example the Nobel laureates James Franck and Fritz Haber. They were just anticipating a later dismissal as the Reich citizenship laws (*Reichsbürgergesetz*) of 1935 revoked the exception clause.
Table 1.1 reports the number of dismissals in the three subjects studied in this chapter: physics, chemistry, and mathematics. Similarly to Harthorne, I focus my analysis on researchers who had the Right to Teach (*venia legendi*) at a German university. According to my calculation about 13.6 percent of the physicists who were present at the beginning of 1933 were dismissed between 1933 and 1934.[5] In chemistry the loss between 1933 and 1934 was about 13.1 percent and thus slightly lower than in physics.[6] Mathematics experienced the biggest loss of the three subjects with about 18.3 percent dismissals between 1933 and 1934.[7] It is interesting to note, that the percentage of dismissals in these three subjects and at the German universities overall was much higher than the fraction of Jews living in Germany. It is estimated that about 0.7 percent of the total population in Germany was Jewish at the beginning of 1933.
My data does not allow me to identify whether the researchers were dismissed because they were Jewish or because of their political orientation. Other researchers, however, have investigated this issue. Deichmann (2001) studies chemists in German and Austrian universities (after

[4] The German university system had a number of different university types. The main ones were the traditional universities and the technical universities. The traditional universities usually covered the full spectrum of subjects. The technical universities focused on technical subjects.

[5] This number is consistent with the number obtained by Fischer (1991) who reports that 15.5 percent of physicists were dismissed between 1933 and 1940.

[6] Deichmann (2001) calculates a loss of about 24 percent from 1933 to 1939. The difference between the two figures can be explained by the fact that she includes all dismissals from 1933 to 1939. Furthermore my sample includes 5 more universities which all have below average dismissals.

[7] Unfortunately there are no comparable numbers for mathematics by other researchers.

the German annexation of Austria in 1938 the Nazi government extended the aforementioned laws to researchers at Austrian universities). She finds that about 87 percent of the dismissed chemists were Jewish or of Jewish decent. The remaining 13 percent were dismissed for political reasons. Siegmund-Schultze (1998) estimates that about 79 percent of the dismissed scholars in mathematics were Jewish. This suggests that the vast majority of the dismissed were either Jewish or of Jewish decent.

Before giving further details on the distribution of dismissals across the different universities I am going to provide a brief overview over the fate of the dismissed researchers. Immediately after the first wave of dismissals in 1933 foreign émigré aid organizations were founded to assist the dismissed scholars with obtaining positions in foreign universities. The first organization to be founded was the English "Academic Assistance Council" (later renamed into "Society for the Protection of Science and Learning"). It was established as early as April 1933 by the director of the London School of Economics, Sir William Beveridge. In the US the "Emergency Committee in Aid of Displaced Scholars" was founded in 1933. Another important aid organization, founded in 1935 by some of the dismissed scholars themselves, was the Emergency Alliance of German Scholars Abroad ("Notgemeinschaft Deutscher Wissenschaftler im Ausland"). The main purpose of these and other, albeit smaller, organizations were to assist the dismissed scholars in finding positions abroad. In addition to that prominent individuals like Eugen Wigner, Albert Einstein or Hermann Weyl tried to use their extensive network of personal contacts to find employment for less well-known scientists. Due to the very high international reputation of German physicists, chemists, and mathematicians many of them could find positions without the help of the aid organizations. Less renowned and older scientists had more problems in finding adequate positions abroad. Initially many dismissed scholars fled to European countries. Many of these countries were only temporary refuges because the dismissed researchers often obtained temporary positions, only. The expanding territory of Nazi Germany in the early stages of World War II led to a second wave of emigration from the countries which were invaded by the German army. The main destinations of dismissed physicists, chemists, and mathematicians were the United States, England, Turkey, and Palestine. The biggest proportion of dismissed scholars in all three subject eventually moved to the United States. For the purposes of this research it is important to note that the vast majority of the emigrations took place immediately after the researchers were dismissed from their university positions. Further collaborations with researchers staying in Germany were thus extremely difficult and did hardly occur. A minority of the dismissed, however, did not leave Germany and most of them died in concentration camps or committed suicide. Very few, managed to stay in Germany and survive the Nazi regime. Even these scientists who stayed in Germany were no longer allowed to use university laboratories and other resources. The possibility of ongoing collaboration of the dismissed scientists with researchers staying at the German universities was thus extremely limited.

The aggregate numbers of dismissals hides the fact that the German universities were affected very differently by the dismissals. Even within a university there was a lot of variation across different departments. Whereas some departments did not experience any dismissals others lost more than 50 percent of their personnel. The vast majority of dismissals occurred in 1933 and 1934. Only a limited number of scientists was dismissed after these years. All dismissals occurring after 1933 affected researchers who had been exempted under the clause for war veterans or for having obtained their position before 1914. In order to have a sharp dismissal measure I thus focus on the dismissals in 1933 and 1934. Table 1.2 reports the number of dismissals in the different universities and departments. An example for the huge variation in dismissals is the university of Göttingen, one of the leading universities at the time. It lost 40 percent of its researchers in physics and almost 60 percent in mathematics. The reduction of peer quality in physics and mathematics was even higher than the fraction of dismissed scholars. In chemistry, however, no a single scholar was dismissed in Göttingen. Table 1.2 also demonstrates that the dismissal did not always have a negative impact on average peer quality. Negative signs in the "Fall in Peer Quality" variable indicate an improvement in the average quality of the stayers' peers. An improvement in average peer quality could occur if below department average researchers were dismissed.

Table 1.3 gives a more detailed picture of the quantitative and qualitative loss in the three subjects. In physics about 13 percent of all researchers were dismissed between 1933 and 1934. The proportion of chaired professors among the dismissals was, however, slightly lower at about 11 percent. The dismissed were on average about 5 years younger than the stayers. It is remarkable that the proportion of Nobel laureates (who either already had received the Nobel Prize or were to receive it in later years) among the dismissed was far higher than one would expect given the total number of dismissals. The fact that the dismissed made above average scientific contributions is also exemplified by the fact that the proportion of publications in the leading journals was about 20 percent which is higher than the 13 percent which would correspond to their head count.[8] When considering a publication's quality by weighting the publications with subsequent citations of a paper, the high productivity of the dismissed becomes even more apparent. The dismissed physicists published about 39 percent of citation weighted publications. The fact that the dismissed physicists were of above average quality has already been noticed by Fischer (1991).

About 33 percent of the publications in the top journals were co-written papers. About 11 percent (for the dismissed 14 percent) of all papers were written with a coauthor who was teaching at a university in Germany. There are two main reasons for the fact that only one third of the coauthors were teaching at a German university. Presumably, a large fraction of coauthors of the physicists in my dataset are their assistants or PhD. students. Furthermore, coauthors could teach at a foreign university or be employed by a research institute. The last line of Table 1.3 shows the low level of cooperation within a department; only about 4 percent (9 percent of the dismissed) of all publications were coauthored with a member of staff from the same university.

[8]For a more detailed description of the publications data see the Data Section.

The dismissed chemists are more similar to the average in their field as can be seen from the second panel of Table 1.3. The proportion of full professors among the dismissed almost corresponded to their proportion among all chemists. Also their average age was very close to the population average in chemistry. The proportions of Nobel Prize winners, publications and citation weighted publications was higher than one would expect from their proportion in the overall population. These differences, however, were smaller than in physics. The percentage of co-written papers was much higher in chemistry compared to physics. About 76 percent of published papers were published by more than one author. Interestingly only about 12 percent of the top publications in chemistry were published with coauthors who had the Right to Teach at a German university. Only about 5 percent of all published papers were copublished with a coauthor who was a member of staff in the same department. This is low given the overall high level of coauthorship in chemistry.

In mathematics the differences between the dismissed scholars and the stayers are even more pronounced than in physics. The dismissed scholars were less likely to be full professors but they were also on average about 7 years younger than the stayers. The dismissed not only published more than their counterparts who remained at the German universities but their publications seem to be of much higher scientific importance. This is exemplified by the fact that their publications were cited far more often than their proportion in the general population would suggest. Only about 11 percent (15 percent for the dismissed) of the publications in mathematics were coauthored. An even smaller fraction of the papers were coauthored with researchers who had the right to teach at the German universities. Coauthorships with mathematicians from the same department only accounted for 3 percent of all published papers (4 percents for the dismissed). This suggests an even lower level of inter-departmental cooperation in mathematics compared to the other two subjects.

Despite a relatively similar quantitative loss in all three subjects the qualitative loss in chemistry was lower than in physics. The qualitative loss in mathematics was even higher than in physics. Before investigating the effect of the dismissal on the productivity of researchers who stayed in Germany I first describe my data sources in the next section.

1.3 Data

1.3.1 Data on Dismissed Scholars

The data on the dismissed scholars is obtained from a number of different sources. The main source is the "List of Displaced German Scholars". This list was compiled by the relief organization "Emergency Alliance of German Scholars Abroad". With the aid of the Rockefeller Foundation it was published in 1936. The purpose of publishing this list was to secure positions for the dismissed researchers in countries outside Nazi Germany. Overall, the list contained about 1650 names of researchers from all university subjects. The list gives a very complete picture of the dismissal of scholars by the Nazi government. I extracted all dismissed physicists, chemists, and mathematicians from the list. In the appendix I show a sample page from the physics section of that list. Interestingly, there are four physicists on that page who had already received the Nobel Prize or were to receive it in later years. Out of various reasons a small number of dismissed scholars did not appear in that list. To get a more comprehensive picture of all dismissals I complement the information in the "List of Displaced German Scholars" with information from other sources.

The main additional source is the "Biographisches Handbuch der deutschsprachigen Emigration nach 1933 - Vol. II : The arts, sciences, and literature". The compilation of the handbook was initiated by the "Institut für Zeitgeschichte München" and the "Research Foundation for Jewish Immigration New York". Published in 1983 it contained short biographies of artists and university researchers who emigrated from Nazi Germany. Kröner (1983) extracted a list of all dismissed university researchers from the handbook. I use Kröner's list to append my list of all dismissed scholars.

In addition to these two main data sources I rely on data compiled by historians who studied individual academic subjects during the Nazi era. Beyerchen (1977) included a list of dismissed physicists in his book about the physics community in Nazi Germany. I use the information included in that list to amend my list of dismissed scholars. Furthermore, I use data which is contained in an extensive list of dismissed chemists which was compiled by Deichmann (2001). In a similar fashion I complement my list with the information listed in Siegmund-Schultze's (1998) book on dismissed mathematicians.

It is important to note that my list of dismissals also contains the few researchers who were initially exempted from being dismissed but resigned voluntarily. The vast majority of them would have been dismissed due to the racial laws of 1935 anyway and were thus only anticipating their dismissal. All of these voluntary resignations were directly caused by the discriminatory policies of the Nazi regime.

1.3.2 Data on all Scientists at German Universities between 1925 and 1938

To investigate the impact of the dismissals on the researchers who stayed at the German universities I construct a full list of all scientists at the German universities from 1925 to 1938. Using the semi-official University Calendar[9] I compile an annual roster of *all* physicists, chemists, and mathematicians at the German universities from the winter semester 1924/1925 (lasting from November 1924 until April 1925) until the winter semester 1937/1938. The data for the technical universities starts in 1927/1928, because the University Calendar included the technical universities only after that date. The University Calendar is a compilation of all individual university calendars listing the lectures held by each scholar in a given department. If a researcher was not lecturing in a given semester he was still listed under the heading "not lecturing". From this list of lectures I infer the subject of each researcher to construct yearly faculty lists of all

[9] The University Calender was published by J.A. Barth. He collected the official university calenders from all German universities and compiled them into one volume. Originally named "Deutscher Universitätskalender". It was renamed into "Kalender der deutschen Universitäten und technischen Hochschulen" in 1927/1928. From 1929/1930 it was renamed into "Kalender der Deutschen Universitäten und Hochschulen". In 1933 it was again renamed into "Kalender der reichsdeutschen Universitäten und Hochschulen".

physics, chemistry, and mathematics departments. This allows me to track yearly changes of all researchers of individual departments between 1925 and 1938.[10,11]

To assess a researcher's specialization I consult seven volumes of "Kürschners deutscher Gelehrten-Kalender". These books are listings of German researchers compiled in irregular intervals since 1925.[12] The editors of the book obtained their data by sending out questionnaires to the researchers asking them to provide information on their scientific career. I use the information in all volumes published until 1950 to ascertain a scientist's specialization. Because of the blurred boundaries of the specializations in mathematics many mathematicians did not provide their specialization. In those cases I infer their specialization from the main publications they list in the "Gelehrtenkalender". As the participation of the researchers in the complication was voluntary not all of them provided their personal information to the editor. If I cannot find a scientist's specialization in any of the volumes of the "Gelehrtenkalender", which occurs for about 10 percent of the scientists, I conduct an internet-search for the scientist to obtain his specialization. Overall I obtain the scientist's specialization for about 98 percent of all researchers.[13] Table 1.18 in the appendix of chapter 1 gives an overview of all specializations and the fraction of scientists in each of them.

1.3.3 Publication Data

To measure a researcher's productivity I construct a dataset containing the publications of each researcher in the top academic journals of the time. At that time most German researchers published in German journals. The quality of these German journals was usually very high because many of the German physicists, chemists, and mathematicians were among the leaders in their field. This is especially true for the time before the dismissal as is exemplified by the following quote; "Before the advent of the Nazis the German physics journals (*Zeitschrift für Physik, Annalen der Physik, Physikalische Zeitschrift*) had always served as the central organs of world science in this domain... In 1930 approximately 700 scientific papers were printed

[10] At that time a researcher could hold a number of different university positions. Ordinary Professors held a chair for a certain subfield and were all civil servants. Furthermore there were different types of Extraordinary Professors. First, they could be either civil servants (*beamteter Extraordinarus*) or not have the status of a civil servant *(nichtbeamteter Extraordinarius)*. Universities also distinguished between extraordinary extraordinary professors *(ausserplanmäßiger Extraordinarus)* and planned extraordinary professors *(planmäßiger Extraordinarius)*. Then as the lowest level of university teachers there were the *Privatdozenten* who were never civil servants. Privatdozent is the first university position a researcher could obtain after the 'venia legendi'.

[11] The dismissed researchers who were not civil servants (Privatdozenten and some Extraordinary Professors) all disappear from the University Calendar between the winter semester 1932/1933 to the winter semester 1933/1934. Some of the dismissed researchers who were civil servants (Ordinary Professors and some Extraordinary Professors), however, were still listed even after they were dismissed. The original law forced Jewish civil servants into early retirement. As they were still on the states' payroll some universities still listed them in the University Calendar even though they were not allowed to teach or do research anymore. My list of dismissals includes the exact year after which somebody was barred from teaching and researching at a German university. I thus use the dismissal data to determine the actual dismissal date and not the date a dismissed scholar disappears from the University Calendars.

[12] The first volume was compiled in 1925. The other volumes I have used were published for the years 1926, 1928/29, 1931, 1935, 1940/41, and 1950.

[13] Some researchers cite more than one specialization. Therefore, physicists and chemists have up to two specializations and mathematicians up to four.

in its (the Zeitschrift für Physik´s) seven volumes of which 280 were by foreign scientists." (American Association for the Advancement of Science (1941)). Simonsohn (2007) shows that neither the volume nor the content of the "Zeitschrift für Physik" changed dramatically in the post dismissal years until 1938. Not surprisingly, however, he finds that the dismissed physicists published less and less in the German journals after the dismissal. It is important to note that the identification strategy outlined below relies on changes in publications of researchers in different German departments which were differentially affected by the dismissal. A decline in the quality of the considered journals would therefore not affect my results as all regressions are estimated including year fixed effects.

The list of top publications is based on all German general science, physics, chemistry, and mathematics journals which are included in the "ISI Web of Science" for the time period 1915 to 1940. Furthermore I add the leading general journals which were not published in Germany, namely Nature, Science, and the Proceedings of the Royal Society of London to the dataset. I also add four non-German top specialized journals which were suggested by historians of science as journals of some importance for the German scientific community.[14] The "Web of Science" is an electronic database provided by Thomson Scientific containing all contributions in a very large number of science journals. In 2004 the database was extended to include publications between 1900 and 1945. The journals included in that extension were all journals which had published the most relevant articles in the years 1900 to 1945.[15] This process insures that all publications which can be obtained for the early time period 1900 to 1945 were published in the most important journals.

Table 1.4 lists all journals used in my analysis. For each of these journals I obtain all articles published between 1925 and 1940 from the "ISI Web of Science". A very small number of the contributions in the top journals were letters to the editor or comments. I restrict my analysis to contributions classified as "articles" as they provide a cleaner measure for a researcher's productivity. The database includes the names of the authors of each article and statistics on the number of subsequent citations of each of these articles. For each researcher I then calculate two yearly productivity measures. The first measure is equal to the sum of publications in top journals in a given year. In order to quantify an article's quality I construct a second measure which accounts for the number of times the article was cited in *any* journal included in the Web of Science in the first 50 years after its publication. This includes citations in journals which are not in my list of journals but which appear in the Web of Science. This therefore includes citations from the international scientific community and is not as heavily based on Germany as the publications measure. This measure, which I call citation weighted publications, is defined as the sum of citations (in the first 50 years after publication) of all articles published in a

[14]The relevant journals for chemists were suggested by Ute Deichmann and John Andraos who both work on chemistry in the early 20th century. Additional journals for mathematics were suggested by Reinhard Siegmund-Schultze and David Wilkins; both are specialists in the history of mathematics.

[15]For that extension Thomson Scientific judged the importance of a journal by the later citatons (cited between 1945 and 2004) in the Web of Science of articles published between 1900 and 1945. This measure insures that the most relevant journals for the time period 1900 to 1945 were included in the extention. For more details on the process see www.thomsonscientific.com/media/presentrep/facts/centuryofscience.pdf.

certain year. The following simple example illustrates the construction of the citation weighted publications measure. Suppose a researcher published two top journal articles in 1932. One is cited 5 times in any journal covered by the Web of Science in the 50 years after its publication. The other article is cited 7 times in 50 years. Therefore the researcher's citation weighed publications measure for 1932 is 5+7=12.

Table 1.19 lists the top researchers for each subject according to the citation weighted publications measure. The researchers in this table are the 20 researchers with the highest yearly averages of citation weighted publications for publications between 1925 and 1932. It is reassuring to realize that the vast majority of these top 20 researchers are well known in the scientific community. Economists will find it interesting that Johann von Neumann is the most cited mathematician. The large number of Nobel laureates among the top 20 researchers indicates that citation weighted publications are a good measure of a scholar's productivity. Nevertheless, the measure is not perfect. As the "Web of Science" only reports last name and the initial of the first name for each author there are some cases where I cannot unambiguously match researchers and publications. In these cases I assign the publication to the researcher whose field is most closely related to the field of the journal in which the article was published. In cases where this assignment rule is still ambiguous between two researchers I assign each researcher half of the publications (and half of the citations). Another problem is the relatively large number of misspellings of authors' names. All articles published between 1925 and 1940 were of course published on paper. In order to include these articles into the electronic database Thomson Scientific employees scanned all articles published in the historically most relevant journals. The scanning was error prone and thus lead to misspellings of some names. As far as I discovered these misspellings I manually corrected them. It is possible, however, that there are still misspellings which I could not detect. Therefore, there may still be articles which are not or wrongly assigned to the relevant author.

I merged the publications data to the roster of all German physicists, chemists, and mathematicians. From the list of dismissed scholars I can identify the researchers who were dismissed and those who stayed at the German universities. The end result is a panel dataset of all physicists, chemists, and mathematicians at all German universities from 1925 until 1938 with detailed information on their publications in the top academic journals and their dismissal status.

1.4 Identification

The main purpose of this chapter is to estimate peer effects among scientists. The standard approach when estimating peer effects consists of regressing an individual's productivity on the average productivity of his peers. The productivity of academic researchers, however, is not only affected by the average quality of their peers but also by the number of peers they can interact with. Having smart colleagues may be useful in many ways: coauthored work may be of higher quality and comments from prolific peers may be useful for their own work. Furthermore, peers may attract more research funding to the department, or have better contacts to researchers

outside the department. Having more colleagues in your department may be important because all these interactions are more likely to occur if there are more peers to interact with, especially because it may be easier to find colleagues who are working on similar research questions. Researchers in larger departments may also benefit from a lower teaching load and from teaching more specialized courses which are more related to their current research.
As university departments differ substantially in the average quality of its researchers and also in size it is important to distinguish these two dimensions of peer effects for academic research. In order to estimate peer effects among scientists I therefore propose the following regression:

$$\text{(1)} \quad \#\ \text{Publications}_{iut} = \beta_1 + \beta_2(\#\ \text{of Peers})_{ut} + \beta_3(\text{Avg. Peer Quality})_{ut} + \beta_4\text{Age Dummies}_{iut} + \beta_5\text{YearFE}_t + \beta_6\text{UniversityFE}_u + \beta_7\text{IndividualFE}_i + \varepsilon_{iut}$$

I regress the number of publications of researcher i in university u and year t on measures of the peer group and other controls. In order to control for the quality of a published article I also use a second dependent variable, namely citation weighted publications. As the subjects in consideration are quite different I estimate these regressions separately for physics, chemistry, and mathematics. The peer group measures are a researcher's number of peers and the average quality of these peers. Average peer quality is calculated as the mean of the average productivity (between 1925 and 1932)[16] of a researcher's peers.[17] Over time changes in the average peer quality measure will therefore occur only if the composition of the department changes. The yearly fluctuations in publications of the same set of peers will not affect the peer group measure. The underlying assumption of this measure is therefore that Albert Einstein always has the same effect on his peers independent of how much he publishes in a given year.
It is quite likely that the effect of peers is only measurable after a certain time lag. Peers influence the creation of new ideas and papers before the actual date of publication. Another delay is caused by the publication lag (the time it takes for a paper to appear in a journal after the paper was submitted by the author). Science research, however, is published faster than research in other subjects like economics. Anecdotal evidence suggests that the effect of peers should thus be measured with a lag of about one year. An illustrative example of the timing of peer interactions in science research at the relevant time is the postulation of the "uncertainty principle" by Heisenberg in 1927. In 1926 Heisenberg was working with Niels Bohr in Copenhagen. It is reported that during that time Heisenberg and Bohr spent days and nights discussing the concepts of quantum mechanics in order to refine them. In early 1927 Niels Bohr went on a holiday and it was during that time that Heisenberg discovered and formulated his famous "uncertainty principle". He published this discovery in the "Zeitschrift für Physik" in 1927.[18] Therefore I use a lag of one year for the peer group variables when estimating equation

[16] I use the pre-dismissal period to measure the average quality of peers as this measure will not be affected by the dismissal. Using a measure which considers the average productivity of each researchers from 1925 to 1938 does not have a substantial impact on my findings.

[17] Say a department has 3 researchers in 1930. One published on average 10 (citation weighted) publications between 1925 and 1932. The other two have 20 and 15 citation weighted publications respectively. Then the average peer quality variable for researcher 1 in 1930 will be (20+15)/2 = 17.5. Average peer quality for researcher 2 will be (10+15)/2 = 12.5 and so on.

[18] For a detailed historic description of the discovery of the uncertainty principle see Lindley (2007).

(1).

As further controls I include a full set of 5-year age group dummies to control for life-cycle changes in productivity when estimating equation (1).[19] Furthermore, I control for yearly fluctuations in publications which affect all researchers by including year fixed effects. To control for individual differences in a researcher's talent I also add individual fixed effects to all specifications. Furthermore, I add university fixed effects to control for university specific factors affecting a researcher's productivity. These can be separately identified because some scientists change universities. I show below that the results are hardly affected by including university fixed effects in addition to individual fixed effects.

A number of issues occur when using OLS to estimate equation (1). One problem is caused by the fact that a researcher's productivity is affected by his peers but at the same time the researcher affects the productivity of his peers. Manski (1993) refers to this problem as the reflection problem. It is therefore important to keep in mind that the estimated effects will be total effects after all productivity adjustments have taken place.

Other problems, however, are potentially more severe in this context. An important problem is caused by selection effects. These occur not only because of self selection of researchers into departments with peers of similar quality but also because departments appoint professors of similar quality. Furthermore, larger departments tend to hire researchers with above average qualities. The inclusion of university fixed effects would in principle address this problem. Differential time trends of different departments, however, would make selection issues an important problem even in models which include university fixed effects. These selection effects introduce a correlation of the peer group measures with the error term and will thus bias the estimates of β_2 and β_3.

Another problem may be caused by omitted variables. Omitted factors may not only affect a researcher's productivity but also the size of the department or the average productivity of his peers. This would again bias OLS estimates of β_2 and β_3.

Furthermore, measurement error could bias the estimates of regression (1). An important measurement problem is the correct peer group of a researcher. In addition to that are average number of publications (citation weighted) of peers are by no means a perfect measure for the quality of a researcher's peer group. Even if the number of publications were a perfect measure of peer quality the variable would still suffer from measurement error due to misspelling of names in the publications data. Omitted variables and measurement error will thus introduce further biases of β_2 and β_3.

An instrumental variables strategy can deal with the selection issues, the omitted variables bias, and the measurement error problem. I therefore propose the dismissal of scholars by the Nazi government as an instrument for the scientists' peer group. Figure 1.1 shows the effect of the dismissal on the peer group of physicists. The top panel shows the average department size for two groups of physicists: physicists in departments with dismissals in 1933 or 1934 and physicists in departments without dismissals. It becomes clear from Figure 1.1 that the

[19] Levin and Stephan (1991) show that age is an important determinant of scientists' productivity.

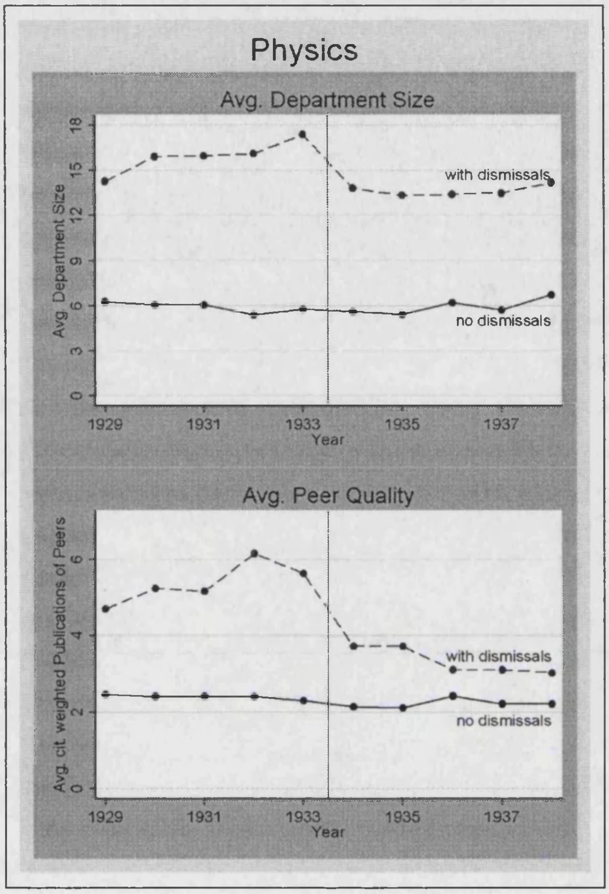

Figure 1.1: Physics: First Stages

affected departments were of above average size. The size of departments without dismissals did hardly change over this time period. In the affected departments, however, the dismissal led to a strong reduction in the number of physicists. The top panel of Figure 1.1 also shows that the dismissals affected the German university system for years after the actual dismissal. This is because the affected departments could not immediately fill their vacancies due to the lack of suitable researchers without a position and the slow appointment procedures. Successors for dismissed chaired professors, for example, could only be appointed if the dismissed scholars gave up all their pension rights because the dismissed professors were originally placed into early retirement. The states did not want to pay the salary for the replacement and the pension for the dismissed professor at the same time. As some of the dismissed hoped to recuperate their chair after what they hoped would be a short Nazi interlude many of them did not immediately cede they pension rights. It thus took years to fill open positions in most cases. Highlighting this problem Max Wien, a physicist in Jena, wrote a letter to Bernhard Rust, the Minister of Education in late November 1934. Describing the situation for chaired professorships at the German universities he stated in this letter that "out of the 100 existing [chaired professor] teaching positions, 17 are not filled at present, while under natural retirements maybe two or three would be vacant. This state of affairs gives cause for the gravest concern..." (cited after Hentschel 1996).

The second panel of Figure 1.1 shows the evolution of average peer quality in the two types of departments. Obviously one would expect a change in the average quality of peers only if the quality of the dismissed was either above or below the pre-dismissal department average. The bottom panel of Figure 1.1 demonstrates two interesting points: the dismissals occurred at departments of above average quality and within those departments the dismissed were on average more productive than the physicists who were not dismissed. As a result the average quality of peers in affected departments fell after 1933 while it remained very stable for researchers in unaffected departments. This graphs only shows averages of the two groups of departments. As can be seen from Table 1.3 some departments with dismissals also lost below average peers. For those departments the average quality increased due to the dismissal. Overall, however, the dismissal reduced average department quality in physics.

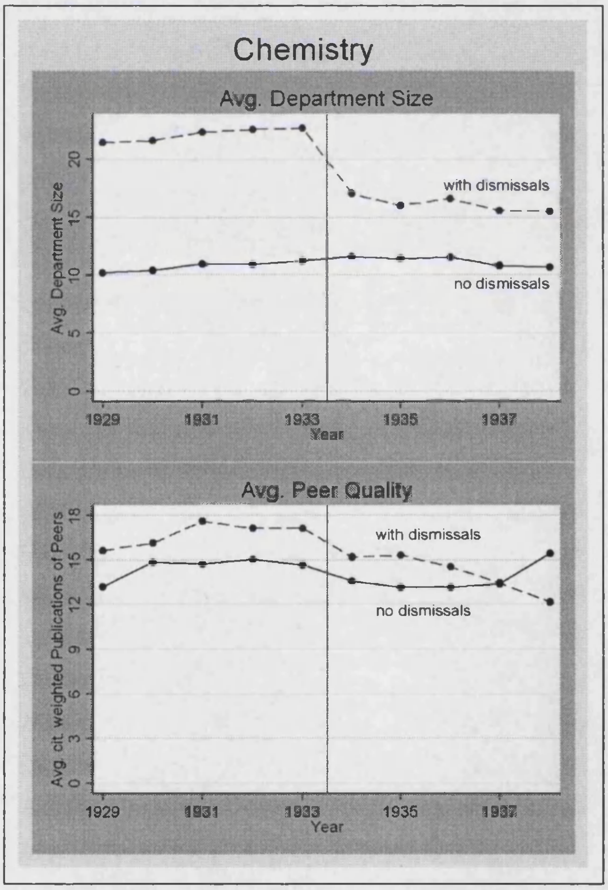

Figure 1.2: Chemistry: First Stages

Figure 1.2 explores the effect of the dismissal on the peer group of chemists. The top panel of Figure 1.2 plots the department size for chemists in affected and unaffected universities. Like in physics most of the dismissals occurred in larger departments and had a strong effect on the department size of these departments. The bottom panel of Figure 1.2 explores the effect of the dismissal on the average quality of peers. The affected departments were also of above average quality but the difference was less pronounced than in physics. As suggested by the summary statistics presented before the dismissal had a smaller overall effect on the average quality of the affected chemistry departments. Despite the fact that the dismissal did not have a large effect on peer quality for the *average* across all departments it did indeed have strong effects on peer quality as can be seen from Table 1.3. The effects in departments with reductions in peer quality and in departments with improvements in peer quality, however, almost cancel out in the aggregate. Figure 1.3 investigates the effect of the dismissals on the peer group of mathematicians. The top panel of Figure 1.3 shows the evolution of department sizes for mathematicians in departments with and without dismissals. Similarly to physics and chemistry the affected departments were larger before the dismissal. After 1933 the department size fell sharply in the affected universities. The bottom panel of Figure 1.3 investigates the effect of the dismissal on the average quality of the researchers' peer group. The mathematicians in the affected departments were above average quality before the dismissal. Due to the dismissal average peer quality fell drastically in the affected departments because average productivity of the dismissed was above average productivity of their respective department.

