Department of Economics

Empirical Essays on Occupations, Reallocation and Wage Differentials

Ayşen Isaoğlu

Thesis submitted for assessment with a view to obtaining the degree of
Doctor of Economics of the European University Institute

Florence, November 2009

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To my family: Sanne, Gülten, Fikret and Fatih

 DOI: 10.2870/13357

Acknowledgements

I am grateful to my supervisors Frank Vella and Rick van der Ploeg for their advice, time and support. I particularly thank Frank for reading each chapter several times and providing detailed and constructive comments to improve them. Rick not only academically contributed to this dissertation but was also always available for a cup of coffee.

During the dissertation period, I have had an invaluable experience at the European University Institute thanks to its capacity to bringing together the best professors in their fields and providing an excellent working environment. Most importantly, I have learned a lot about economics, met very good friends and had the chance to live in an international and intellectually challenging environment. I gratefully acknowledge the four year doctoral research grant by the Italian Ministry of Foreign Affairs through the Mediterranean Programme which made it possible.

In addition, I would like to thank participants of the 9th IZA European Summer School in Labor Economics and the EUI labor lunch meeting for their comments. I also want to thank Thomas Bourke, Marcia Gastaldo, Martin Legner, Jessica Spataro, Julia Valerio and Lucia Vigna for their competent work and logistic support.

All my flatmates at Via Caselli 7 provided a very cosy and friendly home during all these years. Two other persons deserve special mentioning. Pınar Artıran, with whom I started my EUI experience, became a very close friend over countless gelati and prosecci. Sanne Zwart provided useful comments to my work but above all was always there for me to share the good and the bad moments. He is the most precious thing I got from my time at the EUI.

Ayşen İsaoğlu
Florence, November 2009

Contents

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
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Introduction

Economic debates increasingly focus on labor issues as they are not only crucial to understand and assess the functioning of national economies but also provide benchmarks for cross country analysis. Among the topics of interest are the reasons behind the levels and variations in unemployment rates, the implications of differences in labor market institutions, the extent of wage inequalities and the drivers of individuals' choices of work. The increasing interest in labor issues is accompanied and in fact enforced by developments in econometric methods to analyze complex problems and by advances in computer technology providing more computational power everyday. As a result, research on labor economics has been growing remarkably during the last couple of decades. Richard B. Freeman, Professor at Harvard University and Director of the National Bureau of Economic Research (NBER)'s Program on Labor Studies, points out that as opposed to ten published working papers by the Program in the year 1979, nowadays around 20 papers are published in a single month. The Program on Labor Studies has become the largest producer of working papers among all NBER programs.

In this thesis I aim to contribute to the labor economics literature by casting more light on i) measurement errors regarding data on occupational affiliations, ii) worker mobility across occupations and iii) wage differentials between part-time and full-time workers with comparable skills. Throughout, I focus on German labor markets. Germany is one of the major economies in the world and the most important one in Europe. I employ individual level panel data from the German Socio-Economic Panel (GSOEP). The GSOEP has started in the Federal Republic of Germany in 1984 with around 12,000 respondents representative of the entire residential population. Since then, several samples are added occasionally to reflect the changing population structure of the Germany, like the expansion of the GSOEP to the former German Democratic Republic in June 1990. GSOEP has several advantages as it is based on a rather stable set of questions regarding the demographics,

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

education, earnings and labor market dynamics. Due to its panel data structure, individuals can be followed over time. Together with recently developed econometric methods for panel data, this allows for analyzing the importance of dynamics in individual's decisions.

In the first chapter, I focus on identifying the measurement errors in occupational affiliations in the GSOEP. The occupational classifications are considered at the most detailed level, i.e. varying between hundreds to thousands of different occupations depending on the classification system. It is well known that individual level data is prone to measurement errors, especially if very detailed information is considered. For the occupational affiliations provided by the GSOEP, this can be clearly seen from the average annual occupational mobility over the last two decades. An alternating pattern with troughs of 5-7 percent and peaks of 25-55 percent is observed, with the exact values depending on the classification system. Since actual mobility is very unlikely to experience such a behavior for a period of two decades, this pattern thus provides very pronounced and unambiguous evidence for measurement errors in the data.

Initially one may question the stability of the used occupational classification systems over the period under consideration. However, as a retrospective recoding of the occupational affiliations took place in 2002 to update the existing occupational classifications, this is not the issue. A further analysis of the survey structure reveals the likely cause. In the peak years all workers were asked to declare the details of the tasks they were performing. In the trough years, only workers who declared a job or labor market status change were asked for this information, while for other workers their previous occupation was kept. Clearly, the structure of the survey leads to the observed alternating pattern of average occupational mobility.

A correction method taking advantage of the panel data structure is used to address the measurement errors. Since the working life of individuals is followed over time, more reliable and sound individual occupational mobility patterns can be constructed. A detailed analysis of worker mobility across occupations based on different definitions of occupational mobility and for different sub-samples is then carried out. Corrected average occupational mobility averages around 5-7 percent for the last two decades depending on the sample.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

In the second chapter, I investigate the determinants of annual worker mobility across occupations in western Germany. The analysis employs the corrected occupational affiliation data at the most disaggregated level of the International Standard Classification of Occupations (ISCO-88). Analyzing occupational changes at such a detailed level is important as it best reflects career changes. Moreover, occupational change entails a change of tasks for the worker which is not always true for workers changing employers or sectors.

To estimate the probability of an occupational change, a dynamic fixed effects maximum likelihood estimation is carried out. This method is chosen as it allows to control for unobserved time-invariant worker heterogeneity. Moreover it also allows for incorporating the dynamic structure and assessing temporal persistence. However, as the true individual fixed effects are replaced by their sample estimates incidental parameter bias arises. To address this, an analytical bias correction approach designed for dynamic nonlinear models is employed.

The estimation results provide several new insights. Having changed occupation in the previous year decreases a worker's probability to change in the current year by 8 to 9 percent. The effect varies from 2 to 14 percent depending on the worker's characteristics. The probability of changing occupation decreases with age and this effect is declining in the level of education. Another factor that decreases the propensity to change occupation is high regional unemployment rates. This effect is more prominent for foreigner females.

Finally, in the last chapter I analyze the determinants of the part-time/full-time employment decision and the potential part-time hourly wage differential for women in western Germany. There are various reasons suggested by economic theory to expect a difference in hourly wages between part-time and full-time workers with similar characteristics. A straightforward comparison of the average wages for part-time and full-time workers in the analyzed sample indeed suggests a 4 percent part-time wage penalty.

Findings of several studies show that the used empirical method has important implications on the results. It is hence crucial to accurately deal with the estimation related problems inherent to the topic. Problems emerge from the fact that the decision to work part-time or full-time and the wage one receives are affected by

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

the