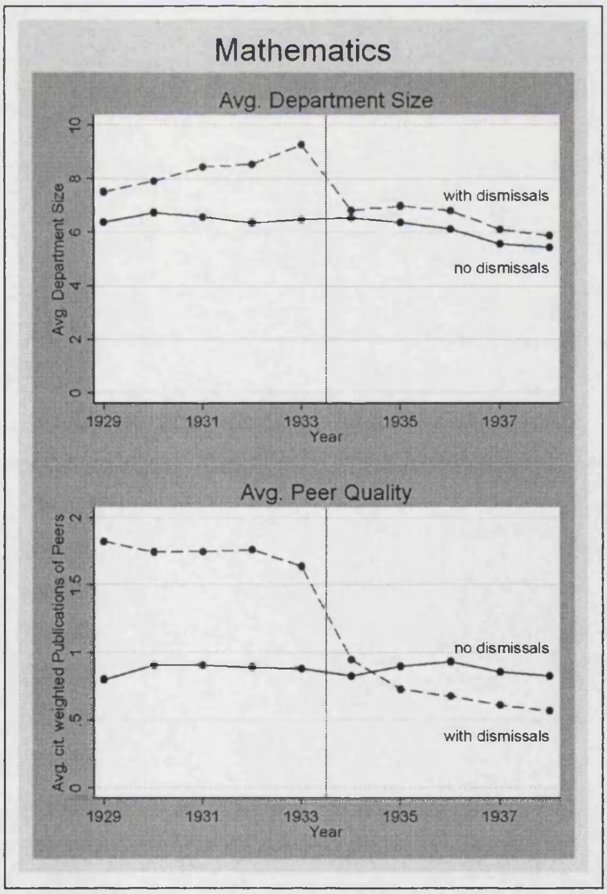

Figure 1.3: Mathematics: First Stages

Figures 1.1 to 1.3 suggest that the dismissal had indeed an effect on the number of peers and their average quality. It is therefore possible to use the dismissal as an instrumental variable for the endogenous peer group variables. In this setting there are two endogenous variables: the number of peers and the average quality of peers. This gives rise to two first stage equations:

$$\text{(2)} \quad \#\text{ of Peers}_{ut} = \gamma_1 + \gamma_2(\#\text{ Dismissed})_{ut} + \gamma_3(\text{Dismissal induced Reduction in Peer Quality})_{ut} + \gamma_4\text{Age Dummies}_{iut} + \gamma_5\text{YearFE}_t + \gamma_6\text{UniversityFE}_u + \gamma_7\text{IndividualFE}_i + \varepsilon_{iut}$$

$$\text{(3)} \quad \text{Avg. Peer Quality}_{ut} = \delta_1 + \delta_2(\#\text{ Dismissed})_{ut} + \delta_3(\text{Dismissal induced Reduction in Peer Quality})_{ut} + \gamma_4\text{Age Dummies}_{iut} + \gamma_5\text{YearFE}_t + \gamma_6\text{UniversityFE}_u + \gamma_7\text{IndividualFE}_i + \varepsilon_{iut}$$

It is important to note that all regressions estimated in this chapter are estimated for scientists who were present at the beginning of 1933 and were not dismissed (the so called stayers). The dismissal is then used as a source of exogenous variation in their peer group. Equation (2) is the first stage regression for department size. The main instrument for department size is the number of dismissed peers between 1933 and 1934 in a given department which is 0 until 1933 and equal to the number of dismissals thereafter.[20] I also include another instrument which captures the dismissal induced reduction in average quality of peers. This will be more important for equation (3), the first stage equation for average peer quality. The dismissal induced reduction in average peer quality is measured as the pre-dismissal average quality of all researchers in the department minus the average quality of the researchers who were not dismissed. The dismissal induced reduction in average quality variable is 0 until 1933. Researchers in departments with dismissals of colleagues of *above* average quality (relative to the department average) have a *positive* value of the dismissal induced reduction in peer quality variable after 1933. Scientists in departments with dismissal of *below* average quality have a *negative* value of the reduction in peer quality variable after 1933. The variable will remain 0 for researchers who did not experience any dismissal in their department or for scientists who lost peers who's quality was exactly equal to the department level average. The dismissals between 1933 and 1934 may have caused some researchers to switch university after 1933. This switching behaviour, however, will be endogenous and thus have a direct effect on the researchers' productivity. To circumvent this problem I assign each scientist the relevant dismissal variables for the department he attended in the beginning of 1933.

The dismissals did affect all stayers in a department in a similar fashion. I therefore account for any dependence between observations within a department by clustering all results at the department level. This not only allows the error to be arbitrarily correlated for all researchers in one department at a given point in time but it also allows for serial correlation of these error terms.

[20]This variable is 0 until 1933 for all departments (As I use a one year lag in the dismissal variables it is 0 for 1933 inclusive). In 1934 it is equal to the number of researchers who were dismissed in 1933 in a given department. From 1935 onwards it is equal to the number of dismissals in 1933 and 1934. The following example illustrates this. In Göttingen there were 10 dismissals in mathematics in 1933 and one dismissal in 1934. The # dismissed variable for mathematicians in Göttingen will therefore take the value 0 until 1933. It will be 10 in 1934 and 11 from 1935 onwards.

Using the dismissal as an instrumental variable relies on the assumption that the dismissal had no other effect on a researcher's productivity than through its effect on the researcher's peer group. Dismissal induced disruption effects would therefore be a potential threat to the identification strategy. These could have occurred if the remaining scientists in the department had to take over more administrative or teaching responsibilities due to the dismissal. These effects would most probably lead to an upward bias of the instrumental variable results. The fact that I do not find evidence for peer effects neither at the department level nor at the specialization level, however, reduces the worry that this problem affects the findings of this chapter.

Another worry may be that the dismissals changed the incentive structure for stayers in the affected departments. Researchers in departments or specializations with many dismissals may have an incentive to work more to obtain one of the free chairs within the department. Their incentives could also be affected in the opposite direction if they lost an important advocate who was fostering their career. In this case they may decide to work less as the chances of obtaining a chair either in their own department or at another university could be lower. In order to address this concern I estimate a regression which regresses a dummy variable of holding a chair (equivalent to being an ordinary professor) on the department level (and specialization level) variables of number dismissed and dismissal induced reduction in average peer quality and the same controls as the regressions proposed before.[21] The results from this regression are presented in Table 1.20. The coefficients on the dismissal variables are all very small and none of them is significantly different from 0. This suggests that the results of this chapter are probably not contaminated by changes in the incentive structures in the affected departments. Before turning to the estimation of peer effects I now investigate the effect of the dismissals on the stayers' productivity.

1.5 Effect of Dismissal on Researchers who remained in Germany

1.5.1 Department Level Dismissal Effect

There is no doubt that the dismissal of the Jewish and "politically unreliable" scholars had a negative impact on the German universities. In this context it is especially interesting to investigate how the dismissal affected the researchers who stayed at the German universities. Did their research productivity suffer because they had less and worse peers? The following figures try to give a graphical answer to this question. Figure 1.4 plots the publications for stayers in two sets of physics departments: those with dismissals and those without dismissals. The yearly fluctuation in top journal publications is relatively large. Despite this fluctuation the figure suggests that the dismissal does not seem to have a very obvious effect on the publications of the stayers.

[21]The estimated regression is:
$(\text{Holder of Chair})_{iut} = \beta_1 + \beta_2(\#\text{ Dismissed})_{ut} + \beta_3(\text{Dismissal induced} \downarrow \text{in Peer Quality})_{ut} + \beta_4\text{Age Dummies}_{iut} + \beta_5\text{YearFE}_t + \beta_6\text{UniversityFE}_u + \beta_7\text{IndividualFE}_i + \varepsilon_{iut}$

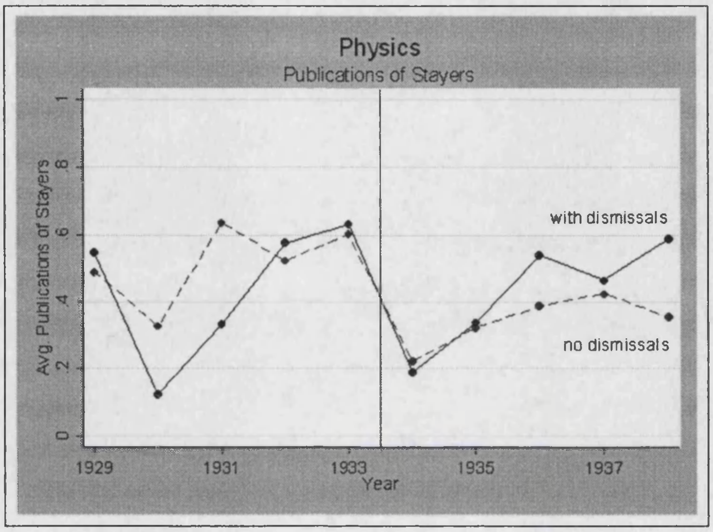

Figure 1.4: Reduced Form Physics

Figure 1.5 shows the evolution of the stayers' publications in chemistry departments. The figure suggests no effect of the dismissal on the stayers' productivity in chemistry.

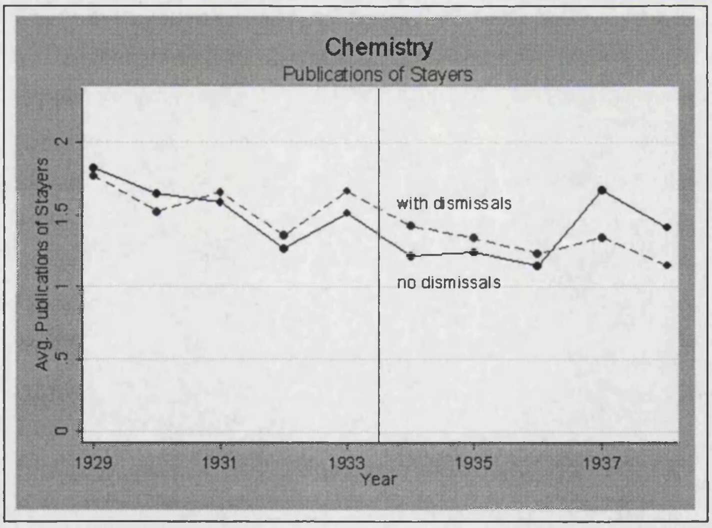

Figure 1.5: Reduced Form Chemistry

Figure 1.6 plots the top journal publications of mathematicians. Similarly to the other two subjects the dismissal does not seem to have a pronounced effect on the publications of the stayers.

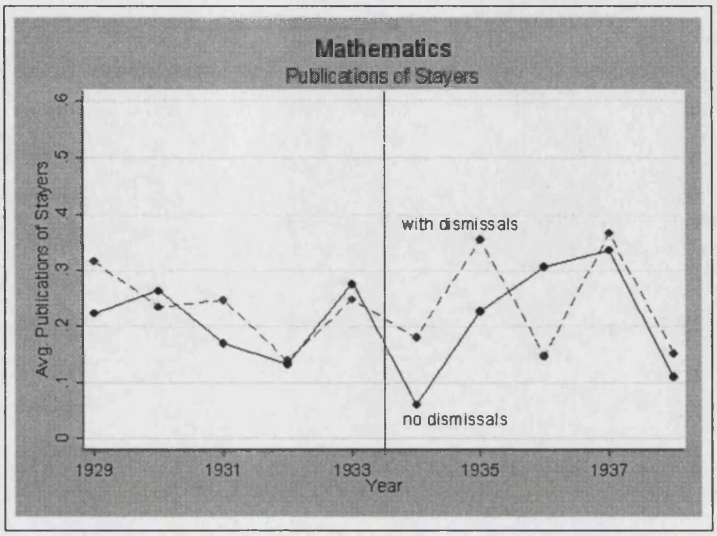

Figure 1.6: Reduced Form Mathematics

Figures 1.4 to 1.6 seem to suggest that the dismissal did not have an effect on the publications of stayers in the affected department. In order to quantify the effect of the dismissal on the stayers I estimate the following reduced form equation.

$$\text{(4)} \quad \#\ \text{Publications}_{iut} = \beta_1 + \beta_2(\#\ \text{Dismissed})_{ut} + \beta_3(\text{Dismissal induced Reduction in Peer Quality})_{ut} + \beta_4\text{Age Dummies}_{iut} + \beta_5\text{YearFE}_t + \beta_6\text{UniversityFE}_u + \beta_7\text{IndividualFE}_i + \varepsilon_{iut}$$

Using only the stayers I regress the researchers' (citation weighted) publications in each year on the instruments proposed above. Namely the number of dismissed peers and the dismissal induced reduction in peer quality. Researchers in departments which were not affected will have a value of 0 for the dismissal variables. Researchers in departments with dismissals will have 0 until 1933 and then the relevant value for the department to which they were affiliated at the beginning of 1933. I also include the same control variables as the ones proposed for regressions (1) to (3). This regression is essentially a difference-in-differences estimate of the dismissal effect. It compares the change in publications from the pre to the post dismissal period for researchers at the affected departments to the change between the two periods for unaffected researchers.

Table 1.5 reports the reduced form results for the three subjects under consideration using the peers in a researcher's department as the relevant peer group. Column (1) shows the results from estimating equation (4) without university fixed effects for physicists using the number of publications as the dependent variable. If the dismissal had a negative effect on the number of publications one would expect negative coefficients on the dismissal variables. Both the coefficient on the number of dismissed researchers and the one on the dismissal induced reduction in peer quality are very close to 0 and not significant. The coefficient on the dismissal induced reduction in peer quality is even positive in sign. This supports the graphical evidence that the publications of the stayers in the affected departments were not strongly affected by the dismissals. In column (2) I add university fixed effects to the specification. The inclusion of university fixed effects hardly affects the results. The dismissal does not have a significant effect on the stayers' productivity. Column (3) shows the results for citation weighted publications as the dependent variable. Also these results are close to 0 and insignificant. Not surprisingly the coefficient is larger than in column (1) because the mean of citation weighted publications variable is 5.1 and thus higher than the mean of publications which is 0.5. The coefficient on the change in peer quality even has the 'wrong' sign if one assumes that losing high quality peers should negatively affect a researcher's productivity. The results including university fixed effects are reported in column (4). As for publications the inclusion of university fixed effects does hardly change the results and does not affect the finding that the dismissal did not affect the stayers' productivity.

Columns (5) to (8) present the same regressions for chemists. The results using publications as the dependent variable not including university fixed effects are reported in column (5). The dismissal induced reduction in peer quality variable is again insignificant and even shows the wrong sign. There is some evidence that the number of dismissed researchers in a stayer's department has a small negative effect on publications. After controlling for university fixed effects, however, the coefficient on the number of dismissed scholars is no longer significantly different from 0. The coefficient on the dismissal induced reduction in peer quality remains insignificant with the wrong sign. Using citation weighted publications all the coefficients are close to 0 and none of the coefficients is significant even without university fixed effects.

The results for mathematicians are reported in columns (9) to (12). Once again the coefficients are very small and all insignificant. These results are a first indication that peers, measured at the department level, may not affect the productivity of scientists. As departments are comprised of scientists with different specializations I want to investigate whether the dismissal had an effect on the stayer's productivity if one considers a narrower definition of his peer group. These results are reported in the next subsection.

1.5.2 Specialization Level Dismissal Effect

It may be the case that the researchers in a scientist's department are an inadequate measure for his peer group if he mostly benefits from interactions with peers in his own specialization within the department. The idea is that theoretical physicists mostly interact with the other theoretical physicists in the department and less with experimental physicists. I therefore explore the dismissal effect on the stayers using only peers from a researcher's own specialization as the relevant peer group.[22] The regression is the same as regression (4) but instead of using the number of department level dismissals I use the number of specialization level dismissals. Similarly I use the reduction in average peer quality in a researcher's specialization instead of the reduction at the department level.

[22] If a researcher has more than one specialization his relevant peer group is defined as the sum of the peers of his specializations.

The results for the specialization level peers are reported in Table 1.6.[23] Columns (1) and (2) show the results for physicists using the number of publications as the dependent variable. The estimated coefficients are very close to 0 and all insignificant. Using citation weighted publications the results are all insignificant as well, as can be seen in columns (3) and (4). Furthermore, all results for physicists have the wrong sign if one expects a negative dismissal effect.

The results for chemistry are reported in columns (5) to (8). None of the coefficients on the dismissal variables is significantly different from 0. The mathematics results are reported in columns (9) to (12). The coefficients on the dismissal variables are small and insignificant when using publications as the productivity measure. When using citation weighted publications as dependent variable I find significant negative effects for the dismissal induced reduction in peer quality. The number of dismissals in a mathematician's specialization however does not affect his productivity.

The department level results suggest that neither the number of peers nor their average quality is very important for a researchers' productivity. Furthermore, the specialization level results indicate that specialization level peer interactions are not important in physics and in chemistry. There is some indication that average peer quality in a researcher's specialization may affect the productivity of mathematicians. The following section explores this in further detail by estimating the peer effects equation (1) instrumenting the peer group variables with the dismissal.

1.6 Using the Dismissal to Identify Peer Effects in Science

1.6.1 Department Level Peer Effects

As suggested by Figures 1.1 to 1.3 the dismissal had a strong effect on the peer group of the stayers at the German universities. I therefore use this exogenous source of variation in a researcher's peer group to identify peer effects. I start by analysing department level peer effect before I investigate specialization level peer effects in the following subsection. As explained in the identification section I estimate two first stage equations: one for the number of peers (i.e. department size) and one for the average quality of peers in a researcher's department. The first stage results are presented in Table 1.7.

Column (1) reports the results from estimating the first stage regression for physicists with department size as the dependent variable. The number of dismissed physicists in a researcher's department has a very strong and significant effect on department size. Reassuringly, the dismissal induced change in the average quality of peers does not seem to have a large effect on department size. The first stage regression for the average peer quality in physics is presented in column (2). The number of dismissals in the department does not have a significant effect on

[23] Due to a small number of missing values for the specialization of a researcher the number of observations is slighly lower than for the department level specifications.

the average quality of peers. The dismissal induced change in peer quality, however, is a very strong and significant predictor of average peer quality for physicists.

Columns (3) and (4) report the first stage regressions for chemists. The results are very similar: the number of dismissals in a department is a very good predictor for department size and the dismissal induced change in peer quality is a very good predictor for the average quality of peers. The regressions for mathematics are presented in columns (5) and (6) and also exhibit a very similar pattern. Overall, the dismissal seems to be a very strong instrument not only for department size but also for the average quality of peers.

Table 1.8 reports the results from estimating the peer effects model as proposed in equation (1). The first columns of Table 1.8 show the results for physicists. Column (1) reports the OLS results with publications as the dependent variable. The OLS results are not very informative due to the problems illustrated in the identification section. I therefore turn immediately to discussing the IV results presented in column (2) in which I use the dismissal to instrument the peer group variables.[24] The coefficient on the number of peers is very small and not significantly different from 0. The coefficient on peer quality is also very small and not significant. It even has the wrong sign if one were expecting positive peer effects from interactions with high quality peers. The standard error implies that one can rule out any positive effects of average peer quality with 89 percent confidence. For the number of peers one can rule out any positive effect larger than .087 with 95 percent confidence. Column (3) reports the OLS result using citation weighted publications as the dependent variable. The IV results with citation weighted publications as the dependent variable are reported in column (4). Once again, the coefficients on the peer group variables are not significantly different from 0 and the coefficient on average peer quality is even negative. Not surprisingly the coefficients are larger in magnitude because the mean of citation weighted publications is much larger than the mean of publications.

The chemistry results are reported in the next few columns of Table 1.8. Column (6) reports IV results when using publications as the dependent variable. The coefficients on department size and on the average number of peers are both very close to 0 and insignificant. The coefficient on the average quality of peers even has a negative coefficient. These results are mirrored in column (8) when using citation weighted publications as the dependent variable. The results for mathematicians are shown in the last few columns of Table 1.8 and are very similar to the ones in physics and chemistry: the coefficients on the peer group variables are all small and not significantly different from 0.

The results presented in Table 1.8 show no evidence for department level peer effects in any of the three subjects. The fact that the results are very similar for all three subjects can be seen as a confirmation that there are indeed no department level peer effects in this setting.

[24]In this setup the instruments are strong predictors of the peer group variables. Furthermore, the model is just identified as the number of instruments is equal to the number of endogenous variables. There is thus no worry of bias due to weak instruments. Stock and Jogo (2005) characterize instruments to be weak not only if they lead to biased IV results but also if hypothesis tests of IV parameters suffer from severe size distortions. They propose values of the Cragg-Donald (1993) minimum eigenvalue statistic for which a Wald test at the 5 percent level will have an actual rejection rate of no more than 10 percent. In this case the critical value is 7.03 and thus far below the Cragg-Donald statistics for the first stages for physics, chemistry, and mathematics which is reported at the bottom of Table 8.

Also the fact that I find very similar results for publications and citation weighted publications is reassuring. This indicates that differences in citation behaviour of articles from scientists in departments with or without dismissals cannot explain these findings. The following subsection analyses peer effects using a narrower definition of a researcher's peer group.

1.6.2 Specialization Level Peer Effects

The relevant peer group considered for the following regressions is defined as all researchers with the same specialization in the scientist's department. For an experimental physicist his peers are now only the other experimentalists in his department but not the theoretical physicists, technical physicists or astrophysicists. The first stage results for specialization level peers are reported in Table 1.9. The first stage results are again very strong and show that the dismissal is a good predictor for a scientist's number of (specialization level) peers and their respective quality especially in physics in chemistry. For mathematicians the dismissal variables are slightly less significant which is due to the fact that many mathematicians have more than one specialization.

Table 1.10 reports the results from estimating equation (1) with specialization level peer variables. The IV results for physics using publications as the relevant dependent variable are reported in column (2). Similarly to before the estimated peer group coefficients are very close to 0 and all insignificant. Both peer group variables even have a negative sign. The standard errors implies than one can rule out any positive effects for the number of peers larger than 0.04 with 95 percent confidence. Similarly any positive effects larger than 0.04 can be ruled out for the quality of peers. Keeping in mind that the mean of the publication variable is about 0.5 for physicists these are precisely estimated zeros. Using citation weighted publications as the dependent variable does not affect these conclusions as can be seen from the results reported in column (4)..

The results for chemists are all close to 0 and insignificant, too. For publications one can rule out any positive effects of having one more peer greater than 0.098 with 95 percent confidence. For the average quality one can rule out any positive effects greater than 0.009 with 95 percent confidence. These are again very small coefficients if one considers the mean of the publication variable for chemistry which is about 1.7.

The results for mathematics are less precisely estimated than for physics and chemistry.[25] But also for mathematics there is no evidence for any significant peer effects. The results on peer effects in a researcher's specialization confirm the finding that peer effects do not seem to play an important role within academic departments. The following section probes the robustness of these results before I turn to investigating peer effects among coauthors.

[25]The Cragg-Donald statistics shows that there is no problem of weak instruments as the relevant critical value is 7.03 in this setup.

1.7 Sensitivity of Department Level IV Results

Table 1.11 shows results from a number of robustness checks for department level peer effects for physics. Columns (1) and (2) show the baseline results for publications and citation weighted publications respectively. The fact that I do not find any significant peer effects may be due to including data from 1933 and 1934 in my estimation. A major part of the dismissals occurred in those two years. The concomitant circumstances of the dismissal may have affected the research and publishing process in a way that made me underestimate peer effects. In order to address this issue I therefore re-estimate the IV equation omitting the data from 1933 of 1934. The results from this exercise are presented in columns (3) and (4). These results suggest that the disruption of 1933 and 1934 does not drive my findings for physicists.

Another hypothesis may be that peer effects are more important in the early stages of a researcher's career. Alternatively they may be more important for older researchers. These hypotheses are investigated in regressions reported in columns (5) to (8). I split the sample into researchers younger than 45 and researchers older than 45. The results indicate that neither young physicists nor old physicists benefit from the number of quality of the peers in their department.

Another worry may be that the productivity of stayers in the affected departments was following an upward trend before the dismissal. This would lead to an underestimate of the dismissal effect and thus bias the IV results downwards. This issue is addressed by including university specific time trends when estimating the IV model. The results for physics including university specific time trends are presented in columns (9) and (10) of Table 1.11. Including university specific time trends does not affect the findings presented before.

Tables 1.12 and 1.13 show the same regressions for department level peer effects among chemists and mathematicians. None of the peer group variables is significantly different from 0 in any of the robustness checks. This indicates that peer effects are indeed absent at the department level. The same robustness checks for specialization level peer effects are presented in Tables 1.21 to 1.23 in the appendix of chapter 1. The coefficients on the peer group variables for specialization level peers do not change much for any of the three subjects. Almost all coefficients remain very close to zero and are insignificant. For older physicists the number of peers variable is significant at the 5 percent level with an unexpected sign. This result would indicate that having more peers negatively affects the productivity of older physicists.

The robustness checks support the evidence that peer effects are inexistent at the department level and at the specialization level. In the following section I explore peer effects among an even smaller set of peers, namely among coauthors.

1.8 Effect of Dismissal on Coauthors

This section analyses peer effects among coauthors. Interactions among coauthors can take very different levels of intensity. The most intense form of interaction is the coauthoring of papers. It is not clear whether one would like to call this interaction a peer effect. Most people would probably call the coauthoring of papers joint production. Nonetheless there are other possible interactions among coauthors. They may also discuss work which they will not publish together. This form of interaction would be a real peer effect.

I investigate peer interactions among coauthors by analysing the change in productivity of scientists who lose a coauthor due to the dismissal. As the fraction of papers coauthored with another faculty level researcher is only 6.3 percent in mathematics, only one mathematician who stayed in Germany lost a coauthor due to the dismissal. Therefore I cannot analyse the effect of losing a coauthor for mathematics. In physics and chemistry, however, there were enough researchers who lost a coauthor due to the dismissal. Figure 1.7 illustrates the impact of losing a coauthor for physics. The figure plots average yearly publications for two groups of researchers; researchers who lost a high quality coauthor due to the dismissal and researchers without dismissed coauthors. Figure 1.7 suggests that physicists who lost a prolific coauthor had a drop in their research productivity but managed to recover after some years.

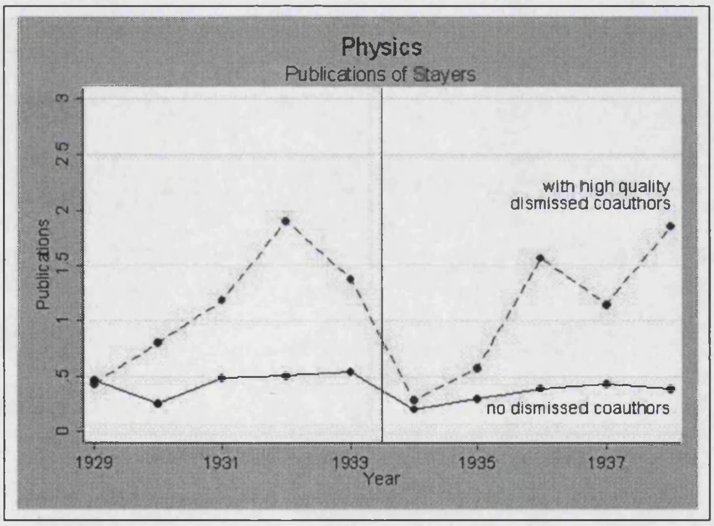

Figure 1.7: Effect of Dismissal on Coauthors Physics

Figure 1.8 shows the same graph for chemists. The productivity of chemists who lost a coauthor falls after the dismissal. Similarly to the effect in physics the productivity of chemists with dismissed coauthors recovers some years after the dismissal.

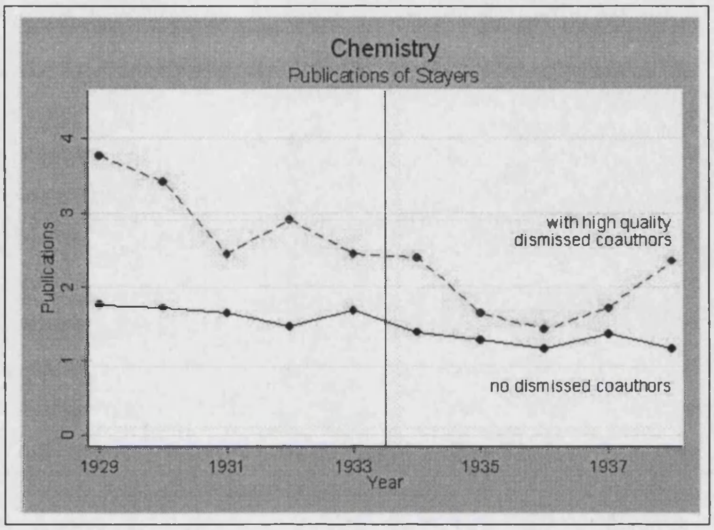

Figure 1.8: Effect of Dismissal on Coauthors Chemistry

In the following I investigate the effect of the dismissal in further detail. I therefore estimate the following reduced form equation:

$$\text{(5)} \quad \#\ \text{Publications}_{iut} = \beta_1 + \beta_2(\#\ \text{Dismissed Coauthors})_{iut} + \beta_3(\text{Avg. Quality of Dismissed Coauthors})_{iut} + \beta_4 \text{Age Dummies}_{iut} + \beta_5 \text{YearFE}_t + \beta_6 \text{UniversityFE}_u + \beta_7 \text{IndividualFE}_i + \varepsilon_{iut}$$

I regress the number of (citation weighted) publications of researcher i in period t and university u on the number of dismissed coauthors, the average quality (measured as before as the yearly average of pre-dismissal citation weighted publications) of the dismissed coauthors, and the same controls as in the regressions reported above. For the basic regression a scientist's coauthors are defined as all colleagues who have coauthored a paper with the scientist in the last five years before the dismissal; i.e. from 1928 to 1932. It is important to note that the dismissed coauthors do not have to be from the same department and indeed are rarely so. As before I estimate this regression for researchers staying in Germany only (the so-called stayers). This regression corresponds to the reduced form regressions reported for the department and specialization level peers. An equivalent instrumental variable approach as before is not feasible for coauthors because the timing of the peer interactions cannot be well defined for coauthors. It is neither clear when peer interactions among coauthors start nor when these interactions end because they are likely to interact also before and after they have coauthored papers. I therefore focus on the reduced form results for coauthors.

The regression estimates of equation (5) are reported in Table 1.14.[26] Columns (1) and (2)

[26] I am estimating these regressions on the same sample as the department level regressions reported before. The number of observations differs slightly from the number of observations in the department level specification because the department level specifications include a researcher twice if he has a joint appointment at two universities (This occurs very rarely. Estimating the department and specialization level with weights to account for the few researchers who are appointed at two departments does not alter those results). The number of researchers in the two sets of regressions, however, is exactly the same as can be seen from the number of included researchers.

show the results for physics. The coefficient on the number of dismissed coauthors is not significantly different from 0. The coefficient on the average quality of dismissed coauthors, however, in column (2) shows that losing a coauthor of average quality reduces the productivity of a physicist of average quality by about 12.5 percent. The results for chemists are reported in columns (3) and (4). The number of dismissed coauthors does not seem to play an important role for the productivity of chemists. The average quality of the dismissed coauthors is, however, highly significant. The estimated coefficient for citation weighted publications indicates that losing a coauthor of average quality reduces the productivity of the average chemist by about 16.5 percent. The regressions reported in Table 1.14 use the total number of publications and citations weighted publications as dependent variable. A coauthored publication is counted as a full publication for both coauthors. Another approach is to normalize joint publications by dividing each publication and the citations of each publication by the number of coauthors. Table 1.15 shows the results obtained when using normalized (citation weighted) publications as the dependent variable. The results are very similar to before.

These results show that scientists who lost high quality coauthors suffered more than scientists who lost less prolific coauthors. The fact that I do not find a significant effect on the number of dismissed coauthors suggests that this effect is not driven by the fact that researchers who lost a coauthor published less because they were lamenting the loss of a coauthor.

The effect of losing a coauthor may depend on the time span which elapsed since the last collaboration. The regressions reported in Table 1.16 explore this in further detail. I split the dismissed coauthors into two groups; recent coauthors who had collaborated with a stayer between 1929 and 1932, and former coauthors who had co-written papers with the stayer between 1924 and 1928 and not thereafter. As expected the estimates indicate that only the dismissal of recent coauthors matter for a stayer's productivity. The dismissal of a former coauthor does not affect the productivity of the stayers.

As mentioned above it is not clear whether one would call the joint publication of papers a real peer effect. I therefore investigate how the dismissal affected the number of publications excluding joint publications with the dismissed coauthors. Finding a negative effect of the dismissal on the publications without the dismissed coauthors would suggest the presence of peer effects among coauthors which are more subtle than coauthoring. This is even more true as one would expect that researchers who lose a coauthor substitute towards single-authored publications and publications with other coauthors. This latter effect should reduce any dismissal effect. The results on publications without the dismissed coauthors are reported in Table 1.17. As before the number of dismissed coauthors does not affect the productivity of scientists. The quality of the dismissed coauthors, however, remains negative and significant. These results suggest the presence of peer effects between coauthors.

1.9 Conclusion

This chapter uses the dismissal of scientists by the Nazi government to identify peer effects in science. I use a newly constructed dataset to estimate a peer effects model including the number of peers and their average quality as determinants of a researcher's productivity. I do not find evidence for peer effects among researchers in the same department. Furthermore, I do not find evidence for peer effects among researchers of the same specialization within the same department. These results are very similar for physicists, chemistry, and mathematics and robust to a number of sensitivity checks.

I also investigate peer effects among coauthors. The number of coauthors does not matter for a researcher's productivity. The quality of coauthors, however, is important for the productivity of physicists and chemists. I find that losing a coauthor of average quality reduces the productivity of an average scientists by 12.5 percent in physics and by 16.5 percent in chemistry. I also show that the loss of coauthors lead to a reduction in the publications excluding the joint publications with the dismissed coauthor. This evidence suggests that there are peer effects between coauthors more subtle than the joint production of research papers.

It is important to note that these results do not mean that being at a good university does not have a positive effect on a researcher's productivity. The regressions reported above include university fixed effects which control for unobserved differences in the quality of laboratories, research seminars, research students, and the like. University quality does matter as the joint significance of the university fixed effects suggests. There is, however, no evidence for peer effects at the university or specialization level.

The evidence in this chapter comes from scientists in Germany from 1925 to 1938. It is quite likely that department level and within department specialization level peer interactions are even less important nowadays as communication and transportation costs have fallen dramatically since then. Furthermore, it is quite likely that my estimates of peer effects among coauthors constitute a lower bound as coauthored papers have become more and more important due to increased specialization and the increased importance of 'big science' projects. See Wuchty et al. (2007) for a description of the increased importance of teams in scientific research.

These results suggest some strong policy conclusions. Just co-locating researchers in order to increase their productivity through spillovers does not seem a useful policy. What seems much more important is to increase the possibility for coauthorships by fostering the mobility of researchers and their exposure to meeting researchers with similar research interests.

1.10 Tables

Table 1.1: Number of Dismissed Scientists across different Subjects

	Physics		Chemistry		Mathematics	
Year of Dismissal	Number of Dismissals	% of all Physicists in 1933	Number of Dismissals	% of all Chemists in 1933	Number of Dismissals	% of all Mathematicians in 1933
1933	34	11.8	51	10.9	35	15.6
1934	6	2.1	11	2.4	6	2.7
1935	4	1.4	5	1.1	5	2.2
1936	1	0.3	7	1.5	1	0.4
1937	1	0.3	3	0.6	2	0.9
1938	1	0.3	4	0.9	1	0.4
1939	1	0.3	2	0.4	1	0.4
1940	1	0.3	0	0.0	1	0.4
1933 - 1934	40	13.9	62	13.3	41	18.3

Table 1.2: Dismissals across different Universities

	Physics						Chemistry						Mathematics					
	Fall in # of Peers			Loss to Peer Quality			Fall in # of Peers			Loss to Peer Quality			Fall in # of Peers			Loss to Peer Quality		
	Scientists 1933	Dismissed 1933-34		Quality 1933	Fall in Peer Quality		Scientists 1933	Dismissed 1933-34		Quality 1933	Fall in Peer Quality		Scientists 1933	Dismissed 1933-34		Quality 1933	Fall in Peer Quality	
University		#	in %		#	in %		#	in %		#	in %		#	in %		#	in %
Aachen TU	3	0	0	2.53	0	0	12	2	16.7	6.24	-0.58	-9.3	7	3	42.9	0.10	-0.03	-33.3
Berlin	38	8	21.1	6.48	4.52	69.8	45	15	33.3	26.03	6.06	23.3	13	5	38.5	5.74	4.17	72.7
Berlin TU	21	6	28.6	8.16	1.40	17.2	41	13	31.7	13.38	3.39	25.4	14	2	14.3	0.07	-0.01	-18.2
Bonn	12	1	8.3	1.03	-0.10	-9.8	16	1	6.3	12.03	2.28	19.0	7	1	14.3	0.75	-0.10	-13.3
Braunschweig TU	4	0	0	0.97	0	0	8	0	0	6.26	0	0	3	0	0	0.00	0	0
Breslau	12	2	16.7	3.22	-0.11	-3.6	10	1	10.0	21.33	5.18	24.3	6	3	50.0	2.74	2.74	100.0
Breslau TU	1	0	0	5.63	0	0	14	2	14.3	30.68	4.06	13.2	5	2	40.0	2.76	2.23	80.7
Darmstadt TU	9	1	11.1	10.49	-0.26	-2.5	18	5	27.8	2.23	1.26	56.6	9	1	11.1	0.33	-0.04	-12.5
Dresden TU	6	1	16.7	8.16	8.16	100.0	17	1	5.9	18.56	14.48	78.0	10	0	0	0.65	0	0
Erlangen	4	0	0	0.41	0	0	8	0	0	9.02	0	0	3	0	0	4.29	0	0
Frankfurt	12	1	8.3	1.43	0.10	6.8	18	5	27.8	15.51	-4.98	-32.1	8	1	12.5	2.35	-0.18	-7.8
Freiburg	8	0	0	1.09	0	0	15	3	20.0	15.79	-2.36	-15.0	9	1	11.1	1.10	0.35	31.5
Giessen	5	1	20.0	1.67	1.58	94.9	10	0	0	4.22	0	0	7	1	14.3	0.33	-0.05	-16.7
Göttingen	21	9	42.9	14.59	10.58	72.5	17	0	0	37.08	0	0	17	10	58.8	3.44	2.34	67.9
Greifswald	6	0	0	5.30	0	0	5	0	0	23.09	0	0	3	0	0	2.33	0	0
Halle	4	0	0	2.36	0	0	9	1	11.1	12.25	-1.64	-13.4	7	1	14.3	0.96	-0.04	-3.7
Hamburg	11	2	18.2	0.40	-0.03	-8.3	11	2	18.2	6.32	-0.25	-4.0	8	0	0	1.65	0	0
Hannover TU	3	0	0	0.00	0	0	14	0	0	29.19	0	0	6	0	0	0.28	0	0
Heidelberg	8	0	0	2.53	0	0.0	18	1	5.6	59.47	-3.82	-6.4	5	1	20.0	2.02	-0.50	-25.0
Jena	13	1	7.7	4.66	-0.38	-8.1	10	0	0	31.82	0	0	5	0	0	0.00	0	0
Karlsruhe TU	8	0	0	2.10	0	0	14	4	28.6	32.66	-6.58	-20.1	6	1	16.7	0.00	0	0
Kiel	8	1	12.5	1.00	0.08	8.3	11	0	0	9.10	0	0	5	2	40.0	0.63	-0.29	-46.7
Köln	8	1	12.5	10.14	-1.41	-13.9	4	1	25.0	10.88	6.42	59.0	6	2	33.3	0.41	-0.23	-56.4
Königsberg	8	0	0	5.76	0.00	0.0	11	1	9.1	6.69	3.62	54.1	5	2	40.0	4.90	1.53	31.1
Leipzig	11	2	18.2	3.67	-0.23	-6.2	24	2	8.3	18.67	0.29	1.6	8	2	25.0	1.48	0.49	33.1
Marburg	6	0	0	2.10	0	0	8	0	0	15.74	0	0	8	0	0	0.17	0	0
München	12	3	25.0	9.18	-3.06	-33.3	18	1	5.6	10.80	0.79	7.3	9	0	0	1.89	0	0
München TU	10	1	10	0.95	-0.11	-11.1	15	0	0	7.41	0	0	5	0	0	0.20	0	0
Münster	5	0	0	1.15	0	0	12	0	0	9.95	0	0	5	0	0	0.85	0	0
Rostock	3	0	0	1.49	0	0	8	0	0	9.74	0	0	2	0	0	0.08	0	0
Stuttgart TU	5	0	0	2.36	0	0	9	1	11.1	9.19	-1.53	-16.6	6	0	0	0.04	0	0
Tübingen	2	0	0	7.54	0	0	10	0	0	7.59	0	0	6	0	0	1.73	0	0
Würzburg	3	0	0	0.00	0	0	11	0	0	10.34	0	0	4	0	0	0.03	0	0

Table 1.3: Characteristics of Dismissed Scholars

	Physics				Chemistry				Mathematics			
	All	Stay-ers	Dismissed 33-34		All	Stay-ers	Dismissed 33-34		All	Stay-ers	Dismissed 33-34	
			#	% Loss			#	% Loss			#	% Loss
Researchers (Beginning of 1933)	287	248	39	13.6	466	405	61	13.1	224	183	41	18.3
# of Chaired Profs.	109	97	12	11.0	156	136	20	12.8	117	99	18	15.4
Average Age (1933)	49.5	50.2	45.1	-	50.4	50.5	49.7	-	48.7	50.0	43.0	-
# of Nobel Laureates	15	9	6	40.0	14	11	3	21.4	-	-	-	-
Avg. publications (1925-1932)	0.47	0.43	0.71	20.5	1.69	1.59	2.31	17.9	0.33	0.27	0.56	31.1
Avg. publications (citation weighted)	5.1	3.5	14.8	39.4	17.3	16.1	25.1	19.0	1.45	0.93	3.71	46.8
% Publ. coauthored	33.3	33.6	31.6	-	76.0	75.8	77.1	-	11.3	9.7	14.8	-
% Publ. coauthored (Coaut. in Germany)	10.6	9.9	13.9	-	11.7	12.1	9.7	-	6.3	5.9	6.7	-
% Publ. coauthored (Coaut. same uni)	4.2	3.4	8.7	-	5.1	5.4	3.8	-	2.7	2.0	4.1	-

Table 1.4: Top Journals

Journal Name	Published in
General Journals	
Naturwissenschaften	Germany
Sitzungsberichte der Preussischen Akademie der Wissenschaften Physikalisch Mathematische Klasse	Germany
Nature	UK
Proceedings of the Royal Society of London A (Mathematics and Physics)	UK
Science	USA
Physics	
Annalen der Physik	Germany
Physikalische Zeitschrift	Germany
Physical Review	USA
Chemistry	
Berichte der Deutschen Chemischen Gesellschaft	Germany
Biochemische Zeitschrift	Germany
Journal für Praktische Chemie	Germany
Justus Liebigs Annalen Chemie	Germany
Kolloid Zeitschrift	Germany
Zeitschrift für Anorganische Chemie und Allgemeine Chemie	Germany
Zeitschrift für Elektrochemie und Angewandte Physikalische Chemie	Germany
Zeitschrift für Physikalische Chemie	Germany
Journal of the Chemical Society	UK
Mathematics	
Jounal für die reine und angewandte Mathematik	Germany
Journal of the London Mathematical Society	Germany
Mathematische Annalen	Germany
Mathematische Zeitschrift	Germany
Zeitschrift für angewandte Mathematik und Mechanik	Germany
Acta Mathematica	Sweden
Proceedings of the London Mathematical Society	UK

Another major journal for physicists at the time was the "Zeitschrift für Physik". Unfortunately, the Web of Science does not include the articles in that journal after 1927. Therefore, I exclude the "Zeitschrift für Physik" from the analysis.

Table 1.5: Reduced Form (Department Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Physics				Chemistry				Mathematics			
Dependent Variable:	Publications	Publications	Citation Weighted Pub.	Citation Weighted Pub.	Publications	Publications	Citation Weighted Pub.	Citation Weighted Pub.	Publications	Publications	Citation Weighted Pub.	Citation Weighted Pub.
Number Dismissed	-0.018	-0.022	-0.220	-0.369	-0.019	-0.017	-0.176	-0.106	-0.022	-0.022	0.009	0.027
	(0.014)	(0.018)	(0.336)	(0.378)	(0.008)*	(0.009)	(0.213)	(0.205)	(0.017)	(0.017)	(0.182)	(0.161)
Dismissal Induced ↓ in Peer Quality	0.026	0.028	0.546	0.653	0.020	0.016	0.782	0.718	0.030	0.035	-0.441	-0.266
	(0.013)	(0.015)	(0.320)	(0.367)	(0.013)	(0.011)	(0.471)	(0.466)	(0.036)	(0.041)	(0.279)	(0.333)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE		✓		✓		✓		✓		✓		✓
Observations	2261	2261	2261	2261	3584	3584	3584	3584	1538	1538	1538	1538
# of researchers	258	258	258	258	413	413	413	413	183	183	183	183
R-squared	0.39	0.39	0.25	0.27	0.67	0.67	0.53	0.54	0.32	0.33	0.2	0.21

**significant at 1% level *significant at 5% level (All standard errors clustered at department level)

Publications are the sum of a scientist's publications in top journals in one year.

Citation Weighted Publications are defined as the sum of subsequent citations (in the first 50 years after publication in any journal included in the "Web of Science", including international journals) of all articles published in a given year.

Number dismissed is equal to the number of dismissed scientists in a researcher's department. The variable is 0 until 1933 for researchers in all departments. In 1934 it is equal to the number of dismissals in 1933 at a researcher's department. From 1935 onwards it is equal to the number of dismissals in 1933 and 1934 in a researcher's department.

Dismissal induced ↓ in Peer Quality is 0 for all researchers until 1933. In 1934 it is equal to (Avg. quality of total department before dismissal) - (Avg. quality of researchers not dismissed in 1933). From 1935 onwards it will be equal to (Avg. quality of total department before dismissal) - (Avg. quality of researchers not dismissed in 1933 and 1934). Researchers in departments with stayers of *below* department level average quality have a positive value of the quality dismissal variable. The quality dismissal variable is negative for scientists in departments with stayers of *above* average quality. Average quality is measured as the department level average of citation weighted publications between 1925 and 1932.

Table 1.6: Reduced Form (Specialization Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Physics				Chemistry				Mathematics			
Dependent Variable:	Publications	Publications	Citation Weighted Pub.	Citation Weighted Pub.	Publications	Publications	Citation Weighted Pub.	Citation Weighted Pub.	Publications	Publications	Citation Weighted Pub.	Citation Weighted Pub.
Number Dismissed	0.021	0.012	0.641	0.499	-0.011	-0.007	0.837	1.055	-0.034	-0.034	0.272	0.349
	(0.026)	(0.027)	(0.423)	(0.419)	(0.047)	(0.048)	(0.869)	(0.840)	(0.035)	(0.035)	-0.369	(0.375)
Dismissal Induced	0.007	0.006	0.163	0.160	0.006	0.006	-0.004	-0.023	0.020	0.027	-0.556	-0.537
↓ in Peer Quality	(0.018)	(0.021)	(0.319)	(0.335)	(0.008)	(0.008)	(0.098)	(0.096)	(0.032)	(0.032)	(0.262)*	(0.250)*
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE		✓		✓		✓		✓		✓		✓
Observations	2257	2257	2257	2257	3567	3567	3567	3567	1538	1538	1538	1538
# of researchers	256	256	256	256	405	405	413	405	183	183	183	183
R-squared	0.38	0.39	0.25	0.27	0.67	0.67	0.53	0.54	0.32	0.33	0.2	0.21

**significant at 1% level *significant at 5% level (All standard errors are clustered at the department level)

Number dismissed is equal to the number of dismissed scientists within the same *specialization* as the researcher (e.g. it will be equal to the number of dismissed theoretical physicists at a researcher's department for a theoretical physicist). The variable is 0 until 1933 for all researchers. In 1934 it is equal to the number of dismissals in 1933 at a researcher's specialization. From 1935 onwards it is equal to the number of dismissals in 1933 and 1934 in a researcher's specialization.

Dismissal induced ↓ in Peer Quality is 0 for all researchers until 1933. In 1934 it is equal to (Avg. quality of *all* researchers within a specialization in the scientist's department before dismissal) - (Avg. quality of researchers within a specialization in a scientist's not dismissed in 1933). From 1935 onwards it is equal to (Avg. quality of *all* researchers within a specialization in the scientist's department before dismissal) - (Avg. quality of researchers within a specialization in a scientist's not dismissed in 1933 or 1934). Average quality is measured as the specialization level average of citation weighted publications between 1925 and 1932.

Table 1.7: First Stages (Department Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)
	Physics		Chemistry		Mathematics	
Dependent Variable:	Department Size	Avg. Quality of Peers	Department Size	Avg. Quality of Peers	Department Size	Avg. Quality of Peers
Number Dismissed	-0.566	0.180	-0.964	-0.077	-0.540	0.013
	(0.110)**	(0.090)	(0.101)**	(0.223)	(0.063)**	(0.031)
Dismissal Induced	-0.054	-0.919	0.011	-1.117	0.188	-1.299
↓ in Peer Quality	(0.111)	(0.079)**	(0.082)	(0.138)**	(0.197)	(0.181)**
Age Dummies	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓
Observations	2261	2261	3584	3584	1538	1538
# of researchers	258	258	413	413	183	183
R-squared	0.93	0.77	0.94	0.76	0.86	0.78
F - Test on Instruments	85.9	231.1	46.0	33.3	84.8	61.5

Table 1.8: Instrumental Variables (Department Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Physics				Chemistry				Mathematics			
Dependent Variable:	Publications		Cit.Weighted Pub.		Publications		Cit.Weighted Pub.		Publications		Cit.Weighted Pub.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Department Size	-0.003	0.028	-0.196	0.419	-0.011	0.019	0.044	0.161	0.008	0.040	-0.004	-0.044
	(0.005)	(0.029)	(0.116)	(0.514)	(0.008)	(0.011)	(0.266)	(0.288)	(0.013)	(0.032)	(0.080)	(0.300)
Peer Quality	0.011	-0.032	0.018	-0.736	0.007	-0.014	0.107	-0.641	0.009	-0.021	0.507	0.198
	(0.007)	(0.020)	(0.097)	(0.395)	(0.003)*	(0.010)	(0.065)	(0.389)	(0.018)	(0.029)	(0.210)*	(0.257)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2261	2261	2261	2261	3584	3584	3584	3584	1538	1538	1538	1538
# of researchers	258	258	258	258	413	413	413	413	183	183	183	183
R-Squared	0.40		0.27		0.67		0.54		0.33		0.21	
Cragg-Donald EV Statistic		36.37		36.37		189.49		189.49		56.08		56.08

**significant at 1 percent level *significant at 5 percent level (All standard errors clustered at the department level)

Table 1.9: First Stages (Specialization Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)
	Physics		Chemistry		Mathematics	
Dependent Variable:	Department Size	Avg. Quality of Peers	Department Size	Avg. Quality of Peers	Department Size	Avg. Quality of Peers
Number Dismissed	-0.817	0.330	-0.987	0.143	-0.419	-0.201
	(0.139)**	(0.203)	(0.108)**	(1.370)	(0.159)*	(0.162)
Dismissal Induced	0.059	-0.869	0.011	-1.096	-0.114	-0.866
↓ in Peer Quality	(0.042)	(0.206)**	(0.015)	(0.146)**	(0.110)	(0.193)**
Age Dummies	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓
Observations	2257	2257	3567	3567	1538	1538
# of researchers	256	256	405	405	183	183
R-squared	0.92	0.70	0.92	0.73	0.88	0.77
F - Test on Instruments	18.3	19.4	42.2	40.8	22.3	60.0

Table 1.10: Instrumental Variables (Specialization Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Physics				Chemistry				Mathematics			
Dependent Variable:	Publications		Cit.Weighted Pub.		Publications		Cit.Weighted Pub.		Publications		Cit.Weighted Pub.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
# of Peers in Specialization	-0.006	-0.018	-0.353	-0.704	-0.054	0.006	-0.539	-1.067	-0.001	0.101	-0.171	-1.205
	(0.017)	(0.028)	(0.249)	(0.494)	(0.029)	(0.046)	(0.376)	(0.850)	(0.017)	(0.126)	(0.167)	(1.423)
Peer Quality (Specialization)	0.007	-0.008	-0.039	-0.231	-0.002	-0.005	0.031	0.010	0.007	-0.045	0.689	0.778
	(0.004)	(0.024)	(0.063)	(0.414)	(0.002)	(0.007)	(0.039)	(0.091)	(0.015)	(0.059)	(0.331)*	(0.505)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2257	2257	2257	2257	3567	3567	3567	3567	1538	1538	1538	1538
# of researchers	256	256	256	256	405	405	405	405	183	183	183	183
R-Squared	0.40		0.27		0.67		0.54		0.33		0.22	
Cragg-Donald EV Statistic		116.53		116.53		206.12		206.12		8.56		8.56

**significant at 1 percent level *significant at 5 percent level (All standard errors clustered at the department level)

Table 1.11: Robustness Checks Instrumental Variables Physics (Department Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Full Sample	Full Sample	omitting 33 & 34	omitting 33 & 34	younger than 45	45 or older	younger than 45	45 or older	Full Sample	Full Sample
Dependent Variable	Publi-cations	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.	Publi-cations	Publi-cations	Cit. weight. Publ.	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.
Department Size	0.028	0.419	0.003	0.356	0.113	-0.015	1.887	-0.586	0.068	0.545
	(0.029)	(0.514)	(0.029)	(0.812)	(0.068)	(0.039)	(0.980)	(0.529)	(0.042)	(0.612)
Peer Quality	-0.032	-0.736	-0.018	-0.956	-0.067	-0.005	-1.030	-0.243	-0.039	-0.509
	(0.020)	(0.395)	(0.018)	(0.752)	(0.047)	(0.029)	(0.703)	(0.330)	(0.030)	(0.349)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University specific. Time Trends									✓	✓
Observations	2261	2261	1866	1866	840	1421	840	1421	2261	2261
# of researchers	258	258	256	256	138	179	138	179	258	258
Cragg-Donald EV Statistic	36.37	36.37	18.80	18.80	10.11	30.88	10.11	30.88	24.36	24.36

Table 1.12: Robustness Checks Instrumental Variables Chemistry (Department Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Full Sample	Full Sample	omitting 33 & 34	omitting 33 & 34	younger than 45	45 or older	younger than 45	45 or older	Full Sample	Full Sample
Dependent Variable	Publi-cations	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.	Publi-cations	Publi-cations	Cit. weight. Publ.	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.
Department Size	0.019	0.161	0.023	0.246	-0.006	0.024	0.325	0.153	0.014	0.239
	(0.011)	(0.288)	(0.015)	(0.197)	(0.040)	(0.015)	(1.305)	(0.151)	(0.029)	(0.525)
Peer Quality	-0.014	-0.641	-0.010	-0.187	-0.018	-0.013	-1.650	-0.168	0.019	-1.080
	(0.010)	(0.389)	(0.010)	(0.170)	(0.027)	(0.016)	(1.398)	(0.219)	(0.031)	(1.011)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University specific. Time Trends									✓	✓
Observations	3584	3584	2926	2926	1308	2276	1308	2276	3584	3584
# of researchers	413	413	411	411	206	295	206	295	413	413
Cragg-Donald EV Statistic	189.49	189.49	167.87	167.87	46.81	145.75	46.81	145.75	41.69	41.69

**significant at 1 percent level *significant at 5 percent level

Table 1.13: Robustness Checks Instrumental Variables Mathematics (Department Level Peers)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Full Sample	Full Sample	omitting 33 & 34	omitting 33 & 34	younger than 45	45 or older	younger than 45	45 or older	Full Sample	Full Sample
Dependent Variable	Publi-cations	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.	Publi-cations	Publi-cations	Cit. weight. Publ.	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.
Department Size	0.040	-0.044	0.066	0.046	0.066	-0.014	-0.160	0.061	0.005	-0.109
	(0.032)	(0.300)	(0.053)	(0.483)	(0.053)	(0.049)	(0.385)	(0.496)	(0.028)	(0.244)
Peer Quality	-0.021	0.198	-0.038	0.254	-0.092	0.030	-0.442	0.048	0.018	0.108
	(0.029)	(0.257)	(0.035)	(0.355)	(0.081)	(0.028)	(1.147)	(0.274)	(0.034)	(0.425)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University specific. Time Trends									✓	✓
Observations	1538	1538	1256	1256	672	866	672	866	1538	1538
# of researchers	183	183	183	183	106	119	106	119	183	183
Cragg-Donald EV Statistic	56.08	56.08	17.68	17.68	23.99	24.9	23.99	24.9	54.03	54.03

**significant at 1 percent level *significant at 5 percent level (All standard errors clustered at the department level)

Table 1.14: Effect of Dismissal on Coauthors

	(1)	(2)	(3)	(4)
	Physics		Chemistry	
Dependent Variable	Publi-cations	Citation Weighted Pub.	Publi-cations	Citation Weighted Pub.
# of Dismissed Coauthors	0.328	7.668	0.398	-1.020
	(0.525)	(7.888)	(0.366)	(5.766)
Avg. Quality of Dism. Coauthors	-0.007	-0.125	-0.013	-0.165
	(0.003)*	(0.045)**	(0.003)**	(0.036)**
Age Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
University FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Observations	2243	2243	3575	3575
# of researchers	258	258	413	413
R-squared	0.40	0.27	0.67	0.54

**significant at 1% level *significant at 5% level
(All standard errors are clustered at the individual level)

Table 1.15: Coauthors: Normalized Publications

	(1)	(2)	(3)	(4)
	Physics		Chemistry	
Dependent Variable	Publi-cations	Citation Weighted Pub.	Publi-cations	Citation Weighted Pub.
# of Dismissed Coauthors	0.574	8.441	0.268	-0.652
	(0.553)	(7.209)	(0.188)	(3.688)
Avg. Quality of Dism. Coauthors	-0.006	-0.100	-0.008	-0.086
	(0.003)	(0.042)*	(0.002)**	(0.026)**
Age Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
University FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Observations	2243	2243	3575	3575
# of researchers	258	258	413	413
R-squared	0.39	0.26	0.68	0.49

**significant at 1% level *significant at 5% level
(All standard errors are clustered at the individual level)

Table 1.16: Coauthors: Timing of Coauthorship

	(1)	(2)	(3)	(4)
	Physics		Chemistry	
Dependent Variable	Publi-cations	Citation Weighted Pub.	Publi-cations	Citation Weighted Pub.
Coauthors 1929 - 1932				
# of Dismissed Coauthors	0.322	7.990	0.048	-8.156
	(0.572)	(7.889)	(0.619)	(11.506)
Avg. Quality of Dism. Coauthors	-0.007	-0.123	-0.012	-0.156
	(0.003)*	(0.039)**	(0.003)**	(0.050)**
Coauthors 1924 - 1928 (not later)				
# of Dismissed Coauthors	-0.011	-2.214	-0.005	-0.087
	(0.959)	(23.495)	(0.405)	(4.609)
Avg. Quality of Dism. Coauthors	0.007	0.108	0.003	0.064
	(0.019)	(0.437)	(0.004)	(0.064)
Age Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
University FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Observations	2243	2243	3575	3575
# of researchers	258	258	413	413
R-squared	0.40	0.27	0.67	0.54

**significant at 1% level *significant at 5% level
(All standard errors are clustered at the individual level)

Table 1.17: Coauthors: Publications without dismissed Coauthors

	(1)	(2)	(3)	(4)
	Physics		Chemistry	
Dependent Variable	Publi-cations	Citation Weighted Pub.	Publi-cations	Citation Weighted Pub.
# of Dismissed Coauthors	0.458	10.777	0.558	-1.523
	(0.553)	(9.978)	(0.360)	(8.019)
Avg. Quality of Dism. Coauthors	-0.007	-0.144	-0.012	-0.290
	(0.003)*	(0.058)*	(0.003)**	(0.048)**
Age Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
University FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Observations	2243	2243	3575	3575
# of researchers	258	258	413	413
R-squared	0.39	0.28	0.67	0.53

**significant at 1% level *significant at 5% level
(All standard errors are clustered at the individual level)

1.11 Appendix for Chapter 1

Physics

BEER, Dr. Arthur P., Researcher; b. 1900., married, 1 child. (English, French, Czech.) 1928/33: Researcher Universitätssternwarte, Breslau, and Deutsche Sternwarte, Hamburg. since 1934: Researcher Solar Physics Observatory, Cambridge University. SPEC.: *Astronomy; Astro- and Geo-Physics.* Temp.

BERG, Dr. Wolfgang, F., Assistant; b. 08., married. (English, French.) 1930/33: Assistant Physikalisches Institut, Berlin University; 1934/36: Researcher Physical Lab., Manchester University; since 1936: Industrial Activity, London. SPEC.: *Experimental Physics. Fluorescence of Atoms and Molecules; Structure and Deformation of Crystals; X-Ray Methods.* Temp.

BERGSTRÄSSER, Dr. Martin, Assistant; b. 02., married. (English, French.) 1927/33: Assistant Technische Hochschule, Dresden; 1933/34: Assistant Deutsche Versuchsanstalt für Luftfahrt, Berlin. SPEC.: *Technical Physics; Testing of Materials; Solidity; Mechanics.* Unpl.

BETHE, Dr. Hans, Privatdozent; b. 06., single. (English.) Till 1933: Privatdozent Göttingen University; 1934/35: Researcher Bristol University; since 1935: Cornell University, Ithaca (N.Y.). SPEC.: *Theoretical Physics. Quantum Mechanics.* Perm.

BIEL, Dr. Erwin, Privatdozent; b. 99., married, 1 child. (English, French, Italian.) Till 1929: Assistant Geographisches Institut, Vienna University; 1929/33: Climatologist Meteorologisches Observatorium, Breslau; 1932/33: Privatdozent Breslau University. SPEC.: *Geo-Physics; Climatology.* Unpl.

BLOCH, Dr. Felix, Privatdozent; b. 05., single. (English.) Till 1933: Privatdozent and Assistant Physikalisches Institut, Leipzig University; since 1933: Prof. Stanford University, California. SPEC.: *Theoretical Physics; Atomic Physics.* Perm.

BOAS, Dr. Walter, Assistant; b. 04., single. (English, French.) 1928/32: Researcher Kaiser Wilhelm Institut für Metallforschung, Berlin; 1933/35: Assistant Fribourg University; since 1936: Researcher Physikalisches Institut, Technische Hochschule, Zürich. SPEC.: *Technical Physics; Metallography; Plasticity and Structure of Metals; X-Rays.* Unpl.

BOEHM, Dr. Gundo, Assistant. Till 1933: Assistant Physikalisches Institut, Freiburg University. SPEC.: *Micellar Structure of Muscles.* Unpl.

BORN, Dr. Max, o. Professor; b. 82., married, 3 children. (English.) 1915/19: a.o. Prof. Berlin University; 1919/21: o. Prof. Frankfurt University; 1921/33: o. Prof. Göttingen University; 1933/35: Lecturer Cambridge University; since 1936: Prof. Edinburgh University. SPEC.: *Theoretical Physics; Quantum Theory; Atomic Structure; Optics; Mathematical Physics.* Perm.

BURSTYN, Dr. Walther, a.o. Professor; b. 77., married. (English, French.) 1920/33: a.o. Prof. Technische Hochschule, Berlin. SPEC.: *Technical Physics.* Unpl.

BYK, Dr. Alfred, a.o. Professor; b. 78., married, 2 children. (English, French, Italian, Dutch.) 1905: Privatdozent Technische Hochschule, Berlin; 1909/33: Privatdozent, later a.o. Prof. Berlin University and Technische Hochschule. SPEC.: *Mathematical Physics; Theoretical Electrotechnics; Quantum Theory; Boundaries of Physics and Chemistry.* Unpl.

COHN-PETERS, Dr. H. Jürgen, Researcher; b. 07. Till 1933: Researcher Berlin University; since 1934: U.S.S.R. SPEC.: *Experimental Physics. High Tension.* Perm.

DEMBER, Dr. Alexis, Assistant; b. 12., single. (English, French.) since 1935: Assistant Physical Institute, Istanbul University. SPEC.: *Electrolytes; Photoelectricity.* Temp.

DEMBER, Dr. Harry, o. Professor; b. 82., married, 2 children. (English, French, Spanish, Turkish.) 1909/33: Privatdozent, later o. Prof. Technische Hochschule, Dresden; and Director Physikalisches Institut; since 1933; o. Prof. Istanbul University and Director Physical Institute. SPEC.: *Cathode and X-Rays; Photo-Electricity; Atmospheric Optics; Atmospheric Electricity.* Perm.

DUSCHINSKY, Dr. F., Assistant; b. 07., single. (French, Italian, Spanish, Dutch.) 1933: Assistant Kaiser Wilhelm Institut für Physik, Berlin; since 1934: Assistant Brussels University. SPEC.: *Experimental Physics; Fluorescence; Molecular Spectra; Optics; High Frequency Technics.* Temp.

EHRENBERG, Dr. Werner, Assistant; b. 01., single. (English, French.) 1924/27: Assistant Kaiser Wilhelm Institut für Faserstoffchemie, Berlin; 1928/30: Researcher Berlin University and Technische Hochschule, Stuttgart; 1930/33: Assistant Technische Hochschule, Stuttgart; since 1935: Electric and Musical Industries, Ltd., Hayes (Middlesex). SPEC.: *Experimental Physics. X-Rays; Cathode Rays; Cosmic Radiation.* Perm.

EINSTEIN, Dr. Albert, o. Professor; b. 79., married. (English.) 1919/33: o. Prof. Berlin University and Director Kaiser Wilhelm Institut für Physik; 1921 Nobel Prize; since 1934: Prof. Institute for Advanced Study, Princeton (N.J.).

EISENSCHITZ, Dr. Robert, Researcher; b. 98., married. (English, French.) 1924/27: Researcher Allgemeine Elektrizitätsgesellschaft, Berlin; 1927/33: Researcher Kaiser Wilhelm Institut für Physikalische Chemie und Elektrochemie, Berlin; since 1934: Researcher Royal Institution, London. SPEC.: *Theoretical and Experimental Physics; Spectroscopy; Viscosity; Application of Physical Theories to Chemical Problems.* Temp.

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Figure 1.9: List of Displaced German Scholars: Sample Page

Squares were added by the author to highlight the researchers who had already received the Noble prize or were to receive it after 1936.

Table 1.18: Specializations

Physics		Chemistry		Mathematics	
Specialization	% scientists in speciali-zation	Specialization	% scientists in speciali-zation	Specialization	% scientists in speciali-zation
Experimental Physics	48.5	Organic Chemistry	26.6	Analysis	45.9
Theoretical Physics	22.3	Physical Chemistry	23.8	Applied Mathematics	36.2
Technical Physics	20.6	Technical Chemistry	19.4	Algebra	19.7
Astronomy	14.7	Inorganic Chemistry	18.6	Number Theory	13.5
		Pharmacology	10.2	Metha Mathematics	5.2
		Medical Chemistry	8.0	Topology	4.8
		Biochemistry	6.7	Foundations of Math.	4.4

Percentages add to more than 100 percent because some physicists and chemists have two specializations. Mathematicians have up to four specializations.

Table 1.19: Top Researchers 1925-1932 (Citation Weighted Publications Measure)

Name	University beginning of 1933	First Special-ization	Second Special-ization	Third Special-ization	Avg. Cit. weighted Publ.	Avg. Publ.	Nobel Prize	Dis-missed 33-34
Physics								
Fritz London	Berlin	Theo. Phy.			149.3	1.3		✓
Lother Nordheim	Göttingen	Theo. Phy.			110.0	0.7		✓
Gerhard Herzberg	Darmstadt TU	Exp. Phy.			78.0	2.0	✓	
Carl Ramsauer	Berlin TU	Exp. Phy.			75.6	3.0		
Max Born	Göttingen	Theo. Phy.			62.5	1.3	✓	✓
Hans Falkenhagen	Köln	Theo. Phy.			57.5	1.9		
Arnold Sommerfeld	München	Theo. Phy.			44.4	1.8		
Eugen Wigner	Berlin TU	Theo. Phy.			44.3	0.5	✓	✓
Heinrich Kuhn	Göttingen	Exp. Phy.	Theo. Phy.		42.0	4.0		✓
Harry Dember	Dresden TU	Exp. Phy.			40.8	1.0		✓
Karl Herzfeld		Theo. Phy.			33.7	1.3		
Richard Gans	Königsberg	Exp. Phy.			29.4	1.6		
Walter Gerlach	München	Exp. Phy.			29.1	3.1		
Wolfgang Pauli		Theo. Phy.			28.0	3.8	✓	
Max Wien	Jena	Exp. Phy.			25.4	2.0		
Werner Heisenberg	Leipzig	Theo. Phy.			25.3	1.0	✓	
Ludwig Prandtl	Göttingen	Tech. P.			23.3	1.1		
Fritz Kirchner	München	Exp. Phy.			22.5	2.5		
Johannes Malsch	Köln	Exp. Phy.			22.0	1.5		
Emil Rupp	Berlin TU	Exp. Phy.			21.4	5.2		✓
Chemistry								
Werner Kuhn	Karlsruhe TU	Physical			262.0	7.0		
Max Bergmann	Dresden TU	Organic	Bioc.		250.2	6.8		✓
Karl Lohmann	Heidelberg	Medical			224.0	6.0		
Ernst Bergmann	Berlin	Physical			223.3	17.0		✓
Carl Neuberg	Berlin	Biochem.			184.9	15.1		
Carl Wagner	Jena	Physical			177.5	5.0		
Otto Meyerhof	Heidelberg	Medical			176.3	5.8	✓	
Otto Ruff	Breslau TU	Inorganic			133.4	7.2		
Wolfgang Ostwald	Leipzig	Inorganic			127.0	8.6		
Hermann Staudinger	Freiburg	Organic			126.8	8.5	✓	
Gustav Tammann	Göttingen	Physical.			118.4	19.0		
Michael Polanyi	Berlin TU	Physical.			116.8	5.6		✓
Max Volmer	Berlin TU	Physical.			114.0	4.2		
Karl Freudenberg	Heidelberg	Organic			111.8	7.0		
Ulrich Hofmann	Berlin TU	Inorganic	Physical		109.0	6.0		
Richard Johann Kuhn	Heidelberg	Physical	Medical		92.1	8.0	✓	
Max Trautz	Heidelberg	Physical			91.9	5.3		
Wilhelm Klemm	Hannover TU	Inorganic			91.4	5.2		
Mathematics								
Johann von Neumann	Berlin	Applied M	Foundation	Analysis	36.3	1.5		✓
Richard Courant	Göttingen	Analysis	Applied M		22.3	1.3		✓
Richard von Mises	Berlin	Applied M	Analysis		15.6	0.9		✓
Heinz Hopf		Algebra	Topology	Geometry	13.3	1.3		
Paul Epstein	Frankfurt	Geometry	Number T	Algebra	11.5	0.6		
Oskar Perron	München	Algebra	Analysis		10.6	1.5		
Willy Prager	Göttingen	Applied M			10.0	0.4		✓
Gabiel Szegö	Königsberg	Applied M	Geometry		9.4	1.4		✓
Werner Rogosinski	Königsberg	Number T	Analysis		9.1	0.6		
Wolfgang Krull	Erlangen	Algebra			8.9	1.4		
Erich Rothe	Breslau TU	Analysis	Applied M		8.0	1.0		✓
Hans Peterssonn	Hamburg	Number T	Analysis		8.0	2.0		
Adolf Hammerstein	Berlin	Number T	Analysis		8.0	0.5		
Alexander Weinstein	Breslau TU	Applied M			6.3	0.7		✓
Erich Kamke	Tübingen	Number T	Foundation	Analysis	6.3	0.8		
Hellmuth Kneser	Greifswald	Applied M	Analysis	Topology	6.3	0.6		
Bartel van der Waerden	Leipzig	Algebra	Geometry		5.8	1.8		
Max Müller	Heidelberg	Analysis			5.3	0.3		
Richard Brauer	Königsberg	Algebra			5.0	0.6		✓
Leon Lichtenstein	Leipzig	Analysis	Applied M		4.9	1.5		✓

University is missing for scientists who retired before 1933

Table 1.20: Probability of Being Chaired Professor

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Chaired Prof. Dummy	Physics		Chemistry		Mathematics	
Peer Group:	Department Level	Specialization Level	Department Level	Specialization Level	Department Level	Specialization Level
Number Dismissed	-0.000	-0.020	0.004	0.011	0.010	0.032
	(0.007)	(0.010)	(0.003)	(0.014)	(0.015)	(0.021)
Dismissal Induced	-0.009	0.000	0.001	0.002	-0.021	-0.035
$\downarrow$ in Peer Quality	(0.005)	(0.002)	(0.003)	(0.002)	(0.035)	(0.023)
Age Dummies	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Observations	2261	2257	3584	3567	1538	1538
# of researchers	258	256	413	405	183	183
R-squared	0.90	0.90	0.89	0.89	0.92	0.92

Table 1.21: Robustness Checks Instrumental Variables Physics (Specialization Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Full Sample	Full Sample	omitting 33 & 34	omitting 33 & 34	younger than 45	45 or older	younger than 45	45 or older	Full Sample	Full Sample
Dependent Variable	Publi-cations	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.	Publi-cations	Publi-cations	Cit. weight. Publ.	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.
Department Size	-0.018	-0.704	-0.044	-1.052	-0.013	-0.057	0.653	-1.847	-0.005	-0.430
	(0.028)	(0.494)	(0.033)	(0.610)	(0.052)	(0.022)*	(0.766)	(0.723)*	(0.039)	(0.588)
Peer Quality	-0.008	-0.231	-0.023	-0.471	0.041	-0.023	0.637	-0.357	-0.003	-0.212
	(0.024)	(0.414)	(0.029)	(0.434)	(0.037)	(0.030)	(0.586)	(0.602)	(0.025)	(0.454)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University specific. Time Trends									✓	✓
Observations	2257	2257	1863	1863	838	1419	838	1419	2257	2257
# of researchers	256	256	254	254	137	178	137	178	256	256
Cragg-Donald EV Statistic	116.53	116.53	61.68	61.68	24.19	89.61	24.19	89.61	131.53	131.53

**significant at 1 percent level *significant at 5 percent level (All standard errors clustered at the department level)

Table 1.22: Robustness Checks Instrumental Variables Chemistry (Specialization Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Full Sample	Full Sample	omitting 33 & 34	omitting 33 & 34	younger than 45	45 or older	younger than 45	45 or older	Full Sample	Full Sample
Dependent Variable	Publi-cations	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.	Publi-cations	Publi-cations	Cit. weight. Publ.	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.
Department Size	0.006	-1.067	0.024	-0.528	-0.065	0.040	-2.284	-0.179	-0.005	-2.016
	(0.046)	(0.850)	(0.046)	(0.683)	(0.138)	(0.048)	(3.051)	(0.472)	(0.083)	(1.031)
Peer Quality	-0.005	0.010	-0.005	0.055	0.005	-0.007	-0.051	0.024	-0.004	0.077
	(0.007)	(0.091)	(0.007)	(0.087)	(0.014)	(0.009)	(0.235)	(0.097)	(0.009)	(0.170)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University specific. Time Trends									✓	✓
Observations	3567	3567	2913	2913	1303	2264	1303	2264	3567	3567
# of researchers	405	405	404	404	203	290	203	290	405	405
Cragg-Donald EV Statistic	206.12	206.12	190.64	190.64	40.11	164.86	40.11	164.86	151.67	151.67

Table 1.23: Robustness Checks Instrumental Variables Mathematics (Specialization Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample	Full Sample	Full Sample	omitting 33 & 34	omitting 33 & 34	younger than 45	45 or older	younger than 45	45 or older	Full Sample	Full Sample
Dependent Variable	Publi-cations	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.	Publi-cations	Publi-cations	Cit. weight. Publ.	Cit. weight. Publ.	Publi-cations	Cit. weight. Publ.
Department Size	0.101	-1.205	-2.141	25.734	0.045	-0.083	-1.228	-1.781	-0.002	-1.381
	(0.126)	(1.423)	(32.147)	(392.778)	(0.121)	(0.303)	(0.984)	(3.641)	(0.117)	(1.214)
Peer Quality	-0.045	0.778	0.751	-8.832	-0.026	0.055	0.613	0.932	0.006	0.858
	(0.059)	(0.505)	(11.462)	(139.978)	(0.040)	(0.136)	(0.349)	(1.661)	(0.078)	(0.773)
Age Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
University specific. Time Trends									✓	✓
Observations	1538	1538	1256	1256	672	866	672	866	1538	1538
# of researchers	183	183	183	183	106	119	106	119	183	183
Cragg-Donald EV Statistic	8.56	8.56	0.021	0.021	16.38	1.81	16.38	1.81	12.93	12.93

**significant at 1 percent level   *significant at 5 percent level   (All standard errors clustered at the department level)

2 Studying Abroad and the Effect on International Labour Market Mobility

Mobilityle Introduction of ERASMUS

2.1 Introduction

International labour market migration has risen dramatically in the recent past, especially among university graduates. Lowell (2007), for example, shows an increase in the emigration rate of university graduates from about 4 percent in 1980 to about 7 percent in 2000 in developed countries. The increased demand for skilled labour and the importance of highly skilled individuals for innovation has induced many countries to implement policies geared to attracting skilled migrants from abroad (OECD, 2002). Understanding the determinants of migration is key to formulating such policies. While attention has traditionally focused on wage differentials, going back to Hicks (1932)[27], it is clear that individual characteristics play an important role in determining the skilled worker's propensity to migrate. One possible determinant which has received particular attention of policymakers over the past years is student mobility during tertiary education. In particular, it has been hypothesized that student mobility may act as a 'stepping stone' for later labour migration (Guellec & Cervantes, 2001). Numerous countries, including the United States, Japan, and the United Kingdom, attempt to attract highly skilled mobile workers through policies relating to student mobility programs (Guellec & Cervantes, 2001).These are based on the assumption that student mobility has a genuine effect on later labour market mobility. Despite the widespread belief in the link between studying abroad and international labour market mobility, empirical evidence is very limited. Establishing a causal link between studying abroad and labour market mobility later in life is a challenging task because students who decide to study abroad are in many ways different from students who undertake all of their education in their home country. The unobserved heterogeneity may also affect the decision of working abroad later in life. This may introduce a bias in OLS estimates of the effect of studying abroad on subsequent international labour migration decisions. In this chapter, we provide evidence on the *causal* effect of studying abroad on later labour market mobility by exploiting an exogenous change in student mobility: the introduction of the ERASMUS student exchange programme.

This programme has been devised by the European Union to foster student exchange in Europe. Introduced in 1987 it offers the possibility of studying in another European country for up to 12 months at very low cost. Different universities and different departments introduced the programme at very different times. We exploit the variation in scholarship availability

[27] For surveys on determinants of migration, see Greenwood (1975, 1985, 1997).

as a source of exogenous variation in a student's probability to study abroad. In order to ascertain a student's exposure to the ERASMUS programme we construct a unique data set, containing annual information on the number of exchange places for each subject at every German university. In order to assess the effect of studying abroad on international mobility later in life we merge this data to a survey of German university graduates. We first show that the ERASMUS programme has a strong impact on a student's probability of studying abroad. We then use the department level variation in international student exchange programs to identify the causal effect of studying abroad on the decision of working in a foreign country later in life. We find that studying abroad increases a person's probability of working abroad by about 15–20 percentage points. This result suggests a strong causal link between international labour market mobility and previous international mobility. Qualitative evidence suggests that besides career concerns soft factors such as interest in foreign cultures or living with a foreign partner are important determinants for the decision to work abroad, and we suggest that the effect of studying abroad may work through these channels.

There are some papers analysing the link between labour market mobility and previous mobility. Kodrzycki (2001) provides descriptive evidence on inter-state mobility in the US and links it to the preceding decision of attending college out of state.[28] Malamud & Wozniak (2006) study the effect of the decision to go to college on interregional mobility in the US. Using an instrumental variables approach to control for selection effects they find that attending college increases the probability of residing out of state later in life by about 20 percentage points. The link between *international* student mobility and the decision to work abroad after graduation has rarely been studied to date. One reason is data availability: Most surveys do not contain information on study abroad spells during a student's undergraduate career, and graduates who work abroad are generally not sampled in national surveys of the sending countries. The available evidence is based on surveys of students. Jahr and Teichler (2001) use data from a survey of European university graduates. They investigate the effect of studying abroad on later international labour market mobility without controlling for possible selection of formerly mobile students. They find that formerly mobile students are between 15 and 18 percentage points more likely to work in a foreign country after graduation. Dreher and Poutvaara (2005) investigate the role of student mobility in explaining aggregate migration flows in a cross-country panel study, focusing on migration to the United States. They find strong effects of previous period's number of foreign students on current period's number of migrants, indicating that a ten percent increase in the number of foreign students increases subsequent migration by around 0.5 percent.

The paper which is most closely related to our work is a study by Oosterbeek and Webbink (2006). They employ a regression discontinuity design to control for unobserved heterogeneity between internationally mobile and non-mobile students. Using data on talented Dutch university students they find that studying abroad increases the probability of living in a foreign country by about 50 percentage points. A key difference to our work is that they look at a

[28]She finds that individuals who attended college out of state are 54 percent more likely to live out-of-state five years after graduation. These results, however, cannot be interpreted as causal effects as she does not address the selection issues affecting mobility decisions.

small sample of particularly talented students, while we use a nationally representative survey of German university graduates. Another important difference is that Oosterbeek and Webbink investigate the effect of *postgraduate* studies abroad. Students pursuing a postgraduate degree abroad may remain in the receiving country while looking for work. Part of the effect they find may also be driven by the fact that some of the respondents abroad are still enrolled in higher education at the time of the survey. In contrast, in our work, the intervention is international mobility during the undergraduate career, after which students return to complete their degree in Germany. Thus, our research design allows us – and in fact forces us – to separate the two mobility investments (studying abroad and working abroad). The effect we find is therefore informative about the dynamic effects of earlier mobility investments.

This chapter presents evidence that previous educational mobility is a very important determinant of mobility later in life. We thus establish a causal link of previous mobility decision to mobility later in life. This highlights the importance of taking earlier mobility into account in economic modelling but also for policy decisions. The European Union, for example, tries to foster labour market mobility in the EU (see "Commission's Action Plan for skills and mobility" (2002)). Our research suggests that supporting international student mobility is a very successful policy instrument to foster labour market mobility later in life. Our results on the effect of the ERASMUS programme on the probability of studying abroad also show that exchange programs are indeed effective in promoting student mobility. This will be important to policy makers as they spend large public funds on these programs.

2.2 Data

We use data on German university graduates, which has been collected by the Higher Education Information System (HIS) institute. This survey is conducted to provide a nationally representative longitudinal sample of university graduates in Germany. A sample of university graduates has been drawn from cohorts graduating in the academic years 1988-89, 1992-93, 1996-97, and 2000-01. In the following, we will refer to these four cross-sections as graduate cohorts 1989, 1993, 1997, and 2001. Graduates in each cohort are surveyed twice. The first survey takes place about 12 months after graduation (the *Initial Survey*). The same individuals participate in a follow-up survey about 5 years after entering the labour market (*Follow-Up Survey*).[29] The following Figure 2.1 illustrates the timing of the different surveys.

[29] For the 2001 cohort, only the initial survey is available so far.

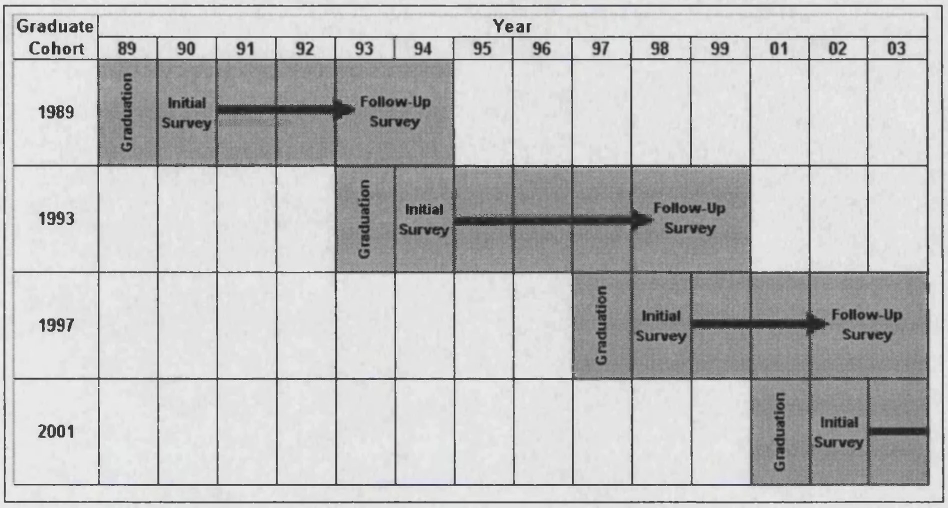

Figure 2.1: Graduate Cohorts in the Sample

The data contains detailed information on the students' background, study history, and labour market characteristics. This allows us to relate study decisions, in particular international educational mobility, to later labour market outcomes. A large advantage of this dataset lies in the fact that individuals graduating from a university in Germany are followed even if they move to a foreign country. This feature makes this dataset particularly valuable to investigate questions concerning international mobility.

The data and the sampling process is described in detail in Briedis & Minks (2004). The sample was drawn as follows: For each cohort, university-subject-degree combinations where sampled randomly, and the respective universities mailed the questionnaire to each student who had graduated within the corresponding academic year. This procedure ensures that the sample contains individuals from a large number of different institutions and subjects. One key advantage of the data is that the population of interest includes all university graduates who completed their studies during a given academic year at any institution of higher education in Germany. The higher education system in Germany consists of a number of different university types catering to different types of students. We include five main types of higher education institutions in our estimation. This includes not only the traditional universities *(Universitäten)* but also the so-called Universities of Applied Sciences *(Fachhochschulen)*, the Comprehensive Universities *(Gesamthochschulen)*, the Colleges of Art and Music *(Kunst- und Musikakademien)*, and the Theological Universities *(Theologische Hochschulen)*.[30] The response rate to the survey is around 25%. While of course a higher response rate would be desirable, an analysis conducted by the HIS has come to the conclusion that the characteristics of the survey respondents are close to those of the target population. The total number of respondents in our data corresponding to the four cohorts is 8,153 (1989), 6,737 (1993), 6,220 (1997), and 8,103 (2001).

The key information for our purposes is whether the student has studied abroad during her

[30] All institutions in our sample would be called universities in most countries outside Germany.

undergraduate studies, and whether the graduate works abroad at the time of the survey. We infer undergraduate mobility from the first question of the questionnaire, which asks the student to report her complete enrolment history. Respondents are instructed to report each change of degree programme or university. The questionnaire makes explicit reference to study abroad as one form of change in status in the 2001 survey. We use this information to construct an indicator of whether the student studied abroad during her undergraduate career. In order to exclude university mobility after finishing the first degree (e.g. to obtain a Master abroad), we only look at international mobility before the graduation date of the first degree. It is important to note that only students who obtain their degree in Germany are surveyed. We are, therefore, not able to observe students who first enrol in Germany and subsequently move to a foreign university and obtain their degree abroad. Also Germans who complete all of their higher education abroad are not included in our sample. These individuals may be different to students who study abroad as part of their degree in Germany. It is quite likely that those who complete their higher education abroad are even more likely to work in a foreign country after graduation than students who obtain their degree in Germany. If this was true we would underestimate the effect of studying abroad. Unfortunately, our data is not suitable to test this hypothesis.

For all students who have ever participated in the labour market, both the initial and the follow-up surveys contain questions about the current (or the last) employment, including the location of work. We infer from this question whether a former student now works in Germany or abroad, and create an indicator accordingly.

The following figure shows the percentages of studying abroad and working abroad (from the initial survey, one year after graduation) for the four graduation cohorts.

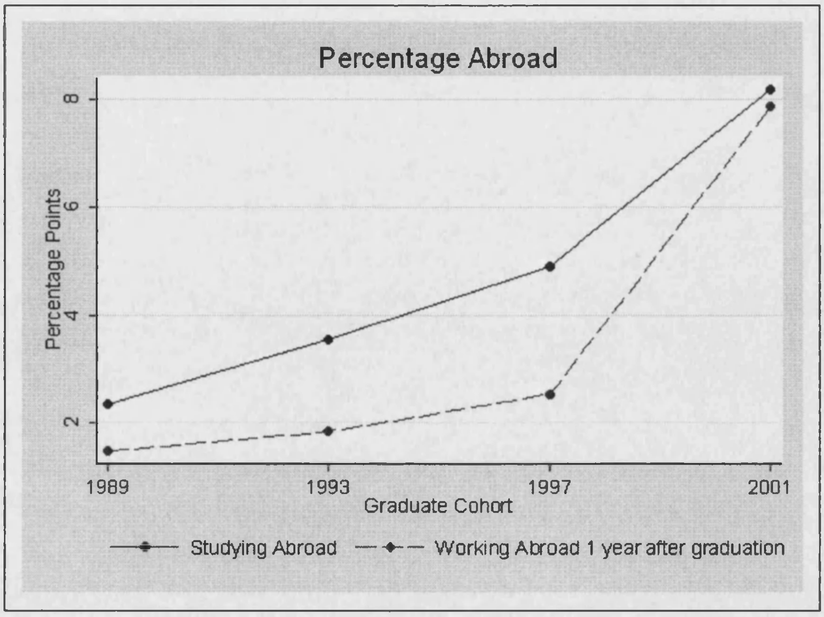

Figure 2.2: International Mobility in HIS Data

It can be seen that both studying abroad and working abroad occurs more frequently among students of later graduation cohorts. It is important to note that we include dummies for the four graduation cohorts in all our regressions. Therefore, we do not identify the effect of studying abroad from the overall time-trend in the two variables. In fact, in our sensitivity analysis, we show that our results are robust to allowing for not only a general time trend, but also for subject-specific time trends.

These percentages can be compared to information on international mobility from other data sources. Isserstedt & Schnitzler (2002) point out that different data sources use different ways to collect data and different definitions of a stay abroad. These differences may result in different estimates of student mobility. With this caveat in mind, we compare the incidence of international educational mobility in our data to data from the 16th Social Survey (*Sozialerhebung*), a large-scale survey of German students in 2000. Of all students surveyed in the Social Survey, about 13 percent of advanced students indicate that they spent part of their studies at a foreign university. While this number is larger than ours, it seems roughly comparable: The students interviewed in the Social Survey in 2000 will on average belong to a later graduate cohort than the academic year 2000/2001, which corresponds to our last cohort. Also, our definition relies on students spending at least one term at a foreign university. Thus, short term exchange will be included in the figure of the Social Survey, but not in ours. The figures from the Social Survey also replicate the strong over-time increase in the fraction of students who study abroad.

With similar caution we use data from the OECD Factbook 2006 to investigate the reliability of our data with respect to international labour market mobility. The OECD estimates that about 5.5 percent of Germans holding a university degree worked as expatriates in an OECD country in the year 2001. This number is lower than the percentage of people working abroad for the 2001 cohort in our dataset. This can be explained by the following two facts: First, the OECD calculates its estimate of expatriates by considering migration to the OECD countries only, while our number includes people working abroad anywhere in the world. Second, differences may also be driven by different methodologies of estimating outmigration: the OECD captures stocks of people abroad while we look at the outflow of graduates from a certain cohort.

We conclude that both the percentage of people studying abroad and the percentage of people working abroad in our data are comparable to estimates from other data sources. This is reassuring as there may be a worry that response rates to the HIS survey may differ for people living abroad. Unfortunately, there is no direct way of testing for differential response rates as we do not have any information on the individuals who do not respond to the HIS survey. One way of addressing this concern is to show that other data sources with different sampling frames exhibit similar numbers to our data.

In addition to the international mobility variables we also use a number of other control variables measured at the individual level. We create a measure of potential experience since graduation, defined as the number of months from graduation to the time of answering the questionnaire.[31]

[31] There is some variation in experience because students were sampled according to whether their graduation fell in a particular academic year. Students graduating at the beginning of the academic year therefore have more potential experience than those graduating towards the end of the year. In addition, there is some variation with

We take this measure of potential experience rather than actual labour market experience, because actual labour market experience could be affected by a study period abroad and might then be endogenous to our outcome. Other controls include a female indicator and an indicator for whether the student completed an apprenticeship before beginning her university studies. We also use variables which control for a students' earlier mobility decisions. In particular we include a variable which controls for whether the student's first university enrolment occurs in the state *(Bundesland)* where she obtained her final high school degree. Furthermore, we include the distance between the state of her university enrolment and the state where she obtained her high school degree.

We use a number of variables to control for a student's parental background. To control for parental education we use a variable that indicates the highest grade completed by either parent, where we split parental education into three categories to account for the characteristics of the education system in Germany. The omitted category contains students with parents who obtained up to 13 years of education. This group consists of students with parents who did not receive a school degree (very few), parents with lower types of secondary schooling *(Hauptschule or Realschule)* usually followed by an apprenticeship, and parents who obtained a high school degree but no further education (very few). The second group is comprised of students where the better educated parent either obtained an advanced craftsmanship degree *(Meister)* or some higher education, such as a degree from a university of applied science *(Fachhochschule)* but not a degree from a university. The third group includes students who have at least one parent holding a university degree.[32] We also construct indicator variables in five categories for each parent to control for parental occupation. As a proxy for credit constraints we use an indicator variable whether the student ever received federal financial assistance (BAFOEG). Students are eligible to this assistance if parental income is below a certain threshold. This threshold varies according to the number of children who are enrolled in a formal education programme.

In order to implement our Instrumental Variables strategy we combine the HIS graduate survey data with a unique dataset of ERASMUS participation. There is no readily available data on the ERASMUS exchange programme for our time period of interest. We obtained data on the number of ERASMUS scholarship holders for each year and each participating institution on a subject-by-subject basis from 1993/94 to 1999/2000 from The German Academic Exchange Service (DAAD). To obtain the data for the earlier years we proceeded as follows: The DAAD provided us with the number of scholarships allocated to each ERASMUS inter-university agreement (Inter-university Cooperation programme, ICP). We combined this information with published listings of all ICPs, which give details about the participating universities and the subjects covered for each inter-university agreement (see, for example, DAAD (1992)). This allows us to construct a panel data set at the university-subject-year level that covers the entire history of the ERASMUS programme in Germany. We use the following approach to establish when the typical student goes abroad: We compute the median academic year in which students go

respect to when the questionnaires were sent out and how quickly graduates responded.

[32] Using a linear years of parental education variable or controlling for mother's and father's education separately does not affect our results.

abroad, separately by graduate cohort, subject and university type (University, University of Applied Science, Comprehensive University, Theological University or College of Art). We then assign to each student the exposure to the ERASMUS programme in that academic year. This approach is preferable to simply assigning ERASMUS characteristics at a fixed point in the student's study period (say the first or second year): since our graduates are sampled when they exit university, and since there is substantial variation in length of studies, there might be a systematic relationship between individual study duration and other unobservable factors. All our regressions include fixed effects both for the graduate cohort and for the year in which the typical student studies abroad, which was used to assign the ERASMUS exposure.

We restrict our sample to those observations for which all variables of interest are observed. As mentioned before, students from the graduate cohorts 1989, 1993, and 1997 have been surveyed twice, the first time one year after graduating from university and a second time five years after graduation. We thus have two observations for the location of work for most individuals from those cohorts. In the estimation below, we pool the observations from the initial and the follow-up survey for efficiency reasons. This allows us to use the information provided in both questionnaires. Means and standard deviations of our estimation sample are reported in Table 2.1. It is evident from comparing columns (2) and (3) that individuals who studied abroad are also more likely to work abroad later in life. One can also see that individuals with more exposure to ERASMUS (as measured by ERASMUS ratio or ERASMUS indicator, which are described in further detail below) are more likely to study abroad. In the following section we explain how we use the ERASMUS programme to identify the causal link between studying abroad and international labour market mobility later in life.

2.3 Identification Strategy

In order to investigate the effect of studying abroad on international labour market mobility we estimate the following equation.

$$\text{(1) Work Abroad} = \beta_1 + \beta_2\text{Study Abroad} + \beta_3\text{X} + \beta_4\text{Cohort FE} + \beta_5\text{Year Abroad FE} \\ + \beta_6\text{Subject FE} + \beta_7\text{University FE} + u$$

Where *Work Abroad* and *Study Abroad* are dummy variables indicating whether an individual worked abroad or studied abroad, respectively. X is a vector of personal characteristics, which may affect the decision to work abroad, such as gender, work experience or an individual's family background. We also include a full set of dummies for each graduate cohort, the year a typical student goes abroad (as explained in the previous section), a student's subject, and university. Our main interest lies in obtaining consistent estimates of β_2.

The summary statistics presented in the previous section clearly indicate that students who study abroad differ systematically in their observable characteristics from those who remain in Germany throughout their undergraduate studies. Although our data set is rich in observed characteristics of the student, many dimensions which are likely to affect the students' mobility decision remain unobserved. A possible factor could be, for example, the students' unobserved motivation. If these unobserved factors are correlated with the outcome, estimating equation (1) using OLS would yield biased estimates, because we would mistakenly attribute the effect of the unobserved covariates to the stay abroad. While it is generally difficult to characterize these unobserved components in its entirety, there is some direct evidence of what factors may play a role. In their sociological analysis of determinants of studying abroad, Muessig-Trapp & Schnitzler (1997) identify as critical factors affecting the decision to study abroad the student's financial situation, whether she holds any part-time job, foreign language skills, the expected labour market benefit of going abroad, and her motivation and personality structure. Clearly, many of these dimensions will be unobserved to the econometrician. Thinking about our outcome of interest it is likely that the same unobserved factors which drive the decision to study abroad will also affect the decision of where to look for a job. It is therefore not clear what at all can be learned from a comparison of means of those who study abroad versus those who do not. This underlines that this context requires a credible identification strategy to learn about the causal impact of the study period abroad. We use the ERASMUS programme as an instrumental variable to identify the causal effect of studying abroad. As our first stage we estimate the following equation:

$$\text{(2) Study Abroad} = \gamma_1 + \gamma_2 \text{ERASMUS} + \gamma_3 \text{X} + \gamma_4 \text{Cohort FE} + \gamma_5 \text{Year Abroad FE} \\ + \gamma_6 \text{Subject FE} + \gamma_7 \text{University FE} + \epsilon$$

ERASMUS is a variable measuring a student's exposure to the ERASMUS programme, which we describe in further detail below. In addition to the main variables of interest we include the same control variables as in equation (1).

Our identification strategy relies on the large scale introduction and expansion of the ERASMUS programme. In 1987, the Council of Ministers of the European Community passed the *European Community Action Scheme for the Mobility of University Students* (ERASMUS). The main objective of ERASMUS is "to achieve a significant increase in the number of students [...] spending an integrated period of study in another Member State." Student mobility was to be increased through the creation of a European university network, individual scholarships, and mutual recognition of academic credits. Since then, ERASMUS has continually expanded. Looking across all participating countries, 1.37 million students have taken part in ERASMUS in the period of the academic years 1987/88 to 2004/05, with 15.7% of those outgoings coming from Germany.[33] The magnitude of the expansion can be clarified by relating ERASMUS outgoing students to the number of students in a given cohort. For example, of those graduates surveyed in 2001, about 5% of German students studied abroad with an ERASMUS

[33] One argument why outmigration of skilled workers does not lead to brain drain is that there may be off-setting immigration from other countries. Statistics on the ERASMUS program indicate that within the ERASMUS student exchange, German outgoing students are not matched by corresponding incoming students from other countries: In 2004/05, German outgoing ERASMUS students exceed incoming ERASMUS students by about 30 percent.

scholarship.[34] The overall incidence of studying abroad for the 2001 cohort is 8 percent. The ERASMUS programme therefore accounts for more than half of international undergraduate mobility in Germany in our last cohort. Particularly noteworthy is the over-time change in the number of ERASMUS scholarships. Figure 2.3 shows the number of German outgoing students for each year since the introduction of the programme.

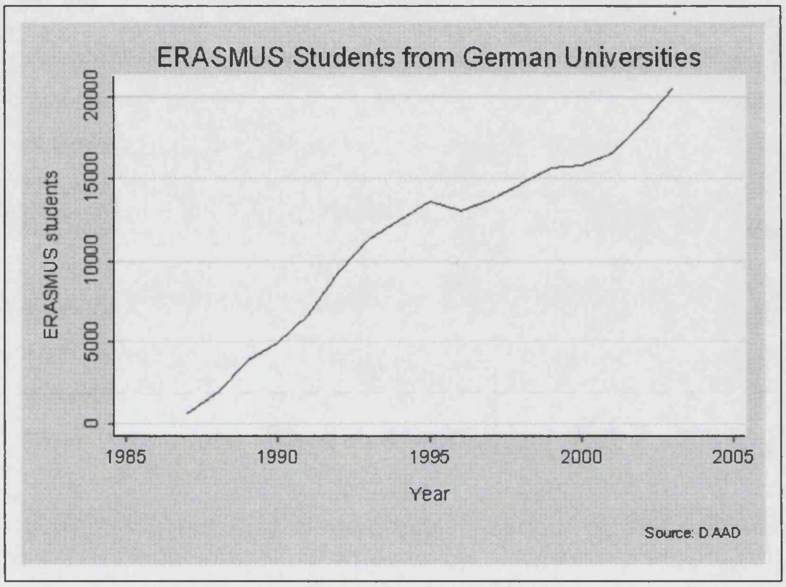

Figure 2.3: ERASMUS in Germany

The dramatic expansion is clearly visible. Students in our four graduate cohorts are therefore affected quite differently by the programme. It is important to be precise about the variation we exploit to identify the effect of ERASMUS. We account for systematic differences between universities by including university fixed effects. Our empirical strategy thus relies on over-time changes in scholarship availability. At the same time, we include dummies for our four graduate cohorts, so that any difference that is common to all students in a cohort is taken out as well. This ensures that we are not relying on any long-term trends (which may possibly affect both the instrument and the outcome). Furthermore, we include dummies for the year a typical exchange student begins her stay at a foreign university. We define this year as the year when the median exchange student in a given subject and graduate cohort enrols at a foreign university. We allow this year to vary across different subjects because students in different departments integrate their stay at a foreign university at different times of their degree. We also allow this timing to vary across different university types. In addition to that we include subject fixed effects in our estimation. This accounts for any systematic difference in international mobility

[34]This number is obtained as follows: In the 2001 graduate cohort, the median student started her tertiary studies in the academic year 1995/96. In that year, about 262,000 students entered university. The typical exchange student in that cohort studied abroad in the third year of her studies. In that year 13785 students from German universities participated in the ERASMUS program. This corresponds to about 5% of the entire cohort.

of students in different subjects. We therefore rely on over-time changes in programme intensity at a given subject and university combination. Probing the robustness of our findings we also include subject specific time trends in our specifications. These allow for a separate linear trend in the probability of studying abroad for each subject. The nature of our results is not affected by including those time trends. In another robustness check we further control for possible unobserved heterogeneity by including fixed effects for the interaction of a student's faculty (such as humanities or science faculty) and her university. We show below that our findings are robust to using these fixed effects.

Students participating in the ERASMUS programme apply for an exchange scholarship at their home university. The award of the scholarship not only secures them a place at a certain partner university abroad but also provides them with a small mobility grant. In the academic year 1997/1998 (the year a typical student from the 2001 graduation cohort went abroad) an outgoing student from Germany received about 138 Euros per month for her stay abroad. In addition to receiving the mobility grant the ERASMUS student receives a tuition fee waiver at the foreign university. Another important benefit of ERASMUS is that it significantly reduces the student's application costs and the time the student needs to apply in advance to be able to organize a stay at a foreign university.

In order give a insight into the variation, which is exploited in our identification strategy, we show the raw data on the number of ERASMUS students at four departments at the two large universities in Munich in the following figure.[35] The introduction of the ERASMUS programme at a certain department occurred at different points in time at the two universities. After the introduction of the programme the number of students going abroad varies over time.

[35] We choose the Ludwig-Maximilians University and the Technical University Munich for our descriptive analyis because they are located in the same city and are of similar quality and reputation. This is exemplified by the fact that these two unversities were among only three universities to be selected as winner of the "Initiative for Excellence". This initiative allocates federal funding to German universities which are considered to have the potential to become world-class research universities. This potential was evaluated based on the universities' past performance and on their strategic plans for the future.

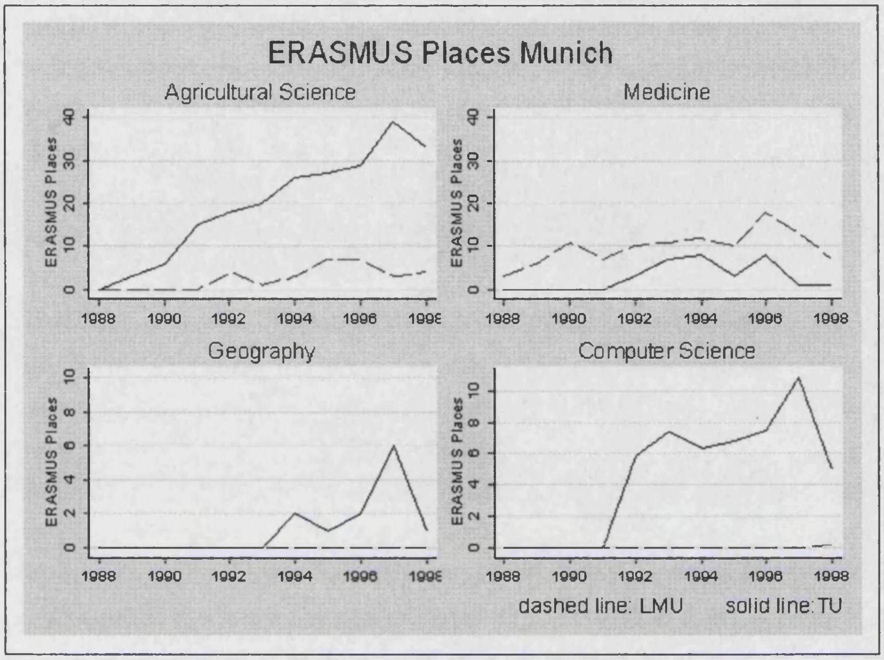

Figure 2.4: ERASMUS at Universities in Munich

We construct two different measures of a student's exposure to the ERASMUS programme. The first variable measures the exact number of ERASMUS scholarships, offered by each department at every university in a given year. In order to account for differences in size of different departments, we normalize the number of scholarships with the number of students competing for these scholarships. We use the number of first year students in the fall semester of the academic year 1992/93 for this normalization. Again, these student numbers are at the university-subject level. In the following we refer to this variable as *ERASMUS ratio.* This measure for a student's exposure to the scholarship programme varies at the university, subject, year level.
The second ERASMUS measure is an indicator, which takes the value one if the student's department offered an ERASMUS scholarship in the relevant year. In almost all cases this variable is 0 until a certain department joins the ERASMUS programme and 1 thereafter, because very few departments leave the programme after they have joined. We denote this variable *ERASMUS indicator,* which varies in the dimensions university, subject, and year as well. On the one hand this variable is less powerful than the other measure because it does not capture changes in the number of ERASMUS scholarships provided, which certainly affect a student's probability of studying abroad. On the other hand, however, this disadvantage may be an advantage if student demand affects the number of ERASMUS places. This would affect the credibility of any instrument using the actual number of ERASMUS scholarships. Even though we believe that this is not an important concern in practice we propose our ERASMUS indicator variable as an alternative, which deals with this concern. The ERASMUS indicator variable is 0 if a department does not offer any ERASMUS scholarships and 1 if any ERASMUS scholarship is offered. Using the ERASMUS indicator as an instrument amounts to a classical

difference-in-differences estimator comparing students before and after the introduction of an exchange programme for their subject at their university. The only way in which student demand may affect this instrument is through triggering the introduction of ERASMUS in the relevant department, which we believe is extremely unlikely. Administrative hurdles when setting up the programme stand in the way of any short term responses to student demand. If a certain department wants to join the ERASMUS programme, the university has to apply for a certification at the European Commission. Moreover, the department has to find partner universities, which are willing to exchange students with the given department. Clearing these administrative hurdles takes time. It is therefore very unlikely that departments are able to set up a new ERASMUS programme in time for a certain cohort to be able to benefit from that introduction.

Where does the over-time variation in ERASMUS come from? University participation in ERASMUS operated through Inter-University Cooperation Programs (ICP), in which groups of university departments from different countries formed a network covered by an ICP agreement, typically initiated through an active professor who happens to have contacts with professors at foreign universities. Departments enter the programme at different times, and this provides us with a lot of variation in programme participation. One way to interpret the evolution in ERASMUS scholarships is to think of the cooperations as an emerging network. Many departments would at some point enter ERASMUS with a few links to departments at foreign universities. Over time other foreign departments would be taken into the network. Similarly the German department itself would enter other (possibly new) cooperation networks.

In order to visualize how students are affected by these shocks of being faced with more or less exchange opportunity, we perform the following event study: For each student's initial university and subject choice, we observe whether there was at any point an ERASMUS cooperation in the time period we observe. We group students by whether they entered the university before or after the introduction of the ERASMUS scheme, and by how many years. In the following figure we plot the time difference between the introduction of ERASMUS and university entry against the probability of going abroad. Keeping in mind that students usually start two or three years before going abroad, we get the following prediction: According to our hypothesis, the probability of studying abroad should be flat for the cohorts starting more than three years before the introduction. The cohorts starting three or two years before the introduction of ERASMUS would then be the first ones to be affected, and we expect an increase in the proportion of students studying abroad from then on. The results can be seen in Figure 2.5. The figure provides evidence that the ERASMUS scheme affects the different cohorts in a very precise way. Closely following our prediction, the probability of studying abroad is low and flat before the introduction of ERASMUS, and goes up steeply afterwards. Furthermore, our data provides evidence that institutions which have *not yet* introduced ERASMUS are similar to those which *never* introduce ERASMUS: Students at institutions which never introduce ERASMUS have a probability of studying abroad of 2.6%, which closely matches the average for the not-yet-affected students in the graph.

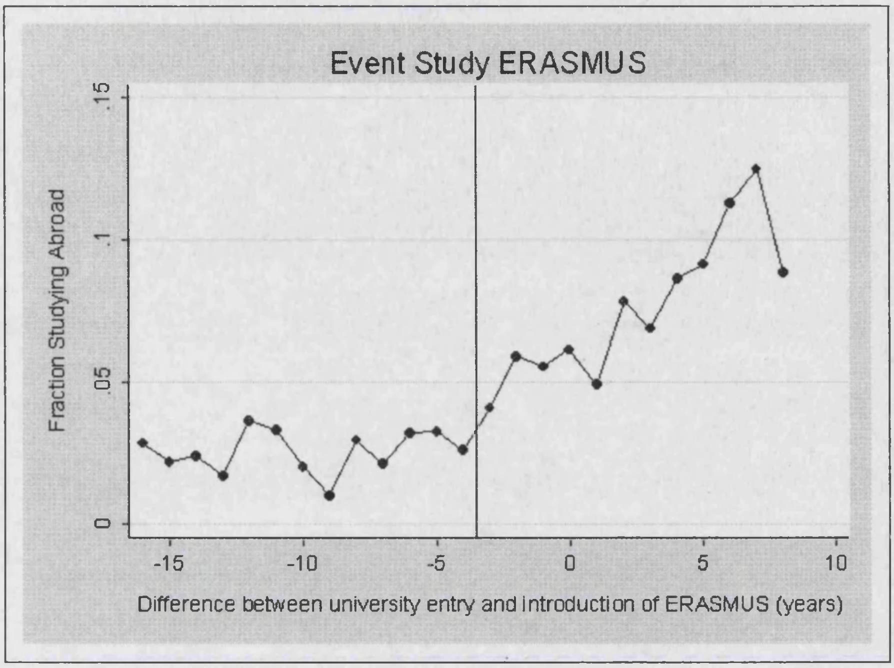

Figure 2.5: Event Study ERASMUS

The usefulness of ERASMUS as an Instrumental Variable (IV) depends on two conditions: First, the IV needs to be correlated with the endogenous variable (studying abroad). Second, it needs to be uncorrelated with the error term u of the outcome equation. The rank condition can be verified by looking at the first stage regression, which we present in the following section of this chapter.

The exclusion restriction requires that there is no direct effect of the instrument on the outcome except through the endogenous variable. Since ERASMUS scholarships are restricted to educational exchange for undergraduate students, this is arguably satisfied. Furthermore, it is required that our IV is not correlated with any other variable which affects the outcome. We argue that this is satisfied through our empirical strategy. We address possible concerns in turn. In particular, we consider the '*university quality*' argument, the '*big push*' argument, and the '*student selection*' argument.

One concern may be that university quality affects both scholarship availability and the outcome: If good universities offered more ERASMUS scholarships, and if at the same time good universities produced higher skilled graduates who are more likely to find a job in a different country, the exclusion restriction would be violated. A similar argument applies if students at good universities were particularly motivated and able, making them more mobile even in the absence of ERASMUS. We take care of this problem by including university fixed effects (FE) in all our regressions, which control for any permanent university attribute. A closely related criticism is that even within a given university some faculties, such as sciences, may be better than other faculties. We show that our results also hold if we include faculty times university fixed effects, which control for any permanent difference between faculties even within a given university.

A common concern in IV estimation is that using a particular policy may carry the risk of not accounting for other policies which were implemented at the same time. Consider a university which at some point decides to raise its profile, and implements a number of measures designed to increase the attractiveness of the institution. For example, it could engage in more active exchange activities also outside Europe and possibly implement other measures at the same time. One way to demonstrate that this is unlikely to be the case is by showing that the ERASMUS programme had a very precise and narrow impact. We use information of where students went to study abroad, grouped into three categories (Europe, United States, and other areas). We show below that the ERASMUS programme only affected the exchange to Europe but not to other areas. This provides additional reassurance that our instrument has a very precise effect, only affecting a student's probability to study abroad in Europe.

Another concern is that students may choose a particular university-subject combination because of scholarship availability. Particularly mobile students might choose universities and departments offering a large number of ERASMUS scholarships. This would again bias our IV results. We do not think that this is likely to occur, however. Since most of our sampled individuals started their university career long before the widespread availability of the internet, information about exchange programs was extremely difficult to obtain. Even nowadays it is hard to obtain information on the availability of ERASMUS scholarships on departmental websites of German universities. It is much more likely that enrolment decisions are based on factors such as reputation of the university or closeness to home. We also address the student selection argument directly by controlling for distance between the state of a student's high school degree and her university. Controlling for earlier mobility does not affect our results. A related worry is that students may change university or department after they figured out that their university and/or department offers little opportunity to study abroad. Using the ERASMUS measures from a student's *first* enrolment enables us to avoid any problems of selective mobility after university entry of the student.

In summary, we believe that in our empirical framework ERASMUS scholarship availability provides us with exogenous variation in the student's decision to study abroad. After controlling for university FE, subject FE, graduate cohort FE, and year abroad FE we argue that the remaining variation can be understood as random shocks to the student. Depending on the cohort, subject, and university she belongs to, she will find a different set of international cooperations at her disposal. These differences in scholarship availability will then translate into variation in the decision to study abroad. Using ERASMUS as an instrumental variable we can therefore estimate equations (1) and (2) to find the causal effect of studying abroad. In all the regressions reported below we account for any dependence between observations by clustering all results on a university-subject level. This leaves the error correlation within clusters completely unrestricted and allows for arbitrary with-in cluster dependence. The clustering, therefore, not only allows arbitrary correlations of errors for students from a graduate cohort at a certain university and subject combination but also allows the errors of a university-subject combination to be serially correlated. The following sections discuss the results we obtain using our identification strategy.

2.4 First Stage Results

Table 2.2 presents the results from our first stage estimates. In this context the first stage regressions are interesting in its own right as one can learn about the factors affecting an individual's decision to study abroad. We regress an indicator for studying abroad on our measure for exposure to the ERASMUS programme and other control variables. In column (1) we use the ratio of ERASMUS places to the number of students in the relevant cohort as our measure for a student's exposure to ERASMUS. The coefficient on ERASMUS is highly significant with an F-statistic of 17.4. The coefficient indicates that an increase in the ratio of ERASMUS places from say 5 percent to 10 percent increases an individual's probability of studying abroad by about 1 percentage point. Analysing the effect of our control variables one can see that a student's gender does not seem to affect her probability of studying abroad. Students who have completed an apprenticeship before enrolling at the university are about 1.3 percentage points less likely to study abroad during their undergraduate studies.

In column (2) we use an indicator for whether the student's department participates in the ERASMUS programme as our measure for exposure to the ERASMUS programme. Once again the coefficient on the ERASMUS measure is highly significant with an F-statistic of 9.1. The coefficient indicates that a student's probability of studying abroad increases by about 1.4 percentage points if her department participates in the ERASMUS programme. The coefficients for the control variables are very similar to the ones reported in column (1).

In columns (3) and (4) we add controls for a student's parental background to our specifications. Parental occupation is measured in five categories for each parent. We include a full set of dummies for these categories in these specification. To save space we do not report the coefficients on all those dummies.[36] Parental education is measured in three categories as the education level achieved by the parent with more education. The results indicate that students with better educated parents are significantly more likely to study abroad. Students with a parent whose education level falls in our second category are about 1 percentage point more likely to study abroad than students with parents who have at most 13 years of education. Students with a parent holding a university degree are about 3.4 percentage points more likely to study abroad. The coefficients and standard errors of our ERASMUS measures are hardly affected by including the controls for parental background. This is reassuring as it indicates that explicitly accounting for socioeconomic background does not alter the power of our intervention on students' behaviour.

The specifications reported in columns (5) and (6) include controls for a student's mobility at the beginning of her studies. The first mobility measure is an indicator for whether the student has her first university enrolment in the federal state *(Bundesland)* where she graduated from high

[36]More detailed results with reported coefficients for the occupational dummies are available from the authors upon request.

school. We add a further control, which measures the distance from the state where a student obtained her high school degree to the state of her first university enrolment. The results indicate that students who study in the state of their final high school degree are about 1.4 percentage points less likely to study abroad. Even though the coefficient on the distance measure for pre-university mobility has the expected positive sign (those who enrol at a university further away from the state where they obtained their high school degree are more likely to study abroad), this variable is not significantly different from 0. The estimates for the effect of the ERASMUS programme are not affected by including the controls for early mobility.

In the following we show that the ERASMUS programme has a very specific effect on studying abroad, as it only affects the probability of studying abroad in a European country but not in countries outside Europe. This is a clear indication that the introduction of ERASMUS was not one of many policies to improve university quality, which in turn could affect the outcome as well. In order to demonstrate the precise effect of studying abroad we create three indicator variables, which take the value 1 if an individual studied abroad in Europe, the USA, or in any other foreign country respectively. We expect that our instrument only affects the probability of studying abroad in Europe as the ERASMUS programme only offers scholarships for studying abroad in European partner universities. In columns (1) and (2) of Table 2.3 we replace the dependent variable of our usual first stage regression (studying abroad in any country) with an indicator for studying abroad *in Europe* instead. The specification reported in column (1) is estimated using the ratio of ERASMUS scholarships. In column (2) we present the results from using the ERASMUS dummy as our measure for exposure to the programme. The coefficients on the ERASMUS measures are strong and highly significant. The magnitude of the ERASMUS coefficient is similar to the one obtained when we use the general definition of studying abroad. We use an indicator for studying abroad in the US as our dependent variable for the specifications reported in columns (3) and (4). The coefficient on the ERASMUS measures is not significantly different from 0. Furthermore, the point estimates of the ERASMUS measures are very close to 0. In columns (5) and (6) we report specifications where we use an indicator for studying abroad in any country outside Europe or the US as the dependent variable. The results indicate that the ERASMUS programme has no effect on the probability of studying abroad in countries outside Europe or the US. The evidence from Table 2.3 strongly suggests that the introduction of the ERASMUS programme was not correlated with the introduction of a broader set of policies, which might themselves affect later labour market outcomes. These results increase our confidence for using the ERASMUS programme as an instrumental variable for studying abroad. In the following section we use this IV to obtain estimates of the effect of studying abroad on the probability of working in a foreign country later in life.

2.5 Main Results and Sensitivity Analysis

The OLS results reported in column (1) of Table 2.4 confirm that graduates who spent some time at a foreign university are more likely to work abroad later in life. Our OLS result indicates that the effect of studying abroad is about 6 percentage points. Note that as before, all our standard errors are clustered at the university-subject level. As discussed before we do not want to attribute causality to the OLS results. This is because the factors affecting an individual's decision to study abroad are likely to affect her decision to work abroad later on as well. Therefore, we now turn to our IV results.

In column (2) of Table 2.4 we present the first set of IV results using the ratio of ERASMUS scholarships to the total number of students in the department as an instrument. We find that studying abroad increases an individual's probability to work in a foreign country by about 24 percentage points. Given the relatively large standard error this effect is significant at the ten percent level. We also find that females are about 0.6 percentage points more likely to work abroad. Furthermore, we find that individuals who completed an apprenticeship before they enrolled at university are about 0.4 percentage points less likely to work abroad, although this effect is not significant. People who complete an apprenticeship may be more likely to go back to work at the same firm where they completed their apprenticeship, which will usually be located in Germany. We also find that labour market experience has an effect on the probability of working abroad. The coefficient indicate that individuals with one more year of experience in the labour market are about 0.5 percentage points more likely to work abroad. Within a survey wave, there is relatively little variation in potential experience, and this estimate also captures the increased probability of working abroad from the initial to the follow-up survey. Over and above this annual measure of potential experience, the indicator variable for the follow-up survey does not show up significantly.

Column (3) adds interactions of the ERASMUS ratio with a full set of subject dummies as instruments. Including these interactions allows the impact of ERASMUS to differ across subjects. This may be relevant as it is quite likely that a student exchange programme has a different impact for students studying different subjects. Since all our specifications include subject fixed effects, identification does not exploit any permanent differences between subjects. Instead, this specification allows the effect of the ERASMUS to vary by subject in the first stage. Including the interactions strongly increases the precision of our estimates. The coefficient on studying abroad is significant at the 5 percent level and indicates that studying abroad increases an individual's probability of working abroad later in life by about 14 percentage points. The coefficients on the control variables are very similar to the ones reported in column (2).

Even though we do not believe that student demand has a large impact on the number of ERASMUS scholarships we address this concern by using the *ERASMUS indicator* as our instrument in the specifications reported in columns (4) and (5). In column (4) we present the results from using the ERASMUS indicator as the only instrument. The standard errors on the coefficient for studying abroad increases a lot, because the dummy for offering any ERASMUS scholarships is a much less precise measure of a student's exposure to exchange opportunities. The point estimate, however, is very similar to the one we obtain if we use the *ERASMUS ratio* instrument.

In column (5) we show that using the interactions of the ERASMUS indicator with a full set of subject dummies increases the precision of our estimates. The estimated coefficient on studying abroad indicates that studying abroad increases an individual's probability of studying abroad by about 19 percentage points. As before, the coefficients on the other variables are hardly affected by using the indicator measure instead of the ratio measure of ERASMUS. Even though we lose some precision by using the ERASMUS indicator as our instrument the results are very similar to ones obtained if we use the ERASMUS ratio. Given these results we are confident to say that our results reflect a supply-side increase in scholarship availability, rather than students' demand. One common concern in IV estimation is a potential bias due to weak instruments (see Bound, Jaeger & Baker (1995) and Stock, Wright and Yogo (2002)). The F-statistic from the first stage, reported at the bottom of Table 2.4, show that for most of our estimates, weak instruments are not likely to pose a problem even using conservative cut-off values for the F-statistic. In the ERASMUS indicator specification with subject interactions (column (5)), the F-statistic is comparatively lower, but the coefficient is very similar to the previous ones, suggesting that weak instrument bias is not likely to be a problem here.

In summary, our IV results indicate that studying abroad increases the probability of working abroad by around 15 to 20 percentage points. In the following, we show that our results are robust to a number of specification checks.

There may be a worry that students from different family backgrounds not only choose universities with different provision of ERASMUS scholarships but also exhibit different propensities to work in a foreign country. As long as this effect is constant over time we deal with this problem by estimating all equations including university fixed effects. It could be possible, however, that people from different backgrounds react differently to the introduction of an ERASMUS programme or changes in the number of scholarships. In order to address this concern we add controls for parental education and occupation to our main specification. It is evident from looking at Table 2.5 that including the measures for parental background hardly affects our estimates of the effect of studying abroad. The results indicate that students from better educated parents are between 0.5 and 1 percentage points more likely to work abroad, although this effect is not always significant.

Another concern is that students with a taste for mobility chose universities or departments with a lot of ERASMUS scholarships. Our IV estimates would be biased if these individuals were more likely to work abroad later in life. In the following we present a powerful test, which directly addresses this concern. We add two variables which control for a student's mobility at the start of her university career. The first variable indicates whether the student enrols in university in the state *(Bundesland)* where she obtained her high school diploma *(Abitur)*. The second mobility variable measures the distance from the state where she obtained her high school diploma to the state of her first university enrolment. Including those two mobility variables hardly affects the estimates for the effect of studying abroad as can be seen from looking at Table 2.6. The coefficient on the distance measure for early mobility indicates that individuals who chose to study further away from the state where they received their high school diploma are more likely to work abroad later in life. At the same time, the results from Table 2.6 indicate that the effect of studying abroad remains unchanged.

Individuals may be more likely to work abroad if they know more foreigners. There are at least two channels through which the number of contacts to foreigners may affect the likelihood of working abroad. One channel may be an increased number of contacts to future business partners. A further channel may be that contacts to foreigners increase an individual's taste for foreign cultures which may affect her probability of working abroad. As the ERASMUS programme is at least partly reciprocal, universities offering more ERASMUS scholarships may also enrol more foreign students. This could then increase the student's propensity to work abroad later on and therefore bias our IV results. In Table 2.7 we present the results from adding the university wide ratio of foreign students over the total number of students in a student's cohort[37] to our specification. Adding this control does not change the coefficient on studying abroad at all. The coefficient on our measure for the exposure to foreign students is highly significant but rather small in magnitude. The estimated coefficient indicates that increasing the percentage of foreign students at a student's home university from say 5 to 15 percent increases her probability of working abroad by about 0.08 percentage points. This exercise is interesting also because it adds university-specific covariates which vary over time, and it is reassuring that the results remain unchanged.

In the following we check whether our results are driven by time trends in our variables of interest. Including graduate cohort FE (as in all specifications) guarantees that we do not identify the effect of studying abroad on working abroad from overall time trends. There may be a worry, however, that students studying certain subjects exhibit time trends in both studying abroad and working abroad. To address this issue we include linear subject specific time trends. The results of this exercise are reported in the second panel of Table 2.8. Apart from the specification reported in column (3) the inclusion of the subject specific time trends hardly affects the coefficient of studying abroad.

It may be the case that groups of departments within a university differ in quality or in their ability to foster international exchange. We address this concern by including a full set of department group times university fixed effects. We thus use a separate fixed effects for say sciences or languages at a certain university. Including this fine level of FEs hardly affects the estimates using the ERASMUS ratio instrument. Not surprisingly the estimates using the ERASMUS indicator instrument are slightly more affected. The order of magnitude of the estimate, however, is preserved.

It is reassuring that the inclusion of time trends or a finer set of fixed effects does not have a huge impact on our estimates. This and the fact that our estimates are hardly affected by including controls for parental background, for early mobility, and for the number of foreign students at the home university makes us confident that using the ERASMUS programme as a source of exogenous variation is a credible identification strategy to estimate the causal effect of studying abroad on later labour market mobility.

One defining feature of our results is that the IV results are substantially higher than the

[37] We use the ratio at the middle of the average student's university career as the relevant measure for contacts to foreigners.

corresponding OLS result. We interpret this finding in terms of heterogeneity in returns: It is unlikely that all students will be affected in the same way by the intervention of studying abroad. It is much more likely that the effect of studying abroad itself varies across the student population. We follow Imbens & Angrist (1994) and interpret our estimates as a Local Average Treatment Effect (LATE): The IV results show the average effect for the subgroup which has been affected by the instrument. In the context of our instrument, this group is well-defined: It is the group of students who would not have studied abroad without the ERASMUS programme, but study abroad when ERASMUS is implemented. Since they are the students who have been affected by the ERASMUS programme, our estimates are of immediate interest to policy makers. What are the characteristics of these switchers? In the absence of credit constraints, this will be the group of students for whom the cost of studying abroad is slightly above the returns without ERASMUS. The introduction of ERASMUS can be understood as a price change which makes the investment into studying abroad worthwhile for these marginal students. In the presence of credit constraints, some students will not be able to invest in studying abroad even though this investment offers a positive return. These students are prevented from realizing the returns to studying abroad by being credit constrained. The following analysis suggests that credit constraints are likely to play a role. We follow Kling (2001) in interpreting the IV estimate as a weighted average of the causal effect of studying abroad, where the weight of each subgroup j is given by the following formula:

$$(3) \qquad weight_j = \frac{w_j \lambda_j \Delta(StudyAbroad)_j}{\sum_j w_j \lambda_j \Delta(StudyAbroad)_j}$$

Here w_j is the sample fraction of each subgroup j, λ_j is the variance of the instrumental variable for subgroup j conditional on all other regressors x, and $\Delta(StudyAbroad)_j$ is the impact of the ERASMUS instrument on the probability of studying abroad for subgroup j. The last term is obtained from estimating the first stage regression separately for each subgroup.[38] We use this decomposition to compute the corresponding weight for two subgroups: students who are credit constrained and a subgroup which is unlikely to be credit constrained. We proxy credit constraints with an indicator variable which takes the value 1 if the student ever received any federal financial assistance in the BAFOEG scheme during the course of study. In our sample, this is about 41% of all observations (see column (1) in Table 2.9). Here, we use the ERASMUS indicator variable as instrument. Not surprisingly, the overall proportion of students who study abroad is smaller for the credit-constrained group than for the non-credit constrained group, reflecting differences in investment behaviour between these two groups. Interestingly, column (2) indicates that the first stage is stronger for credit constrained students: They react more strongly to the introduction of ERASMUS. This seems sensible as it indicates that credit-constrained students rely more heavily on the ERASMUS programme. Exposure to ERASMUS as measured by the conditional variance λ is similar between the groups (column (3)). Computing the resulting weights, column (4) states that the IV estimate places a weight of 54% on the group of the credit constrained students, which make up only 41% of the sample. This

[38] See Kling (2001) for further details.

underlines that credit constrained students contribute disproportionately to our IV estimates of the effect of studying abroad.

2.6 How Studying Abroad Affects International labour Market Mobility

The results presented in the previous sections indicate that individuals who study abroad are more likely to work in a foreign country. It is interesting to understand how studying abroad affects an individual's decision to migrate to a foreign country later in life. We address this in two ways: First, we make use of observed location choices to study the type of skills acquired during the stay abroad. Second, the survey provides us with direct qualitative evidence on why graduates move abroad, and we show how this varies depending on whether the student studied abroad earlier. As these qualitative questions were only administered to one cohort we cannot apply our instrumental variable strategy here. We therefore provide a descriptive analysis, which – if only suggestive – may shed light on the way studying abroad affects later labour market mobility.

We can think of the effect of studying abroad as affecting the set of skills the student acquires during her studies. One important question is whether these skills have a strong location-specific component. We can shed some light on this question by investigating whether individuals who have studied abroad return to work in the same country when they decide to work in a foreign country. There are a number of reasons why mobile graduates may be more likely to work abroad in the countries where they studied abroad before: During their study period abroad they may have obtained skills that are of particular relevance in that labour market, e.g. language skills, knowledge about the local labour market, or personal contacts which facilitate a match. On the other hand, it is possible that studying abroad affects the probability of working abroad equally for different work destinations. This would be the case, for example, if studying abroad widens the horizon of the student generally and leads her to search for a job internationally, independent of where she studied before. Especially, studying abroad could operate as a stepping stone to increase the set of feasible destinations. This question is also highly relevant from a policy perspective: The ability of the ERASMUS scheme or other student mobility programs to achieve an integrated European labour market depends on the assumption that students who went abroad to study in Europe are internationally mobile after graduation, but remain in Europe.

Here we present descriptive evidence to address this question for the cohorts 1993, 1997, and 2001.[39] We again group location choices into Europe, US, and other areas, and restrict attention to students who work abroad. For each study abroad treatment and study abroad location, Table 2.10 shows the conditional probability of being in each work location. Table 2.10 provides evidence that choices about study abroad locations are sticky, that is that students tend to return to work to the region where they studied abroad. In particular, of the students who studied abroad in Europe and worked internationally after graduation, two thirds end up working in a

[39]For the cohort 1989, we do not have information on locations of study abroad.

European country. A χ^2-test of independence between the study abroad location and the work abroad location is rejected at the one percent level with a test statistic of 28.5.

We now turn to qualitative evidence from the survey on why graduates moved abroad. The HIS questionnaire asked individuals who had already worked abroad to give reasons for why they had chosen to do so. Unfortunately, this question was only administered to individuals from the 1997 graduation cohort.

Students who had worked in a foreign country for at least one month in the five years since graduation were asked to identify the reasons for their decision to work abroad. In Table 2.11 we present the percentage of the people who indicated that a certain reason had been important in their decision to work abroad. The table shows that the main reasons for working abroad are interest in foreign cultures, interesting offers from abroad, and the initiative of the employer. We split the sample into those who complete all their university education in Germany and those who study abroad for some time during their undergraduate education. Interestingly, while the means are similar in some categories, there are a number of noteworthy differences. Those who have studied abroad are more likely to indicate that their interest in foreign cultures has led them to seek employment abroad. It may be the case that studying in a foreign country increased the individual's taste for living abroad, which may in turn increase her probability of migrating later in life. Students who have studied abroad are also significantly more likely to indicate that they chose to work abroad to be with their partner. The answers to this question may suggest that people who studied abroad may have met their partner while studying abroad and therefore consider to work abroad later in life. Of course, this difference may also be driven by assortative mating with more mobile people having more mobile partners, and the way this question was asked makes it impossible to distinguish between these alternatives. Meeting a partner abroad may, nonetheless, be a possible channel of the effect of studying abroad. The summary statistics also indicate that those who have studied abroad are somewhat more likely to say that they work abroad because of better employment opportunities in the foreign labour market, where we obtain a p-value of 0.06 when we test for a significant difference in the means of the two groups for this response. It is possible that a stay at a foreign university makes it easier to realize opportunities in foreign labour markets, either because those who studied abroad have better information on the foreign labour market or because employers are more willing to offer employment to those individuals. Interestingly, rather than the employment outlook, it is the career prospects abroad where the means are significantly different at the 1% level, suggesting that those with international study experience seem to be more likely to consider a career abroad.

The statistics presented here provide some suggestive evidence of how studying abroad may alter later international labour market mobility. Further research is necessary to get a better insight into the channels of the effect of studying abroad on working abroad later on.

2.7 Conclusion

Using exogenous variation in scholarship availability, we are able to identify a causal effect of undergraduate student mobility on later international labour migration. Our strategy exploits the introduction and expansion of the ERASMUS scholarship programme. The extent to which students were exposed to the scholarship scheme varied widely. We exploit cross-sectional and over time-changes in scholarship availability. Accounting for permanent differences between different institutions, different subjects, and different graduate cohorts, our identification relies only on differential over-time change, and can be interpreted as a Difference-in-Differences estimator. Our first-stage shows that the ERASMUS scheme has indeed a strong effect on the students' decision to go abroad, which is not surprising given its scale. We show that the instrument is precise in that it only affects the decision to study in Europe, but not in other locations. Our event study adds further credibility to our instrument, by showing that the probability of studying abroad is low and flat before ERASMUS is introduced, and increases strongly for those students affected by the scholarship.

Our OLS results indicate that the group of students who studied abroad are about 6 percentage points more likely to work abroad later on, controlling for a set of background characteristics, institution and time fixed effects. Our IV results are substantially higher than that, and indicate that the effect of study abroad is between 15 and 20 percentage points. We also provide results which interact the instrument with the students' degree subject. That allows for a differential effect of ERASMUS in different subjects, and adds precision to the results. We interpret the difference between OLS and IV as an indication of heterogeneity in effects: The population which is affected by our instruments reacts particularly strongly to the incentives of the mobility programme. This Local Average Treatment Effect (LATE) interpretation is of particular interest to policy makers, since it evaluates the effect for the affected subgroup. We show that individuals who are credit constrained are particularly affected by the ERASMUS instrument, and suggest channels through which the effect of studying abroad may operate.

Our results suggest that educational mobility programs may have a potentially large role in affecting students' behaviour in their labour market mobility decision. It implies that an opportunity to attract talented graduates is to provide student exchange opportunities. Attractive universities and scholarship programs may yield a return through attracting students, part of whom will remain as skilled workers later on. In the context of the policy change under consideration, ERASMUS is successful in that this student mobility scheme appears to contribute to the development of an integrated European labour market. This is especially so if we take into account the descriptive evidence from the previous section that location choices are sticky, i.e. that mobile students tend to return to the region where they studied before.

More generally, our work allows insight into the dynamic implications of educational mobility decisions. Our results indicate that the effects of educational mobility programs go far beyond affecting the decision to study abroad for some time period, but rather reach far into the labour market, and it will be interesting to follow the sample of graduates as their careers unfold. But already at this early stage our results indicate that even short-term mobility investments can lead to significant further mobility investments later on.

2.8 Tables

Table 2.1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)
		Study	Study	Work	Work
	All	Abroad=0	Abroad=1	Abroad=0	Abroad=1
Working abroad	0.035	0.031	0.107	0.000	1.000
	(0.184)	(0.175)	(0.309)	(0.000)	(0.000)
Undergraduate study abroad	0.045	0.000	1.000	0.042	0.138
	(0.207)	(0.000)	(0.000)	(0.120)	(0.345)
ERASMUS ratio	0.024	0.023	0.047	0.024	0.039
	(0.055)	(0.054)	(0.072)	(0.055)	(0.063)
ERASMUS indicator	0.436	0.425	0.662	0.429	0.622
	(0.496)	(0.494)	(0.473)	(0.495)	(0.485)
Female	0.421	0.419	0.463	0.419	0.496
	(0.494)	(0.493)	(0.499)	(0.493)	(0.500)
Experience	2.824	2.830	2.696	2.817	3.015
	(2.030)	(2.030)	(2.021)	(2.028)	(2.093)
Apprenticeship	0.309	0.315	0.183	0.312	0.234
	(0.462)	(0.465)	(0.387)	(0.463)	(0.424)
Mother's Education (years)	11.825	11.741	13.609	11.775	13.222
	(3.306)	(3.275)	(3.453)	(3.295)	(3.290)
Father's Education (years)	13.331	13.247	15.120	13.28	14.736
	(3.644)	(3.637)	(3.332)	(3.649)	(3.216)
Final University Grade[1]	2.068	2.078	1.859	2.074	1.906
	(0.685)	(0.627)	(0.660)	(0.686)	(0.644)
Bafoeg indicator[2]	0.414	0.418	0.346	0.417	0.355
(Financial Assistance)	(0.493)	(0.493)	(0.476)	(0.493)	(0.479)
Observations	38527	36798	1729	37182	1345

[1]The final university degree is only available for 37644 students in our sample. (The best grade is 1.0 the worst 4.0)
[2]The question on financial assistance has only been administered since 1993. We have information on Bafoeg for 24405 individuals in our sample.
Note: This table contains sample means and (in brackets) standard deviations.

Table 2.2: First Stages

Dependent Variable: Indicator for Study Abroad	(1)	(2)	(3)	(4)	(5)	(6)
ERASMUS Ratio	0.1866		0.1847		0.1819	
	(0.0447)***		(0.0442)***		(0.0440)***	
ERASMUS Indicator		0.0136		0.0134		0.0135
		(0.0045)***		(0.0045)***		(0.0045)***
Female	-0.0027	-0.0027	-0.0041	-0.0042	-0.0040	-0.0041
	(0.0033)	(0.0033)	(0.0033)	(0.0033)	(0.0033)	(0.0033)
Apprenticeship	-0.0131	-0.0130	-0.0086	-0.0084	-0.0089	-0.0088
	(0.0031)***	(0.0031)***	(0.0030)***	(0.0030)***	(0.0030)***	(0.0030)***
Experience	-0.0019	-0.0020	-0.0019	-0.0019	-0.0019	-0.0020
	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Parental Education Dummy 2			0.0101	0.0102	0.0096	0.0097
			(0.0036)***	(0.0036)***	(0.0035)***	(0.0035)***
Parental Education Dummy 3			0.0339	0.0338	0.0327	0.0326
			(0.0043)***	(0.0043)***	(0.0043)***	(0.0043)***
Studying in State of Highschool Degree					-0.0136	-0.0137
					(0.0067)**	(0.0067)**
Distance to High School State (100km)					0.0018	0.0018
					(0.0018)	(0.0017)
Follow-up Survey (Dummy)	0.0076	0.0078	0.0073	0.0076	0.0074	0.0077
	(0.0054)	(0.0054)	(0.0054)	(0.0054)	(0.0054)	(0.0054)
Parental Occupation Dummies			✓	✓	✓	✓
Graduate Cohort FE	✓	✓	✓	✓	✓	✓
Year Abroad FE	✓	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓
Instruments:						
ERASMUS	Ratio	Indicator	Ratio	Indicator	Ratio	Indicator
N	38527	38527	38527	38527	38527	38527
R-squared	0.066	0.065			0.073	0.072
F-stat of Instrument(s)	17.41	9.09	17.43	8.89	17.11	9.04

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)

Table 2.3: Falsification Test: First Stages with Different Destinations

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Study Abroad in	Europe	Europe	USA	USA	Rest	Rest
ERASMUS Ratio	0.1840		0.0142		0.0046	
	(0.0419)***		(0.0155)		(0.0070)	
ERASMUS Indicator		0.0159		-0.0018		0.0009
		(0.0039)***		(0.0019)		(0.0012)
Female	-0.0016	-0.0016	-0.0011	-0.0011	-0.0015	-0.0015
	(0.0026)	(0.0027)	(0.0011)	(0.0011)	(0.0009)*	(0.0009)*
Apprenticeship	-0.0049	-0.0048	-0.0018	-0.0017	-0.0007	-0.0007
	(0.0025)*	(0.0025)*	(0.0011)	(0.0011)	(0.0008)	(0.0008)
Experience	-0.0011	-0.0012	0.0004	0.0004	-0.0004	-0.0004
	(0.0011)	(0.0011)	(0.0005)	(0.0005)	(0.0003)	(0.0003)
Parental Education Dummy 2	-0.0014	-0.0015	-0.0002	-0.0002	-0.0000	-0.0000
	(0.0024)	(0.0024)	(0.0011)	(0.0011)	(0.0009)	(0.0009)
Parental Education Dummy 3	0.0154	0.0151	0.0037	0.0036	-0.0001	-0.0001
	(0.0033)***	(0.0033)***	(0.0015)	(0.0015)	(0.0009)	(0.0009)
Studying in State of Highschool Degree	-0.0105	-0.0106	-0.0005	-0.0005	0.0013	0.0013
	(0.0054)**	(0.0054)**	(0.0022)	(0.0022)	(0.0022)	(0.0022)
Distance to High School State (100km)	0.0010	0.0011	-0.0002	-0.0002	0.0006	0.0006
	(0.014)	(0.014)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Follow-up Survey (Dummy)	0.0045	0.0048	-0.0014	-0.0014	0.0014	0.0014
	(0.0043)	(0.0044)	(0.0019)	(0.0018)	(0.0022)	(0.0012)
Parental Occupation Dummies	✓	✓	✓	✓	✓	✓
Graduate Cohort FE	✓	✓	✓	✓	✓	✓
Year Abroad FE	✓	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓
Instruments:						
ERASMUS	Ratio	Indicator	Ratio	Indicator	Ratio	Indicator
N	38527	38527	38527	38527	38527	38527
R-squared	0.064	0.063	0.074	0.074	0.032	0.032
F-stat of Instrument(s)	19.29	16.82	0.83	0.08	0.44	0.57

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)

Table 2.4: Main Results

Dependent Variable: Work Abroad	(1)	(2)	(3)	(4)	(5)
Estimation Method	OLS	IV	IV	IV	IV
Study Abroad	0.0611	0.2386	0.1444	0.2342	0.1890
	(0.0092)***	(0.1416)*	(0.0582)**	(0.2556)	(0.0820)**
Female	0.0060	0.0064	0.0062	0.0064	0.0063
	(0.0024)**	(0.0025)***	(0.0024)***	(0.0025)***	(0.0024)***
Apprenticeship	-0.0065	-0.0042	-0.0054	-0.0043	-0.0048
	(0.0023)***	(0.0029)	(0.0024)**	(0.0039)	(0.0026)*
Experience	0.0047	0.0051	0.0049	0.0051	0.0050
	(0.0014)***	(0.0014)***	(0.0014)***	(0.0015)***	(0.0014)***
Follow Up Survey (Dummy)	-0.0038	-0.0053	-0.0045	-0.0052	-0.0049
	(0.0052)	(0.0054)	(0.0052)	(0.0057)	(0.0053)
Graduate Cohort FE	✓	✓	✓	✓	✓
Year Abroad FE	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓
Instruments:					
ERASMUS		Ratio	Ratio	Indicator	Indicator
Interactions with subject			✓		✓
N	38527	38527	38527	38527	38527
R-squared	0.038				
F-stat First Stage		17.41	11.11	9.09	3.66

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)
Dependent variable is an indicator for whether the respondent works abroad at the time of the survey.
Study abroad is an indicator for whether the student spends part of her university career at a foreign university.

Table 2.5: Sensitivity Analysis 1: Parental Background

Dependent Variable: Work Abroad	(1)	(2)	(3)	(4)	(5)
Estimation Method	OLS	IV	IV	IV	IV
Abroad	0.0589	0.2354	0.1408	0.2404	0.1858
	(0.0091)***	(0.1424)*	(0.0586)**	(0.2602)	(0.0840)**
Female	0.0051	0.0059	0.0055	0.0059	0.0057
	(0.0024)**	(0.0025)**	(0.0024)**	(0.0027)**	(0.0025)**
Apprenticeship	-0.0043	-0.0028	-0.0036	-0.0028	-0.0033
	(0.0023)*	(0.0026)	(0.0024)	(0.0031)	(0.0024)
Experience	0.0047	0.0051	0.0049	0.0051	0.0050
	(0.0014)***	(0.0014)***	(0.0014)***	(0.0015)***	(0.0014)***
Follow Up Survey (Dummy)	-0.004	-0.0054	-0.0046	-0.0054	-0.0050
	(0.0052)	(0.0054)	(0.0052)	(0.0057)	(0.0053)
Parental Education Dummy 2	0.0076	0.0057	0.0066	0.0056	0.0061
	(0.0027)***	(0.0031)*	(0.0028)**	(0.0039)	(0.0028)**
Parental Education Dummy 3	0.0103	0.0043	0.0075	0.0041	0.0060
	(0.0031)***	(0.0056)	(0.0036)**	(0.0095)	(0.0043)
Parental Occupation Dummies	✓	✓	✓	✓	✓
Graduate Cohort FE	✓	✓	✓	✓	✓
Year Abroad FE	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓
Instruments:					
ERASMUS		Ratio	Ratio	Indicator	Indicator
Interactions with subject			✓		✓
N	38527	38527	38527	38527	38527
R-squared	0.040				
F-stat First Stage		17.429	10.907	8.893	3.419

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)

Table 2.6: Sensitivity Analysis 2: Early Mobility

Dependent Variable: Work Abroad	(1)	(2)	(3)	(4)	(5)
Estimation Method	OLS	IV	IV	IV	IV
Abroad	0.0581	0.2305	0.1378	0.2451	0.1789
	(0.0091)***	(0.1434)	(0.0569)**	(0.2592)	(0.0830)**
Female	0.0052	0.0059	0.0055	0.0060	0.0057
	(0.0024)**	(0.0025)**	(0.0024)**	(0.0027)**	(0.0024)**
Apprenticeship	-0.0046	-0.0031	-0.0039	-0.0030	-0.0035
	(0.0023)**	(0.0014)**	(0.0024)*	(0.0032)	(0.0025)
Experience	0.0047	0.0051	0.0049	0.0051	0.0050
	(0.0014)***	(0.0014)***	(0.0014)***	(0.0015)***	(0.0014)***
Follow Up Survey (Dummy)	-0.0040	-0.0053	-0.0046	-0.0054	-0.0050
	(0.0052)	(0.0054)	(0.0052)	(0.0057)	(0.0053)
Parental Education Dummy 2	0.0072	0.0056	0.0065	0.0055	0.0061
	(0.0027)***	(0.0031)*	(0.0028)**	(0.0038)	(0.0028)**
Parental Education Dummy 3	0.0096	0.0040	0.0070	0.0035	0.0057
	(0.0031)***	(0.0055)	(0.0035)**	(0.0091)	(0.0042)
Studying in Highschool State	0.0021	0.0045	0.0032	0.0047	0.0038
	(0.0057)	(0.0061)	(0.0057)	(0.0070)	(0.0060)
Distance to High School State	0.0033	0.0030	0.0031	0.0029	0.0030
(100km)	(0.0016)**	(0.0016)*	(0.0016)**	(0.0016)*	(0.0016)*
Parental Occupation Dummies	✓	✓	✓	✓	✓
Graduate Cohort FE	✓	✓	✓	✓	✓
Year Abroad FE	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓
Instruments:					
ERASMUS		Ratio	Ratio	Indicator	Indicator
Interactions with subject			✓		✓
N	38527	38527	38527	38527	38527
R-squared	0.040				
F-stat First Stage		17.107	11.126	9.035	3.425

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)

Table 2.7: Sensitivity Analysis 3: Foreign Students at Home University

Dependent Variable: Work Abroad	(1)	(2)	(3)	(4)	(5)
Estimation Method	OLS	IV	IV	IV	IV
Abroad	0.0581	0.2307	0.1373	0.2475	0.1781
	(0.0091)***	(0.1434)	(0.0566)**	(0.2592)	(0.0826)**
Female	0.0052	0.0059	0.0055	0.0060	0.0057
	(0.0024)**	(0.0025)**	(0.0024)**	(0.0027)**	(0.0024)**
Apprenticeship	-0.0046	-0.0031	-0.0039	-0.0029	-0.0035
	(0.0023)**	(0.0026)	(0.0024)*	(0.0032)	(0.0025)
Experience	0.0047	0.0050	0.0049	0.0054	0.0049
	(0.0014)***	(0.0014)***	(0.0014)***	(0.0015)***	(0.0014)***
Follow Up Survey (Dummy)	-0.0039	-0.0053	-0.0045	-0.0054	-0.0049
	(0.0052)	(0.0054)	(0.0052)	(0.0057)	(0.0053)
Parental Education Dummy 2	0.0072	0.0056	0.0065	0.0054	0.0061
	(0.0027***	(0.0031)*	(0.0028)**	(0.0038)	(0.0028)**
Parental Education Dummy 3	0.0096	0.0040	0.0071	0.0034	0.0057
	(0.0031)***	(0.0054)	(0.0035)**	(0.0091)	(0.0042)
Studying in Highschool State	0.0021	0.0045	0.0032	0.0047	0.0038
	(0.0057)	(0.0061)	(0.0057)	(0.0070)	(0.0059)
Distance to High School State	0.0032	0.0030	0.0031	0.0029	0.0030
(100km)	(0.0016)**	(0.0016)*	(0.0016)**	(0.0016)*	(0.0016)**
Foreign Students/Total Students	0.0083	0.0077	0.0080	0.0077	0.0079
	(0.0018)***	(0.0018)***	(0.0018)***	(0.0019)***	(0.0018)***
Parental Occupation Dummies	✓	✓	✓	✓	✓
Graduate Cohort FE	✓	✓	✓	✓	✓
Year Abroad FE	✓	✓	✓	✓	✓
Subject FE	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓
Instruments:					
ERASMUS		Ratio	Ratio	Indicator	Indicator
Interactions with subject			✓		✓
N	38527	38527	38527	38527	38527
R-squared	0.040				
F-stat First Stage		17.11	11.12	9.06	3.42

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)

Table 2.8: Sensitivity Analysis 4: Time Trends and Additional FE

Dependent Variable: Work Abroad		(1)	(2)	(3)	(4)	(5)
		OLS	IV	IV	IV	IV
Baseline specification	coefficient	0.0581	0.2305	0.1378	0.2451	0.1789
(as in Table 2.6)	(st. err.)	(0.0091)***	(0.1434)	(0.0569)**	(0.2592)	(0.083)**
	F-stat 1st stage		*17.107*	*11.126*	*9.035*	*3.425*
Including Subject-Specific	coefficient	0.0576	0.2438	0.0776	0.2235	0.1860
Time Trends	(st. err.)	(0.0091)***	(0.1515)	(0.0605)	(0.2953)	(0.1138)
	F-stat 1st stage		*15.793*	*8.682*	*6.809*	*1.898*
Including	coefficient	0.0568	0.2892	0.1122	0.3243	0.1161
University * Subject group FE	(st. err.)	(0.0092)***	(0.1690)*	(0.0609)*	(0.2817)	(0.0837)
	F-stat 1st stage		*16.851*	*13.789*	*8.130*	*2.971*
Distance home-college variables		✓	✓	✓	✓	✓
Highest Parental Education Dummies		✓	✓	✓	✓	✓
Parental Occupation Dummies		✓	✓	✓	✓	✓
Graduate Cohort FE		✓	✓	✓	✓	✓
Year Abroad FE		✓	✓	✓	✓	✓
Subject FE		✓	✓	✓	✓	✓
Instruments:						
ERASMUS			Ratio	Ratio	Indicator	Indicator
Interactions with subject				✓		✓
N		38527	38527	38527	38527	38527

***significant at the 1% level **significant at the 5% level *significant at the 10% level
(All standard errors are clustered at the university*subject level)
Note: This table only shows results for the coefficient of interest, studying abroad. Regressors not listed include female indicator, apprenticeship, potential experience. Panel 1 reports the results from Table 6. Panel 2 adds time trends for each subject to the main specification. In these regression we also include university FE as before. Panel 3 adds Fixed Effects at the university*subject group level to the main specification. The specifications reported in Panel 3 do not include university FE because we use the finer level of university*subject group FE. See text for details.

Table 2.9: Heterogeneity in Returns and Credit Constraints

	(1) Fraction in Sample w_j	(2) Delta (First Stage) $\Delta(StudyAbroad)_j$	(3) Lambda λ_j	(4) Kling Weight
Financial Aid = 0	0.59	0.011	0.11	0.46
Financial Aid = 1	0.41	0.016	0.12	0.54

Table 2.10: Destinations of work abroad

		Work abroad location			
		Europe	US	Rest	Total
Study abroad = 0		55.7	8.1	36.2	100.0
Study abroad = 1 in	Europe	66.4	4.9	28.7	100.0
	US	45.5	27.3	27.3	100.0
	Rest	16.7	0.0	83.3	100.0

Note: For all graduates working abroad, this table shows conditional probabilities of working abroad in one of the three locations Europe, US, and rest of world, conditional on the study abroad treatment and the destination of the stay abroad. Based on 1,316 observations from graduate cohorts 1993, 1997, and 2001.

Table 2.11: Reasons for working abroad

	All	Study Abroad = 0	Study Abroad = 1	Difference in means (p-value)
Interest in Foreign Cultures	52.95	50.93	67.21	0.000
	(1.59)	(1.71)	(4.27)	
Received Interesting Offer	35.85	35.35	39.34	0.389
	(1.53)	(1.63)	(4.44)	
At Employer's Instance	33.40	34.07	28.69	0.239
	(1.51)	(1.62)	(4.11)	
Better Career Prospects in Germany after Return	25.36	25.81	22.13	0.382
	(1.39)	(1.49)	(3.77)	
Obtain Qualifications Abroad	16.80	16.86	16.39	0.897
	(1.19)	(1.28)	(3.37)	
International Research Project	14.77	14.65	15.57	0.788
	(1.13)	(1.21)	(3.30)	
Partner	10.90	9.77	18.85	0.003
	(0.99)	(1.01)	(3.56)	
Employment Outlook Abroad	8.66	8.02	13.11	0.061
	(0.90)	(0.93)	(3.07)	
Career Prospects Abroad	6.52	5.70	12.30	0.006
	(0.79)	(0.79)	(2.99)	
Number of Observations	982	860	122	

Note: Based on all respondents from the 1997 follow-up survey who have work experience abroad. Table shows percentage of respondents who indicate that a particular reason led them to take up work abroad. Example: 50.93% of respondents indicate that interest in foreign cultures led them to take up work abroad.

3 Does Tracking Exacerbate the Role of Family Background for Students' Test Scores?

3.1 Introduction

The results from the Programme for International Student Assessment (PISA) have triggered a serious debate about the functioning of education systems.[40] An important finding of PISA is the widely varying influence of a student's family background on her educational achievement across different countries. There are a number of possible explanations for this phenomenon, with features of the schooling system being widely seen as key factors. A frequently discussed feature is ability tracking of students. Ability tracking, or simply tracking, means placing students into different school tracks according to some measure of their ability. For the purposes of this chapter I refer to tracking as a policy which places students of different abilities in different *schools*.[41] In practice, the use of tracking varies greatly across countries; some educate all children in the same type of school until high-school graduation while others separate children as soon as they reach the age of 10. Differences in tracking policies and the widely varying impact of family background on educational outcomes across different countries has lead politicians but also people involved in educational research to argue that tracking has a causal impact on the importance of family background for educational achievement.

One of the main theoretical arguments is that children develop gradually and ability levels are difficult to observe at early ages. In a system that tracks students early, children from higher socioeconomic backgrounds may be more likely to be placed in academic tracks for reasons independent of their true ability. The reasoning behind this argument is that parents from higher socioeconomic backgrounds are more aware of the importance of sending their children to the more academic school tracks.

Also in untracked schooling systems do parents from higher socioeconomic backgrounds have the possibility to ensure a better education for their children; albeit through different channels: parents may move closer to a better school or place their children in a private school. Whether tracking actually exacerbates the importance of parental background for educational achievement is ultimately an empirical question.

The findings of PISA and other cross-country educational studies show a positive correlation between tracking and the importance of the family background for students' test scores. Nonetheless, I argue in this chapter that it may be overhasty to interpret this correlation as a causal

[40] I thank Holger Breinlich, Rocco Macchiavello, Steve Machin, Ralph Ossa, Matthias Parey, Giacomo Rodano, Justin Smith and especially Steve Pischke for very helpful discussions and comments. I also thank seminar participants at the LSE and the CEPR conference on the Economics of Education in Padua for helpful suggestions.

[41] Placing students of different abilities into different classes within a school type is less strong form of tracking. In this paper, however, I do not analyze this within-school tracking.

effect. In fact, I use a difference-in-differences methodology to show that tracking does not exacerbate the importance of parental background for students' test scores once controls for pre-tracking differences are added to the econometric specification.

I use cross-country differences in tracking policies to identify the impact of tracking on the importance of family background for students' test scores using data from three large international educational studies: PISA (Programme for International Student Assessment), PIRLS (Progress in International Reading Literacy Study), and TIMSS (Third International Mathematics and Science Study).

The following two graphs illustrate the effect I estimate in this chapter. For Figure 3.1, I first regressed post-tracking reading test scores on the three parental background characteristics shown on the vertical axes of the graphs, separately for each country. I then plot each country's coefficient against its tracking grade for those background characteristics.[42]

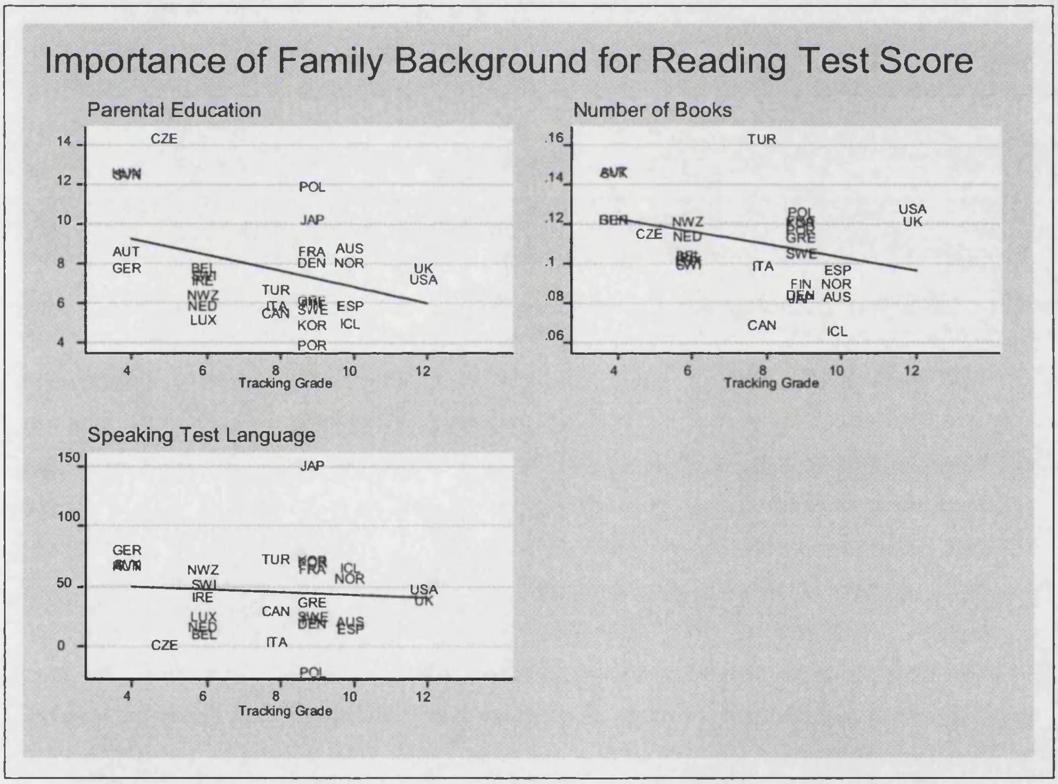

Figure 3.1: Family Background and Tracking PISA 2003 (Grades 9/10)

Figure 3.1 shows clearly that background has a higher impact on reading test scores in early tracking countries for all parental background measures. In Figure 3.2 I use the same methodology as for Figure 3.1 but replacing PISA reading scores with TIMSS mathematics scores. This figure shows that the same relationship exists for mathematics.

[42]The estimating equation is: $TestScore = \beta_1 + \beta_2 ParentBackground + \beta_3 StudentCharacteristics + \epsilon$. The β_2 coefficient for each country is then plotted on the vertical axis of each graph.

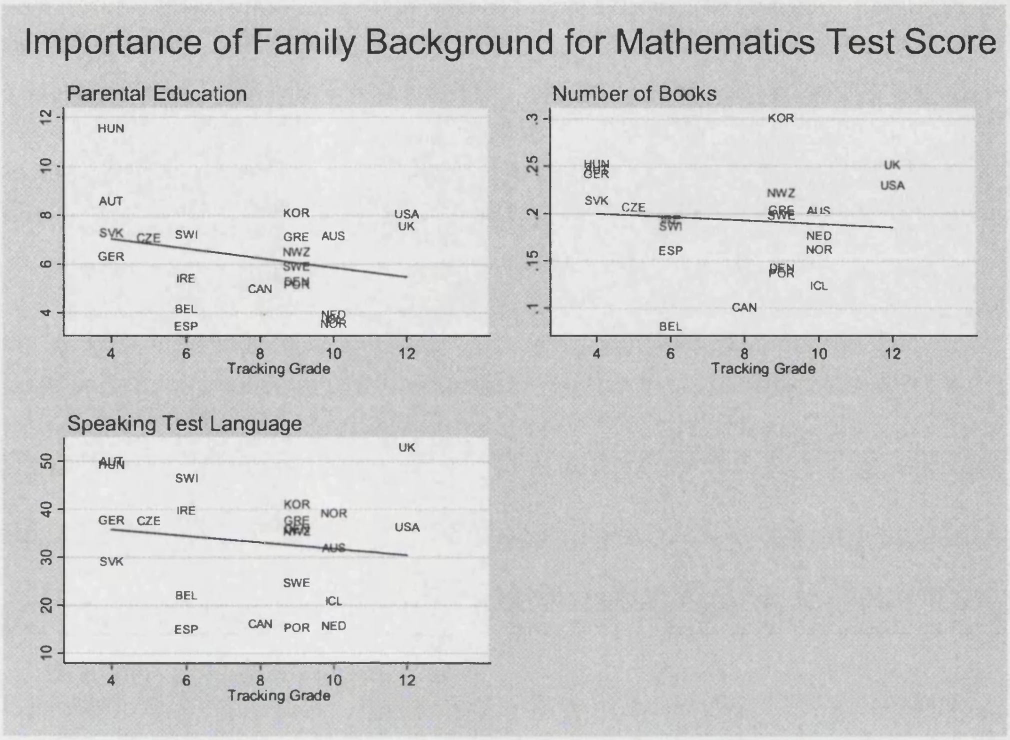

Figure 3.2: Family Background and Tracking TIMSS 1995 (Grades 7/8)

The visual evidence from Figures 3.1 and 3.2 clearly indicate that family background is more important for secondary school students' test scores in early tracking countries. The fact that similar relationships exist for different studies (PISA and TIMSS, which are carried out by different organizations and use different methodologies) hints that this relationship is not a random feature of any particular dataset. The graphs do not, however, provide evidence about whether tracking has a causal impact on the importance of parental background on students' test scores. It may well be the case that there exist unobserved country-level factors which affect the impact of parental background on educational test scores. If these unobserved factors are correlated with a country's tracking regime, estimates using a simple cross-section of students would be biased.

A simple way to investigate the role of unobserved factors is to repeat the graphical analysis from Figures 3.1 and 3.2 using tests administered to students before tracking has taken place in any of the countries in the sample. In the absence of anticipation effects there should be no relationship between the tracking regime (which affects students after taking the test) and the importance of the students' parental background. For reading, I use data from the Progress in International Reading Literacy Study (PIRLS), which tested primary school students in grade 4. It is important to note, that none of the countries in the sample tracks students before the end of grade 4. Applying the same techniques used in the previous graphs, Figure 3.3 shows the relationship between the importance of family background for test scores and the tracking grade (which will eventually affect students) for this new data. Figure 3.4 then repeats this exercise with the grades 3 and 4 mathematics data from TIMSS.[43]

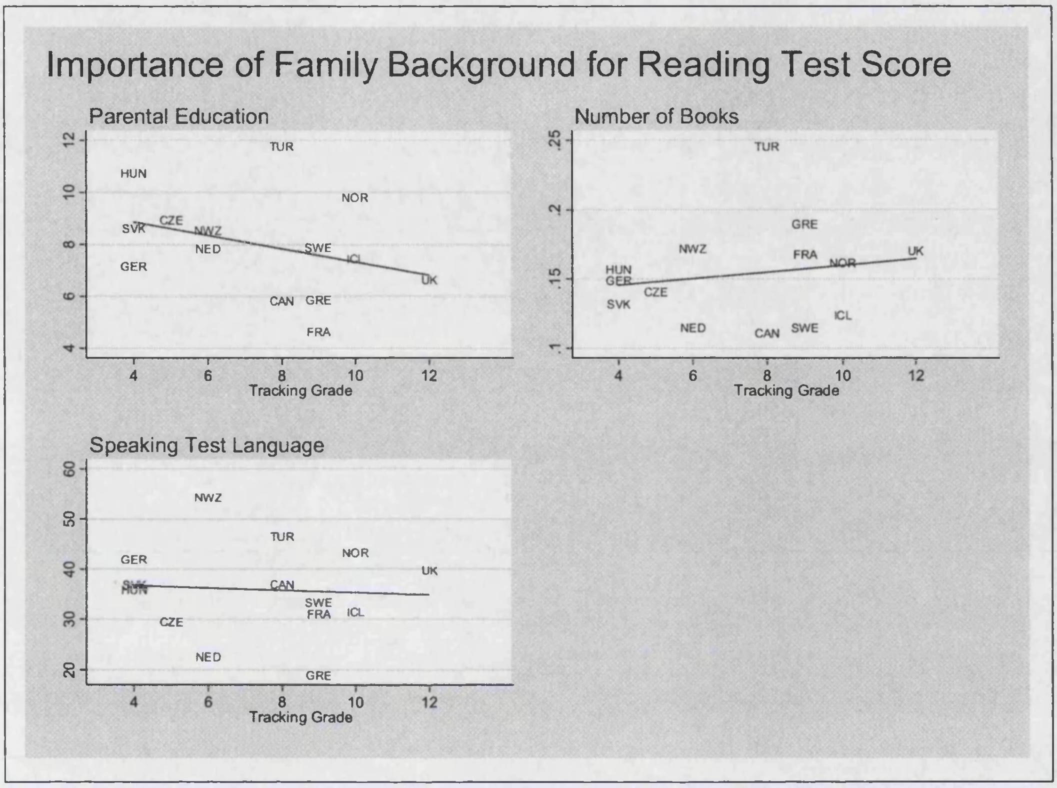

Figure 3.3: Family Background and Tracking PIRLS 2001 (Grade 4)

Apart from the number of books variable for the reading test score, there is a negative relationship between the importance of parental background and the tracking regime even before tracking has taken place in any country. This suggests that there are unobserved factors affecting the impact of parental background that are correlated with a country's tracking regime.

[43]Unfortunately, the TIMSS data for the primary school grades does not include parental education. Therefore, I omit this variable from the graphical analysis for mathematics test scores of primary school students.

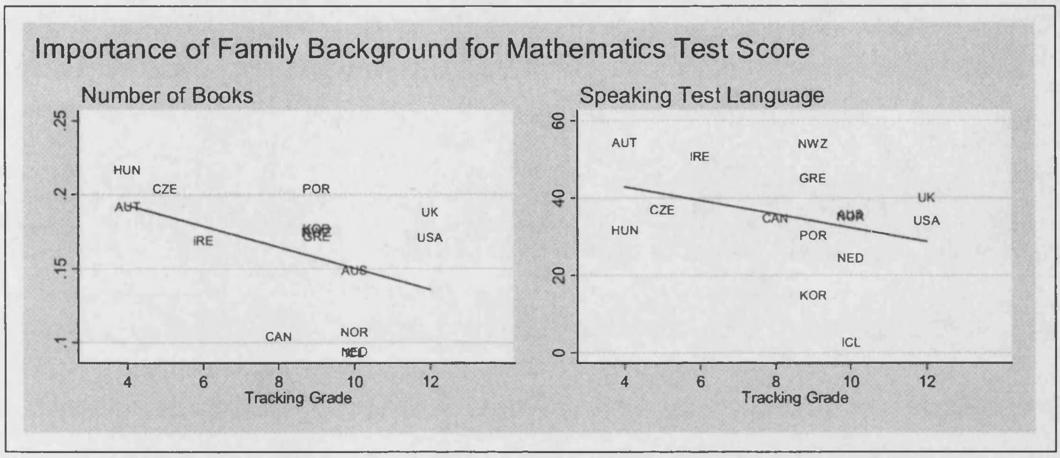

Figure 3.4: Family Background and Tracking TIMSS 1995 (Grades 3/4)

Investigating the effect of tracking therefore requires an identification strategy that controls for these preexisting differences across early and late tracking countries. To address this problem, I use a difference-in-differences (DiD) methodology. The DiD results presented below indicate that, once the "pre-tracking" level of the family background effect on children's test scores is controlled for, tracking no longer affects the impact of family background.[44]

These results cast serious doubt on the conclusions of a number of concurrent papers, such as Hanushek and Woessmann (2006) or Ammermueller (2005), which find that tracking increases educational inequality and exacerbates the effect of family background for students' test scores. I show, however, that slight changes in tracking variables, samples, or specifications renders the results of the relevant cross-country studies insignificant.

Understanding whether ability tracking intensifies the impact of parental background is important not only for educational researchers but also for policy makers. Countries that would like to increase educational equity should delay tracking if it increases the importance of family background for educational outcomes. Delaying the tracking age, which is often suggested as a way to reduce the link between parental background and educational outcomes, has been an important aspect of school reform in a number of different countries. Since the 1960s, the UK, some Scandinavian countries, Spain, and most recently Poland delayed their tracking age. If tracking, however, does not intensify the importance of family background, any such move would induce large costs without benefit. Careful studies on the effect of tracking are therefore needed to understand its effect on educational inequality.

The remainder of this chapter is organized as follows. Section 3.2 provides a review of the relevant literature. Section 3.3 presents the identification strategy used to identify the causal effect of tracking. Section 3.4 discusses the data coming from a number of cross-country school studies: PIRLS, PISA and TIMSS. Section 3.5 presents the main regression results for reading and mathematics test scores separately. Section 3.6 probes the sensitivity of my findings and

[44] It is worthy to note that this method does not control for anticipation effects of parents or students which may affect the importance of family background already before tracking has taken place.

Section 3.7 discusses these results and concludes.

3.2 Previous Research

There are a number of studies that investigate the effect of tracking on educational equity. I classify these studies into three broad categories: those using educational reforms of the tracking grade within a country, those using existing within country variation in tracking, and those using cross-country differences in tracking regimes to identify the effect of tracking.

A widely studied educational reform is the abolishment of ability tracking in Britain during the 1960s and 1970s (see for example Galindo-Rueda and Vignoles (2005)). In spite of the attention paid to this reform, Pischke and Manning (2006) show that no existing study has successfully solved selection effects into tracked and untracked schools during the transition period from a selective to a comprehensive schooling system.

Another strand of the literature uses existing within-country variation in tracking to identify its effect on educational equity. Using data mostly from the United States, the general finding is that there are at most small effects of tracking on educational equity.[45] Papers exploiting within-country variation of tracking, however, suffer from three main problems. First, the tracking measure is based on teachers', headteacher's or students' judgements about whether a school tracks students. It is not clear whether these measures for tracking are treated consistently across schools. A second problem with these studies is that unobserved factors that affect test scores (student motivation, for example) could be correlated with attending a tracked or an untracked school, producing biased results. A third problem is that tracking regimes have little variation within a country; there is much more variation in tracking regimes across different countries.

These problems have led a number of researchers to use cross-country variation in tracking to identify the effect of tracking. The reasoning is that data from different schooling systems may alleviate the problem of having a noisy tracking measure. Furthermore, one may deal with the second problem stated above, as it is unlikely that individual student unobservables are correlated with a country level measure for tracking. These advantages, however, come at the expense of another omitted variable bias problem. It could be the case that the cross-country variation in tracking is correlated with country-level unobservables, which influence test scores.

The increasing availability of data with internationally comparable test scores has triggered research on the effect of tracking on students' test scores in the very recent past.[46] Hanushek and Woessmann (2006) carry out an analysis which uses cross-country variation in tracking to identify its effect on the within-country variance of educational test scores. They address the problem of unobserved country level variables by using a DiD approach. Their findings "provide (...) reasonably strong support for the disequalizing effects of early tracking." One of

[45] See Betts and Shkolnik (1998), Rees, Brewer and Argys (2000) and Figlio and Page (2000) for studies which try to control for sorting of students across tracked and untracked schools.

[46] Brunello and Checchi (2006) investigate the effect of tracking for outcomes later in life and find that tracking does not affect the importance of family background for reading literacy of adults. They find, however, that tracking exacerbates the effect of parental background for wages later in life.

the main problems of their study, however, is the use of country-level data of the dispersion of test scores, since this leads to extremely small sample sizes. Furthermore, I show below that using a different measure for the tracking regime renders their results insignificant. Their results are further weakened by restricting their sample to OECD countries, only.

There are two recent studies which are more closely related to the research presented here, as they are trying to investigate whether tracking affects the impact of the family background on students' test scores. Schuetz, Ursprung, and Woessmann (2005) interact a measure for a country's tracking system with the students family background to see whether tracking changes the impact of parental background for educational achievement. Using data on secondary school students tested in TIMSS they find that "the family-background effect is larger...the earlier a country tracks its students into different school types by ability." They try to control for confounding country-level factors by including some controls for other institutional features of the education system. It is virtually impossible, however, to control for all unobserved country level factors that are correlated with the tracking measure and affect the importance of parental background on students' test scores. Any unobserved factor may bias the results, casting doubt on the findings of studies that do not control for pre-tracking differences in the importance of parental background for students' test scores.

The second paper looking explicitly at the importance of the socioeconomic background is a study by Ammermueller (2005). Using PISA 2000 and PIRLS data, he finds that "(T)he social origin of students ... increases its effect on student performance in countries with a differentiated schooling system...". Ammermueller's tracking measure uses the number of tracks in a schooling system. This measure does not capture the differential timing of tracking across different countries, which should have an impact on the influence of tracking. In checking the robustness of his results, I show that using a slightly different specification I do not find an effect of tracking even using Ammermueller's tracking measure.

3.3 Identification

Like recent research on ability tracking, I exploit the fact that different countries have different tracking policies. I use cross-country variation in tracking to identify the effect of tracking on the importance of parental background for children's test scores. One strategy to investigate whether tracking exacerbates the impact of parental background for educational achievement would be to estimate a standard education production function adding an interaction term of the parental background variables with the tracking regime. This is the approach used by Schuetz, Ursprung, and Woessmann (2005) and Brunello and Checchi (2006). They estimate an equation similar to equation (1):

$$(1) \qquad T_{isc} = \beta_1 + \beta_2 ST_{isc} + \beta_3 F_{isc} + \gamma_1 (F_{isc} * ET_c) + \beta_4 SQ_{sc} + \beta_5 C_c + \varepsilon_{isc}$$

Where T is a test score for individual i in school s and country c. ST is a vector of individual characteristics of student i in school s and country c. F is a vector of family characteristics of student i in school s and country c. ET is a dummy variable indicating whether a country tracks students at an early stage of their student life. SQ is a vector of school quality variables of school s in country c. C is a vector of country dummies. ε is an error term. The main interest then lies in obtaining consistent estimates of γ_1; the interaction of the family background measures with the tracking regime. Finding γ_1 to be positive would then be evidence that parental background is more important in early tracking countries.

As in most cross-country studies, the major concern is that any country level variable (in this case the tracking measure) is correlated with unobserved country level variables. This is not an issue as long as these variables do not affect the importance of parental background for students' test scores because the regression includes country fixed effects. It is quite likely, however, that some country level unobservables affect the influence of family background on test scores, like for example pre-primary care. The coverage of pre-primary care may be correlated with the tracking regime. At the same time, pre-primary care may affect the impact of the family background on students' educational achievement and thus biasing the estimates of in equation (1). The graphical analysis presented in the introduction already indicates that unobserved variables may pose a severe problem in this context. I showed above that parental background seems to be more important in early tracking countries even before actual tracking has taken place.

To solve this problem, I use a difference-in-differences strategy, with test scores taken at two points of a child's educational career. The first point is in primary school, before tracking has taken place in any of the countries in the sample. The second is in secondary school, after tracking has occurred in some countries. I then compare the change between the early and the late test in the importance of family background in early versus late tracking countries.

This is a legitimate strategy to control for unobserved country level variables under the identifying assumption that the unobserved country characteristics do not change between the primary and secondary school grades.[47] In a regression framework this difference-in-differences methodology is implemented by estimating equation (2).[48]

$$\begin{aligned}(2)\qquad T_{isct} = {} & \beta_1 + \beta_2 ST_{isct} + \beta_3 F_{isct} + \beta_4 SECONDARY_t + \beta_5 SQ_{sct} + \beta_6 C_c \\ & + \gamma_1 (F_{isct} * ET_c) + \gamma_2 (F_{isct} * SECONDARY_t) \\ & + \gamma_3 (F_{isct} * ET_c * SECONDARY_t) + \varepsilon_{isct}\end{aligned}$$

The abbreviations are the same as for equation (1). The subscript t indicates the two cohorts: primary school students and secondary school cohorts. Compared to the first specification this specification now adds the dummy variable SECONDARY, which indicates that an observation is taken from the late test examining secondary school students. This variable controls for any

[47] It is worthy to note that the differences are not taken for the same individual. Therefore, a further identifying assumption is that the random assignment of the studies ensured that they tested a representative sample.

[48] In results not reported here I also allow the student characteristics to vary across the two different studies. This does not affect the findings on γ_3.

systematic difference between the two test scores.[49] The main interest now lies in identifying the coefficient γ_3. If turns out to be positive family background becomes relatively more important in early tracking countries between primary and secondary school. If γ_3 was negative family background becomes relatively less important in early tracking countries after actual tracking has taken place. This model is estimated using the pooled data from a study testing primary school students and a study testing secondary school students. To allow for arbitrary correlations of the error term for students within one country all standard errors are clustered at the country level. The following part describes the data I use to estimate equation (2).

3.4 Data

3.4.1 Data on Test Scores, Student Characteristics, Family Characteristics, and School Quality Variables

The data on test scores, student characteristics and family background originates from the microdatasets of three school studies. The data on reading skills comes from PISA and PIRLS, while the data on mathematics comes from TIMSS. I only use data from OECD countries because I want to compare countries with a similar development of the educational sector. Table 3.1 gives an overview of the countries in the different estimation samples.

Data for the Reading Results

To implement the DiD strategy, I use two reading test scores taken at different points in time of the students' school career. Firstly, I use test scores from PIRLS 2001, a study testing students before tracking has occurred in any country. This study was carried out by the International Association for the Evaluation of Educational Achievement (IEA) in the year 2001. PIRLS tested students in grade 4 of primary school, before tracking has taken place in any country of the sample. Each student obtained a test score that was scaled to have an international mean of 500 and a standard deviation of 100. In addition to testing the reading skills of primary school students, the students' parents were asked to provide information on the student and on family background characteristics. Every participating school had to provide information on class sizes and other measures of school quality.

Secondly, I use a test score from a study testing students after tracking occurred in some countries. For the reading results I use data from PISA in the years 2000 and 2003. Administered by the OECD, this study evaluated reading skills of 15-year-old students, usually attending grades 9 or 10 of secondary school. Like PIRLS, the PISA study was scaled to have an international mean of 500 and a standard deviation of 100.[50] The information on the parental

[49] As both test scores may not have tested the same skills one has to assume that systematic differences in the two tests are uncorrelated with the family background measures and the tracking regime to obtain consistent estimates of γ_3.

[50] The scores obtainable in the database are plausible values, which are not exactly test scores. "They are random numbers drawn from the distribution of scores that could be reasonably assigned to each individual...". Refer to the Technical Reports of PISA for more information.

background variables and the student characteristics is taken from the student questionnaire; the information on school quality variables from a questionnaire which had to be answered by each participating school.

The DiD estimation requires a primary school and a secondary school test score for each country. Therefore I can only use observations from countries that participated in both the PISA 2003 and the PIRLS studies in the main specification for reading.[51] The second column of Table 3.1 lists all countries in this specification. Column 3 lists the countries used in a robustness check with data from PIRLS and PISA 2000.

Data for the Mathematics Results

To estimate the specification for the mathematics results I use data from TIMSS. Carried out by the IEA, TIMSS 1995 tested mathematics and science skills of two different age cohorts (one cohort of primary school students and one of secondary school students) across different countries. I use the test results of the primary school cohort (attending grades 3 or 4) as an early test score before tracking has occurred in any of the participating countries. TIMSS also tested the mathematics skills of secondary school students attending grades 7 and 8, which I use as the later test score after which tracking has taken place in the early tracking countries. Again, I complement the data on test scores with data from student questionnaires and from the answers to the school questionnaires. The countries in this sample are shown in column 4 of Table 3.1.

In 1999 the primary cohort was re-tested when they were in grades 7 or 8. As a robustness check for the mathematics results, I combine the TIMSS data from grades 3/4 of 1995 and grades 7/8 from 1999 as can be seen from the last column of Table 3.1.

Parental Background Variables Used for Estimation Purposes

For my estimation I rely on the parental background data available in the above datasets. An important parental background factor for children's educational attainment is parental education. More educated parents may be better informed about good parenting practices and may create a home environment, which stimulates the learning of their children. In the tables reported below, I use highest parental education, which is measured as the years or education of the parent with more education. To save space I do not report the specifications where I use father's and mother's education separately. The results are very similar and do not affect any of the conclusions drawn below.

Furthermore, I use the number of books in a student's home as an alternative measure for parental background. This variable is frequently used by educational researchers, and may

[51] In PIRLS England and Scotland are sampled independently. PISA, however, did not sample them separately. To estimate my results I combine the information of these two countries. Furthermore, in PIRLS Canada is represented only by the provinces Ontario and Quebec whereas PISA sampled students from all Canadian provinces. In the reported results for the difference-in-differences specification I include the combined data on Great Britain and data on Canada. Discarding all observations from these two countries does only have a small effect on the estimated coefficients and does not affect the significance or insignificance of the results.

capture parents' valuation of education or serve as a proxy for income as books are consumption goods.[52] Finally, I also use a variable indicating whether children speak the language of school instruction at home as another measure of parental background.

3.4.2 Data on Tracking

I complement the data on test scores, student characteristics, parental background, and school quality variables with a country level measure of the tracking regime. Some countries educate students in the same type of school up to the end of secondary school. Other countries, however, separate students at early stages of their schooling career. Figure 3.5 demonstrates the tracking policy of a representative late tracking country, Finland, and a representative early tracking country, Austria. Whereas Finland does not track students up to the end of grade nine, students in Austria are placed in different school tracks after four years of schooling.

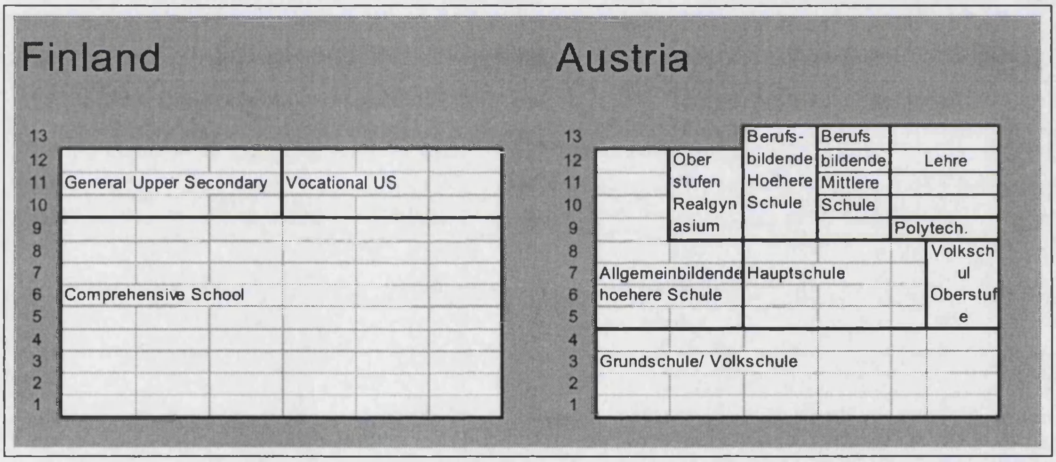

Figure 3.5: Tracking Policies in Finland and Austria

I construct a measure that indicates after how many grades tracking occurs in each of the countries of my sample. For the purposes of this chapter I define tracking as educating students in different types of schools. This constitutes the strongest form of tracking. Therefore, I do not consider specialization tracks for certain subjects within a school as tracking. Instead, I focus on a very strong definition because the effects of tracking are potentially largest for this stark form of tracking. Furthermore, other definitions would result in a much more noisy tracking measure.[53] Table 3.2 gives an overview of the grade after which tracking takes place in the countries of my sample.

My tracking measure is very similar to the one developed by Hanushek and Woessmann (2006).[54]

[52]See Schuetz et al. (2005) for a discussion on using the number of books as a measure for parental background.

[53]Some countries exhibit within-country variation of tracking; with some regions tracking students at different grades than others. Unfortunately, I cannot distinguish different regions in the PISA, PIRLS and TIMSS databases. I therefore assign the grade at which most regions track as the tracking measure for these countries.

[54]I thank Ludger Woessmann for kindly providing me with their measure of tracking.

Their measure captures the age at which tracking takes place. Mine, on the other hand, relates to the grade after which tracking takes place, which will capture differences in the school starting age across countries. They classify some countries differently to my classification, yet a comparison of the two measures indicates that they are quite similar, which is indicated by the correlation of .81 between the two measures as reported in Table 3.3. I show, in fact, that my results hold up when I use the Hanushek and Woessmann tracking measure.
Alternatively, Ammermueller (2005) uses a tracking measure that counts the number of school tracks of a country in lower secondary education. This does not capture how many years a student is exposed to ability tracking, which is a disadvantage as it is likely that possible effects of tracking are stronger the longer students are educated in different tracks. Nevertheless, this measure has an advantage over measures using tracking grade or tracking age, as it captures how many school tracks exist in each country. By construction, higher values of the number of school track index indicate more tracking. According to my measure, however, a higher tracking grade indicates less tracking. Therefore the two measures should be negatively correlated. Table 3.3 confirms a negative correlation of the two measures of -0.75. I show below that my results hold even if I use the number of tracks measure.
To estimate equation (2), I define an early tracking threshold that indicates whether a country tracks students early or late. I vary the early tracking threshold between tracking after grade 4 and tracking after grade 8. I show below that my conclusions do not vary if I use different threshold levels for early tracking, indicating that the results are not driven by an arbitrary choice of the early tracking threshold.
For the countries with later tracking it is sometimes problematic to define the correct tracking grade. To overcome this problem, I use a dummy variable early versus late tracking. This provides a good measure because it is not affected by whether a later tracking country tracks at grade 9 or grade 10. The measurement problems are much less severe for the early tracking countries, as most of these have very clear tracking rules. In particular, the grade 4 to grade 6 thresholds provide measures of tracking, which should be free from major measurement error. I use these tracking measures to identify the effect of tracking on the importance of parental background for educational attainment, with results presented in the following section.

3.5 Main Results

3.5.1 Reading Results

I use data from PIRLS as the early test score (grade 4 students) and data from PISA 2003 (grade 9 or 10 students) as test score of secondary school students to estimate the reading results. Pooling the data from these two educational studies, I estimate equation (2), and present results in Table 3.4.[55] The specification used to generate these results takes grade

[55] All specifications reported in the table include school quality variables. Excluding school quality increases the sample in each country because of fewer missing values and also allows to keep France in the estimation sample. Results from these regressions are reported in Table 3.13 of the appendix.

5 as the early tracking threshold: countries which track at the end of grade 5 or before are classified as early tracking countries and countries tracking at later stages of a student's career are classified as late tracking countries. I show below that the results are robust to different definitions of the early tracking thresholds, such as grade 4, grade 6 or grade 8.

To summarize the results, the coefficients on the student characteristics have the expected signs and are all highly significant in all specifications. Females do significantly better than males in reading, and native pupils do better than immigrants. Not surprisingly, children living with only one of their parents or those who live without their parents (because they live with other guardians) do significantly worse than students who live with both parents.

In column (1) I report the specification that uses highest parental education as the sole parental background variable. Students whose better-educated parent has one more year of education score about 6 points better in reading. Interestingly, the importance of parental education for reading is as important for primary school students as it is for secondary school students. This is verified by inspecting the very small and insignificant coefficient on the interaction of parental education with a dummy for the secondary school test. The positive and significant interaction of parental education and an indicator for early tracking countries, on the other hand, indicates that parental education is more important in early tracking countries. The point estimate shows that an increase of one year of parental education (for the better educated parent) increases the student's reading test score by about 5 points more in early tracking countries compared to countries with later tracking grades. This coefficient just indicates that parental education is more important in early tracking countries. Whether early tracking itself is causing parental education to be more important or whether other unobserved country level factors, that are correlated with the tracking regime, play a role has to be investigated in greater detail. To assess this directly, I include the triple interaction of parental education, an indicator for early tracking, and a dummy for the secondary school test into equation (2). To make this variable more visible in the tables it is reported in bold characters. The negative and insignificant coefficient on this interaction indicates that the importance of parental education for students' test scores does not increase between the grades tested in PIRLS and PISA in early tracking countries. This suggests that parental background does not become more important after actual tracking has taken place.

Column (2) of Table 3.4 reports the results from a specification using the number of books in the students' home as the relevant family background variable. The coefficient on the number of books variable indicates that an increase of 100 books in the student's home increases her test score by 13 points.[56] The results on the interaction of the number of books variable with the secondary school dummy indicates that the number of books in a student's home is more important for secondary school students than for primary school students. The positive and

[56]The number of books variable is coded in 6 categories. I assigned each student the category midpoint as the number of books at her home. This imposes some restrictions on the functional form of the relationship between the number of books and test scores. Reestimating this specification by using dummy variables for each book category gives similar results, namely that the number of books is significantly more important in early tracking countries. It does not become more important after actual tracking has taken place, however. For the sake of clear exposition I only report the results from the continuous number of books measure.

significant interaction between the number of books variable and the indicator for early tracking countries demonstrates that the number of books in a student's home is more important in early tracking countries. The fact that the triple interaction of the number of books, the indicator for early tracking, and the dummy for the secondary school test is negative and insignificant shows that the number of books in a student's home do not become more important in early tracking countries after actual tracking has taken place.

In column (3) I report the specification using an indicator whether the student speaks the test language with his parents. Speaking the test language at home is a significant and important factor of doing well in reading. The interaction of the language indicator with the secondary school dummy shows that this factor is less important in secondary school compared to primary school. This is not surprising given that secondary school students have many more opportunities to speak the language of the country they live in with people other than their parents. The positive and significant interaction of speaking the test language and the indicator for early tracking countries shows that speaking the test language is more important in these countries compared to countries which track later. Once again, however, the triple interaction of speaking the test language, the early tracking dummy, and an indicator for the secondary school test is negative and insignificant.

Including all parental background measures in the same specification does not affect the conclusions drawn before. The results reported in column (4) indicate that the importance of parental background does not increase in early tracking countries after actual tracking has taken place. These results therefore cast serious doubt on interpreting the correlation of tracking and the importance of the family background as causal.

3.5.2 Mathematics Results

To further investigate the role of tracking in intensifying the importance of parental background, I present results with mathematics scores as the outcome. Unfortunately, TIMSS 1995 did not assess parental education for the students in the primary school cohort. Therefore, I only report results for the number of books and the language measures of parental background.[57]

Table 3.5 presents the results from estimating equation (2) using the TIMSS data. The coefficients on the student characteristics all have the expected signs and are similar to the ones obtained for the reading test score. The main differences are that being native matters less for mathematics, and that girls do worse in mathematics.

In the first column I use the number of books in a pupil's home as the relevant parental background measure, which appears to be an important factor affecting student performance. Increasing the number of books by 100 increases a student's test score by about 15 points. The positive and significant coefficient on the interaction of the number of books variable and an indicator for the secondary school cohort indicates that the number of books are more important

[57] Again these results are reported for the sample including the school quality controls. The results without school quality data which result in a larger sample with more countries are reported in Table A2 in the appendix. As can be seen, the results do not differ in any significant way.

in later stages of a student's career. The coefficient of interest, the triple interaction, is negative and marginally significant, indicating that family background becomes less important in early tracking countries after tracking has taken place.

The specifications reported in column (2) of Table 3.5 use an indicator for speaking the test language at home as the relevant parental background measure. Speaking the test language at home has a strong and significant effect on mathematics test scores, but seems to be less important in secondary school. The coefficient on the interaction of the language indicator and the dummy for the secondary school cohort, however, is not significant. Speaking the test language at home seems to be more important in early tracking countries but also the coefficient on the interaction of the language dummy with the indicator for early tracking countries is not significantly different from 0. The coefficient on the triple interaction again is not significantly different from 0 and has a point estimate which is very close to 0. Speaking the test language at home does not become more important in early tracking countries after actual tracking has taken place.

Including both family background measures at the same time does not affect any of the above conclusions as can be seen be inspecting the results reported in column (3) of Table 3.5.

The results for mathematics confirm the reading results presented before. There is clearly no support for the hypothesis that tracking exacerbates the importance of family background after actual tracking has taken place.

3.6 Sensitivity Analysis

These results run contrary to the findings of the existing studies using cross-country data to investigate the impact of tracking on educational equality. This section therefore investigates the robustness of my findings. My first test uses different thresholds for the grade identifying early versus late tracking countries. The results of these regressions are reported in Tables 3.6 and 3.7 for the reading and mathematics test scores respectively. To save space I only report the results for the specification using all parental background variables in the same regression.[58] For the reading test score this is the equivalent to the specification reported in column (4) of Table 3.4. Table 3.6 shows that the coefficient on the triple interaction term of family background, early tracking, and the dummy indicating whether the observation comes from the secondary school (PISA) sample is hardly ever significantly different from zero. The only exception is the specification using grade 4 as the early tracking threshold. In this case the coefficient on the triple interaction for parental education is marginally significant but with a negative coefficient. This would indicate that parental education becomes less important in early tracking countries after actual tracking has taken place. The results reported in Table 3.6 do not support the hypothesis that tracking exacerbates the importance of family background for students' test scores.

Table 3.7 reports the results for the mathematics test score. Using the end of grade 4 as

[58]Using the other specifications does not affect the conclusions drawn from this exercise.

the threshold for early tracking, the coefficients of the triple interactions indicate that family background becomes more important in early tracking countries between grades 3/4 and grades 7/8. Using any other grade as the threshold to divide the sample into early versus late tracking countries I find no evidence that the importance of family background increases between grades 3/4 and grades 7/8.

As mentioned above, there is more than one way to measure the extent of tracking. Hanushek & Woessmann and Schuetz et al., for example, use the age at which tracking takes place as their measure. Ammermueller defines tracking slightly differently by the number of school tracks that exist during secondary school in a certain country. Table 3.8 compares the results from using my tracking measure (grade after which tracking occurs), and the other two defined in this paragraph. The triple interactions of the family background variable, the tracking measure, and the dummy indicating whether the observation is from the secondary grades are mostly not significantly different from zero, independent of the tracking measure used in the estimation. The results presented in column 2 of Table 3.8 show that the number of books at home is relatively less important in countries with later tracking ages in grades 9/10 compared to grade 4. This result, however, is only significant at the 10 percent level. Speaking the test language seems to be less important in later grades in countries with more tracks (thus more tracking). Again, this coefficient is only significant at the 10 percent level. These two marginally significant triple interactions propose conflicting conclusions on whether more tracking increases or reduces the importance of family background for children's reading test scores. All other coefficients on the triple interaction terms are not significantly different from zero and indicate no effect of tracking after tracking has taken place.

For the mathematics test score, with results reported in Table 3.9, none of the coefficients on the triple interaction terms is significantly different from zero. Furthermore, the point estimate of these coefficients is always close to 0. There is no evidence that tracking influences the importance of parental background for educational attainment after tracking has taken place.

Another worry may be that factors varying at the country level and affecting the importance of the family background are not constant over time, and would thus not be controlled using the fixed effects strategy. Using tests administered in alternative years may address this problem. Because PISA 2003 and PIRLS 2001 were not carried out in the same year, there may have been changes in the schooling systems affecting the importance of the family background on students' test scores between 2001 and 2003. Unfortunately there are no comparable international pupil ability tests testing reading skills of different cohorts in the same year. Nonetheless, using the PISA 2000 data instead of the 2003 wave of PISA may be a suitable way to deal with this problem. This is because changes between 2001 and 2003 will not have had an effect on the PISA 2000 results.

Table 3.10 reports the results from the same specifications as reported in Table 3.4 but using PISA 2000 and PIRLS 2001 as the data sources. The coefficients on the triple interactions show that there is no evidence that the importance of family background increases relatively more in early tracking countries between grade 4 and grades 9/10.

The DiD results for the mathematics test score presented above were estimated using the 1995 wave of TIMSS, which tested students of both the grades 3/4 and grades 7/8 cohorts. Nonetheless, these results may be problematic because the factors affecting the importance of family background for mathematics test scores may have changed in a year shortly before 1995. Students from the two cohorts may have been exposed to this changing environment at a different age. I use the 1999 wave of TIMSS which tested students in grades 7 or 8 to address this potential problem. Table 3.11 reports the results from this exercise. Note that the number of countries which can be used for estimation purposes is very small, so any conclusion drawn from this exercise should be handled with caution.[59] Again, the triple interactions are almost always insignificant. The only exception is the interaction involving the language spoken at home if considered as the only family background variable which is reported in column 2. In this case, the coefficient indicates that family background becomes relatively more important in early tracking countries between grades 3/4 and grades 7/8.

3.7 Discussion and Conclusion

The results obtained from estimating equation (2) show that family background is more important in early tracking countries. I find, however, that family background does not become more important after tracking occurs in the early tracking countries. In the absence of anticipation effects this is evidence that tracking does not causally affect the importance of family background for educational attainment. While this result is interesting in itself, it is even more interesting that at the same time, two other papers find that tracking exacerbates educational inequalities using some of the datasets I use in this chapter. One study was carried out my Hanushek and Woessmann (2006) and one by Ammermueller (2005). I discuss the likely reasons for obtaining different results in turn.

Hanushek and Woessmann do not look at the specific effect of family background on test scores but look at whether tracking increases inequality measured by the within country standard deviation, the 75th-25th or the 95th-5th percentile differences of test scores. To address the concern of bias due to unobservables, they employ a similar DiD strategy to the one in this chapter: they look at the change in inequality between a primary school test score (no tracking in any country) and a test score for secondary school students (after tracking in some countries). In their main specification they use data on reading from the same data sources I use for my main specification on reading test scores. In order to assess the reasons of obtaining different

[59] I use a different set of student characteristics compared to all other regressions presented in this paper to estimate the specification reported in Table 11. In all other regressions I was using the same set of student characteristics: a gender dummy, age, a dummy whether the student was born in the country and dummies indicating whether the student lives with a singleparent or without parents. If I did include the dummies indicating the family structure in this estimation the sample would be reduced by about 30 percent and I would lose 3 out of 8 countries because 3 countries did not report the family structure variables. This affects the results in a non-random way. Therefore I omit the two dummies indicating whether the student lives with a singleparent or without parents. Table A3 shows that omitting these 2 variables does not have an impact on the results obtained from estimating the difference-in-differences results for the original TIMSS 1995 sample as can be seen from comparing the results presented in Table A3 and Table 5. This makes me confident that omitting these two variables is the preferred option for having a more representative sample.

findings, I replicate their results and show that these results are not stable if one considers a different measure for tracking and a slightly different sample. Column (1) of Table 3.12 reports the second column of their main table (Table 2 in Hanushek and Woessmann (2006)).[60] They regress the within country standard deviation (appropriately normalized) for secondary school students on an indicator for early tracking. As a measure for tracking they use a threshold of tracking before the age of 15. To control for pre-existing levels of inequality they also include the within-country standard deviation of test scores for primary school students. They conclude that inequality is higher for early tracking countries based on a positive and significant coefficient on the early tracking dummy as reported in the first column of Table 3.12. In column (2) of Table 3.12, I show, however, that using the same sample but a different tracking measure (here tracking at the end of grade 5 or before) the results are no longer significantly different from 0. Furthermore, using a sample of OECD countries only, which reduces the sample by only three countries, further weakens their results. Using small variations in their specification I can, therefore, no longer replicate their findings and obtain results which are in line with the results presented in this chapter. Namely, that tracking does not increase educational inequality.

My results are also slightly different from the ones obtained by Ammermueller (2005) even though the identification strategies used are very similar and he uses data from PISA and PIRLS as well. He finds that the number of books at a students home do not become more important in tracking systems with more tracks after actual tracking has taken place which is the same result as I find. He finds that speaking the test language becomes less important in countries with more tracks (and thus more tracking). This is similar to the finding on speaking the test language in this chapter of my thesis. On the other hand, he finds that students with native parents do better in tracked countries after actual tracking has taken place. Furthermore, he finds that the importance of parental education increases in countries with more tracking. Therefore 2 of his 4 results indicate that tracking does not affect or reduces the importance of family background for students' test scores. The results on the other 2 family background variables indicate that tracking exacerbates the importance of family background.

How can I reconcile this with my findings? Ammermueller uses a different definition for parental education, namely having a parent with a university degree whereas I use a linear years of education measure. Furthermore, he drops 2 countries (Canada and England) because not all regions in those countries were sampled. Re-estimating my results with this reduced sample does not change the conclusion from my work: tracking does not intensify the role of family background for students' education. The factor which probably explains a big part of the difference between our work is that I cluster my standard errors at the country level, while he is clustering the standard errors at the school level. Clustering at the country level allows for any arbitrary correlation of the error terms of students within one country. This is the level of clustering which is appropriate in this setting. Clustering at the school level leads to much smaller standard errors, and thus to significant coefficients which may well be insignificant if one clusters at the country level.

[60] I thank Ludger Woessmann for providing me with details on the way they estimate their results.

My results clearly indicate that the estimates in the existing literature are not stable to using slightly different tracking measures, samples, and specifications. This further increases the confidence in my finding that tracking does not exacerbate the importance of parental background for educational test scores. The question remains why tracking does not affect the importance of family background. As mentioned above, parents have the possibility to self select according to socioeconomic status even in untracked systems, so one path may be through residential segregation. This is exemplified by the literature on the effect of school quality on house prices which mainly uses evidence from the US and the UK - both untracked countries.[61]

Another way of improving a child's education even in untracked systems is to enrol the child in a better and possibly expensive private school. Furthermore, choosing certain subjects (e.g. learning ancient languages) may lead to better peers and thus better learning even in a system which has no official tracking policy. Thus even untracked systems give parents and students opportunities to select into better schools. Further research is needed to pin down the factors which enable parents to improve their child's education and may thus create inequalities in opportunity for children from different backgrounds. Nonetheless, my findings could be consistent with the view that tracking has a causal impact on the importance of family background for children's test scores if the above mentioned effects of tracking happened before actual tracking takes place. I use studies which tested students in grades 3 or 4 as a baseline level of the role of parental background. This is just a short time before some of the early tracking countries sort students according to ability. Parents may anticipate the effect of tracking and motivate their children to do better in school already before tracking takes place. My DiD methodology may fail to pick up these anticipatory effects of tracking. Given that my results are independent of the tracking threshold I choose may be an indication that anticipation effects are not very important. Research strategies which circumvent this problem may shed more light on the debate of the effect of tracking. I conclude, however, that the cross-country evidence to date, if carried out carefully, does not suggest that tracking exacerbates the importance of family background for students' test scores.

[61] See Black (1999) and Figlio and Lucas (2004) for careful studies of the effect of school quality on house prices.

3.8 Tables

Table 3.1: Countries in Different Estimation Samples

Secondary School Test Primary School Test	PISA 2003 PIRLS[1]	PISA 2000 PIRLS	TIMSS 1995 7/8[2,3] + TIMSS 1995 3/4[4]	TIMSS 1999 7/8 + TIMSS 1995 3/4
	Main Reading	Robustness Check Reading	Main Specification Mathematics	Robustness Check Mathematics
Australia			■□	■
Austria			□	
Canada	■□	■	■□	■
Czech Republic	■□	■	■□	■
France	□	■		
Germany	■□	■		
Greece	■□	■	□	
Hungary	■□		■□	■
Iceland	■□	■	■□	
Ireland			■□	
Italy	■□	■		
Korea			■□	■
Netherlands	■□	■	□	
New Zealand	■□	■	■□	■
Norway	■□	■	□	
Portugal			■□	
Slovakia	■□			
Sweden	■□	■		
Turkey	■□			
United Kingdom	■□	■	■□	■
USA			■□	■
Number of Countries	14/15	12	11/15	8

■ including school quality □ without school quality

[1]The US drops out because the parental background information is missing for all students in PIRLS. [2]Also France and Japan participated in TIMSS. As the information on being born in the country is missing for all observations they are not in my sample. [3]School quality is missing for all observations from Austria, Greece and Norway. [4]The information on school quality for the Netherlands is missing for the Grade 4 wave.

Table 3.2: Countries in the PISA 2003 and TIMSS 95 (Secondary) Samples, Tracking Grade, and Means of Relevant Variables

Tracking Grade	Country	Mean Test Score Reading PISA 2003	Mean Parental Education PISA 2003	Mean Number of Books PISA 2003	Speaking Test Language (%) PISA 2003	Mean Test Score Maths TIMSS 95-8	Mean Parental Education TIMSS 95-8	Mean Number of Books TIMSS 95-8	% Speaking Test Language TIMSS 95-8
4	Austria	497.4	13.1	170.2	91.5	535.3	12.4	136.9	88.5
4	Germany	497.5	13.1	206.7	92.8	498.5	10.6	140.3	88.4
4	Hungary	480.7	12.9	243.7	99.3	522.0	12.5	178.4	98.7
4	Slovakia	475.2	13.8	161.3	97.0	527.8	12.1	124.7	88.9
5	Czech Republic	505.4	13.9	251.9	99.1	538.7	11.8	164.3	98.5
6	Ireland	517.6	12.5	158.4	97.4	515.3	11.5	124.4	97.5
6	Netherlands	518.0	12.8	181.1	85.4	534.8	12.0	129.8	90.2
6	New Zealand	523.7	13.5	205.7	90.7	493.3	12.7	178.7	90.8
8	Canada	516.1	14.3	209.0	89.3	505.2	13.2	155.1	82.9
8	Italy	500.3	12.7	178.1	73.9				
8	Turkey	444.0	9.0	82.9	97.3				
9	France	500.1	12.0	162.9	93.9	518.1	11.5	118.0	94.7
9	Greece	467.2	13.0	140.3	96.3	464.0	10.7	95.7	95.5
9	Korea	533.4	12.4	172.1	99.9	592.4	11.8	127.7	95.3
9	Portugal	476.1	9.1	128.8	98.6	440.9	8.2	98.3	97.0
9	Sweden	513.2	13.5	236.6	92.7	513.7	12.9	177.2	91.3
10	Australia	524.1	13.1	232.6	91.9	524.3	12.3	181.7	92.2
10	Iceland	491.7	14.5	251.8	98.3	469.8	12.0	168.3	95.5
10	Norway	499.6	14.6	251.1	94.8	485.9	13.0	184.9	94.3
12	UK	512.0	12.9	175.5	96.9	485.0	12.0	133.8	94.2
12	USA	493.8	13.5	160.1	90.9	483.1	13.4	136.3	87.2

The differences in the means of the family background variables across PISA and TIMSS originate from the fact that the tests were carried out at different times, for students in different grades, and by different organizations with different questionnaires. The particularly big differences for the number of books variable can be explained by the fact that for PISA the top category for books is "more than 500 books", whereas for TIMSS the top category is "more than 200 books".

Table 3.3: Correlations of Tracking Measures

	Tracking Grade	Tracking Age	Number of Tracks
Tracking Grade	1		
Tracking Age	0.81	1	
Number of Tracks in Secondary School	-0.75	-0.69	1

Table 3.4: Difference-in-Differences Reading (PIRLS 2001 and PISA 2003)

Dependent Variable: Reading Score	(1)	(2)	(3)	(4)
Highest Parental Education (years)	6.16			4.68
	(0.81)***			(0.62)***
HPE*PISA	0.14			-0.39
	(1.05)			(0.82)
HPE*Earlytrack	4.78			3.44
	(1.27)***			(1.29)**
HPE*Earlytrack*Secondary	-1.28			-1.24
	(0.73)			(1.25)
Number of Books		0.13		0.10
		(0.02)***		(0.01)***
NoB*PISA		0.06		0.06
		(0.02)**		(0.02)**
NoB*Earlytrack		0.05		-0.01
		(0.02)**		(0.02)
NoB*Earlytrack*Secondary		-0.03		0.04
		(0.02)		(0.03)
Language			38.35	33.51
			(4.23)***	(3.76)***
Language*PISA			-18.74	-20.98
			(6.72)**	(7.37)**
Language*Earlytrack			19.02	7.51
			(8.22)**	(5.37)
Language*Earlytrack*Secondary			-10.64	-6.93
			(7.73)	(11.19)
Female	28.33	25.49	26.44	25.97
	(1.27)***	(1.16)***	(1.28)***	(1.33)***
Age of Student	6.71	5.03	3.83	6.89
	(3.24)*	(3.29)	(3.39)	(2.90)**
Native Student	26.14	22.09	20.75	17.39
	(3.95)***	(4.25)***	(4.78)***	(4.18)***
Living with Singleparent	-11.55	-9.50	-14.08	-8.87
	(2.44)***	(2.37)***	(2.30)***	(2.19)***
Living without Parents	-39.84	-39.27	-46.56	-36.34
	(6.98)***	(5.32)***	(5.69)***	(6.11)***
Secondary School Test Dummy	✓	✓	✓	✓
School Quality	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Number of Countries	14	14	14	14
Observations	116366	118690	124826	113018
R-squared	0.18	0.20	0.14	0.22

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

Table 3.5: Difference-in-Differences Mathematics (TIMSS 95 Grades 3/4 and Grades 7/8)

Dependent Variable: Mathematics Score	(1)	(2)	(3)
Number of Books	0.15		0.14
	(0.02)***		(0.02)***
NoB*Secondary	0.05		0.05
	(0.01)***		(0.01)***
NoB*Earlytrack	0.07		0.05
	(0.02)**		(0.02)**
NoB*Earlytrack*Secondary	-0.04		-0.01
	(0.02)*		(0.04)
Language		33.89	30.67
		(4.77)***	(4.23)***
Language*Secondary		-7.91	-12.00
		(7.56)	(6.86)
Language*Earlytrack		6.78	3.69
		(4.61)	(4.84)
Language*Earlytrack*Secondary		0.86	-1.54
		(8.50)	(5.04)
Female	-4.98	-4.81	-5.19
	(1.36)***	(1.53)**	(1.36)***
Age	14.5	13.51	14.82
	(3.56)***	(3.72)***	(3.39)***
Native	7.75	5.69	2.8
	(4.96)	(5.54)	(5.07)
Living with Singleparent	-12.31	-16.17	-12.47
	(2.01)***	(1.96)***	(1.97)***
Living without Parents	-27.61	-31.19	-25.82
	(4.66)***	(4.80)***	(4.47)***
Secondary School Test Dummy	✓	✓	✓
School Quality	✓	✓	✓
Country FE	✓	✓	✓
Number of Countries	11	11	11
Observations	102245	96873	95616
R-squared	0.20	0.16	0.21

*** significant at 1% ** significant at 5% * significant at 10%

(All standard errors clustered at the country level)

Table 3.6: Sensitivity Analysis 1: Early Tracking Thresholds (Reading)

Dependent Variable: Reading Score	Grade 4	Grade 5	Grade 6	Grade 8
Highest Parental	4.88	4.68	4.73	5.57
Education (years)	(0.61)***	(0.62)***	(0.62)***	(0.48)***
HPE*Secondary	-0.40	-0.39	-0.45	-1.34
	(0.83)	(0.82)	(0.80)	(0.94)
HPE*Earlytrack	3.33	3.44	2.34	-0.36
	(1.74)*	(1.29)**	(1.15)*	(0.76)
HPE*Earlytrack*Secondary	-2.07	-1.24	-0.81	0.99
	(1.15)*	(1.25)	(0.99)	(0.84)
Number of Books	0.10	0.10	0.10	0.07
	(0.01)***	(0.01)***	(0.01)***	(0.01)***
NoB*Secondary	0.06	0.06	0.05	0.11
	(0.02)***	(0.02)**	(0.02)**	(0.02)***
NoB*Earlytrack	-0.02	-0.01	-0.00	0.04
	(0.02)	(0.02)	(0.02)	(0.02)**
NoB*Earlytrack*Secondary	0.05	0.04	0.04	-0.07
	(0.03)	(0.03)	(0.03)	(0.02)**
Language	34.30	33.51	34.10	37.15
	(3.48)***	(3.76)***	(3.63)***	(6.98)***
Language*Secondary	-22.61	-20.98	-22.26	-26.54
	(6.99)***	(7.37)**	(7.63)**	(10.68)**
Language*Earlytrack	7.26	7.51	4.59	-2.36
	(6.71)	(5.37)	(5.92)	(7.23)
Language*Earlytrack*Secondary	2.38	-6.93	-2.90	4.23
	(7.98)	(11.19)	(9.04)	(12.27)
Secondary School Test Dummy	✓	✓	✓	✓
Student Characteristics	✓	✓	✓	✓
School Quality	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Number of Countries	14	14	14	14
Observations	113018	113018	113018	113018
R-squared	0.22	0.22	0.22	0.22

Table 3.7: Sensitivity Analysis 1: Early Tracking Thresholds (Mathematics)

Dependent Variable: Mathematics Score	Grade 4	Grade 5	Grade 6	Grade 8
Number of Books	0.15	0.14	0.14	0.17
	(0.02)***	(0.02)***	(0.02)***	(0.02)***
NoB*Secondary	0.04	0.05	0.04	0.05
	(0.01)**	(0.01)***	(0.01)***	(0.01)***
NoB*Earlytrack	0.03	0.05	0.03	-0.03
	(0.02)	(0.02)**	(0.03)	(0.03)
NoB*Earlytrack*Secondary	0.05	-0.01	0.01	-0.01
	(0.01)***	(0.04)	(0.03)	(0.02)
Language	31.20	30.67	29.46	34.12
	(3.90)***	(4.23)***	(4.45)***	(7.48)***
Language*Secondary	-12.85	-12	-12.33	-15.74
	(6.68)*	(6.86)	(7.07)	(10.3)
Language*Earlytrack	3.25	3.69	8.56	-5.91
	(5.99)	(4.84)	(5.92)	(7.80)
Language*Earlytrack*Secondary	20.15	-1.54	1.33	6.75
	(5.90)***	(5.04)	(6.15)	(7.97)
Secondary School Test Dummy	✓	✓	✓	✓
Student Characteristics	✓	✓	✓	✓
School Quality	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Number of Countries	11	11	11	11
Observations	95616	95616	95616	95616
R-squared	0.21	0.21	0.21	0.21

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

Table 3.8: Sensitivity Analysis 2: Tracking Measures (Reading)

Dependent Variable: Reading Score	Grade of Tracking	Age of Tracking	Number of Tracks
Highest Parental	4.68	9.71	5.25
Education (years)	(0.62)***	(2.77)***	(1.26)***
HPE*Secondary	-0.39	-2.36	-0.42
	(0.82)	(2.28)	(1.13)
HPE*Tracking Measure (TM)	3.44	-0.28	0.11
	(1.29)**	(0.17)	(0.72)
HPE*TM*Secondary	-1.24	0.11	-0.19
	(1.25)	(0.14)	(0.49)
Number of Books	0.10	0.06	0.10
	(0.01)***	(0.05)	(0.03)***
NoB*Secondary	0.06	0.18	0.02
	(0.02)**	(0.05)***	(0.04)
NoB*Tracking Measure (TM)	-0.01	0.00	0.00
	(0.02)	(0.00)	(0.01)
NoB*TM*Secondary	0.04	-0.01	0.02
	(0.03)	(0.00)*	(0.02)
Language	33.51	37.09	34.91
	(3.76)***	(14.38)**	(5.83)***
Language*Secondary	-20.98	-66.34	-3.12
	(7.37)**	(31.90)*	(10.53)
Language*TM	7.51	-0.17	-0.16
	(5.37)	(0.88)	(3.78)
Language*TM*Secondary	-6.93	2.80	-10.38
	(11.19)	(1.85)	(5.45)*
Secondary School Test Dummy	✓	✓	✓
Student Characteristics	✓	✓	✓
School Quality	✓	✓	✓
Country FE	✓	✓	✓
Number of Countries	14	14	14
Observations	113018	113018	113018
R-squared	0.22	0.22	0.22

Table 3.9: Sensitivity Analysis 2: Tracking Measures (Mathematics)

Dependent Variable: Mathematics Score	Grade of Tracking	Age of Tracking	Number of Tracks
Number of Books	0.14	0.28	0.11
	(0.02)***	(0.05)***	(0.05)**
NoB*Secondary	0.05	0.04	0.04
	(0.01)***	(0.08)	(0.04)
NoB*Tracking Measure (TM)	0.05	-0.01	0.03
	(0.02)**	(0.00)**	(0.03)
NoB*TM*Secondary	-0.01	0.00	0.01
	(0.04)	(0.00)	(0.03)
Language	30.67	39.81	20.9
	(4.23)***	(16.09)**	(9.44)*
Language*Secondary	-12.00	-7.51	-13.66
	(6.86)	(22.45)	(10.54)
Language*Tracking Measure (TM)	3.69	-0.51	8.56
	(4.84)	(0.88)	(5.92)
Language*TM*Secondary	-1.54	-0.26	1.33
	(5.04)	(1.11)	(6.15)
Secondary School Test Dummy	✓	✓	✓
Student Characteristics	✓	✓	✓
School Quality	✓	✓	✓
Country FE	✓	✓	✓
Number of Countries	11	11	11
Observations	95616	95616	95616
R-squared	0.21	0.21	0.21

*** significant at 1%   ** significant at 5%   * significant at 10%
(All standard errors clustered at the country level)

Table 3.10: Sensitivity Analysis 3: Different Data Reading: PIRLS 2001 and PISA 2000

Dependent Variable: Reading Score	(1)	(3)	(4)	(5)
Highest Parental	5.99			4.67
Education (years)	(0.40)***			(0.35)***
HPE*Secondary	2.52			0.96
	(0.76)***			-0.74
HPE*Earlytrack	4.06			1.74
	(0.64)***			(0.26)***
HPE*Earlytrack*Secondary	0.13			1.69
	(0.56)			(1.18)
Number of Books		0.13		0.09
		(0.01)***		(0.01)***
NoB*Secondary		0.09		0.08
		(0.01)***		(0.02)***
NoB*Earlytrack		0.04		0.01
		(0.02)**		(0.01)*
NoB*Earlytrack*Secondary		-0.03		0.00
		(0.02)		(0.02)
Language			36.35	29.93
			(3.05)***	(3.37)***
Language*Secondary			6.17	-2.23
			(5.29)	(5.15)
Language*Earlytrack			14.11	11.67
			(11.25)	(7.18)
Language*Earlytrack*Secondary			-7.72	-28.73
			(5.87)	(14.49)*
Secondary School Test Dummy	✓	✓	✓	✓
Student Characteristics	✓	✓	✓	✓
School Quality	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Number of Countries	12	12	12	12
Observations	73108	76323	82500	71242
R-squared	0.18	0.19	0.12	0.23

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

Table 3.11: Sensitivity Analysis 3: Different Data Maths: TIMSS 95 3/4 and TIMSS 99 7/8

Dependent Variable: Mathematics Score	(1)	(2)	(3)
Number of Books	0.16		0.15
	(0.02)***		(0.02)***
NoB*Secondary	0.05		0.06
	(0.01)***		(0.01)***
NoB*Earlytrack	0.06		0.05
	(0.03)		(0.02)**
NoB*Earlytrack*Secondary	0.01		0.03
	(0.02)		(0.03)
Language		35.22	30.90
		(4.28)***	(3.71)***
Language*Secondary		-14.90	-17.40
		(10.77)	(9.74)
Language*Earlytrack		4.55	5.24
		(5.58)	(6.80)
Language*Earlytrack*Secondary		20.44	6.22
		(6.41)**	(5.14)
Secondary School Test Dummy	✓	✓	✓
Student Characteristics	✓	✓	✓
School Quality	✓	✓	✓
Country Dummies	✓	✓	✓
Number of Countries	8	8	8
Observations	60228	56436	55756
R-squared	0.19	0.15	0.21

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

Table 3.12: Robustness of Hanushek and Woessmann Results

Dependent Variable: Country-Level SD of Reading Score	(1)[1]	(2)	(3)
Early Tracking Measure	Age of Tracking	Grade 5 or before	Grade 5 or before
Sample	Full Sample	Full Sample	OECD
Early Tracking	0.25	0.17	0.12
	(0.11)**	(0.14)	(0.16)
Inequality in Primary School	0.59	0.48	0.40
	(0.13)***	(0.16)***	(0.20)**
Number of Countries	18	18	15
R-squared	0.48	0.38	0.26

*** significant at 1% ** significant at 5% * significant at 10%
[1]Column (1) is the same as column (1) of Table 2 in Hanushek and Woessmann (2006)

3.9 Appendix for Chaper 3

Table 3.13: DiD Reading (PIRLS 2001 and PISA 2003) Without School Quality

Dependent Variable: Reading Score	(1)	(2)	(3)	(4)
Highest Parental	6.27			4.66
Education (years)	(0.69)***			(0.52)***
HPE*PISA	0.01			-0.50
	(1.08)			(0.80)
HPE*Earlytrack	4.99			3.67
	(1.31)***			(1.04)***
HPE*Earlytrack*PISA	-1.25			-1.27
	(0.67)*			(1.08)
Number of Books		0.14		0.10
		(0.02)***		(0.01)***
NoB*PISA		0.06		0.06
		(0.02)***		(0.02)***
NoB*Earlytrack		0.05		-0.01
		(0.02)**		(0.02)
NoB*Earlytrack*PISA		-0.03		0.04
		(0.03)		(0.03)
Language			35.58	31.07
			(3.83)***	(3.26)***
Language*PISA			-10.98	-14.58
			(7.49)	(7.89)*
Language*Earlytrack			20.35	9.59
			(8.49)**	(3.85)**
Language*Earlytrack*PISA			-12.06	-7.86
			(8.25)	(13.54)
Secondary School Test Dummy	✓	✓	✓	✓
Student Characteristics	✓	✓	✓	✓
School Quality				
Country FE	✓	✓	✓	✓
Number of Countries	15	15	15	15
Observations	146631	150348	158682	143957
R-squared	0.18	0.20	0.14	0.19

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

Table 3.14: DiD Mathematics (TIMSS 95) Without School Quality

Dependent Variable: Mathematics Score	(1)	(2)	(3)
Number of Books	0.15		0.13
	(0.01)***		(0.01)***
NoB*Secondary	0.05		0.06
	(0.01)***		(0.01)***
NoB*Earlytrack	0.05		0.06
	(0.02)**		(0.02)***
NoB*Earlytrack*Secondary	0.00		-0.02
	(0.03)		(0.03)
Language		35.74	31.96
		(2.69)***	(2.54)***
Language*Secondary		-7.75	-11.71
		(5.61)	(5.09)**
Language*Earlytrack		17.26	11.74
		(5.60)***	(4.58)**
Language*Earlytrack*Secondary		11.38	9.97
		(6.51)	(4.76)*
Student Characteristics	✓	✓	✓
School Quality			
Country FE	✓	✓	✓
Number of Countries	15	15	15
Observations	210224	189915	185947
R-squared	0.18	0.15	0.19

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

Table 3.15: DiD Mathematics (TIMSS 95) Omitting Family Structure

Dependent Variable: Mathematics Score	(1)	(2)	(3)
Number of Books	0.15		0.14
	(0.02)***		(0.02)***
NoB*Secondary	0.05		0.05
	(0.01)***		(0.01)***
NoB*Earlytracking	0.07		0.05
	(0.02)**		(0.02)**
NoB*Earlytracking*Secondary	-0.04		-0.02
	(0.02)*		(0.04)
Language		34.60	31.17
		(4.89)***	(4.33)***
Language*Secondary		-8.10	-12.30
		(7.61)	(6.90)
Language*Earlytrack		7.03	3.94
		(4.71)	(4.90)
Language*Earlytrack*Secondary		0.34	-1.55
		(8.54)	(5.06)
Student Characteristics	✓	✓	✓
School Quality	✓	✓	✓
Country FE	✓	✓	✓
Number of Countries	11	11	11
Observations	102522	97310	95864
R-squared	0.20	0.16	0.20

*** significant at 1% ** significant at 5% * significant at 10%
(All standard errors clustered at the country level)

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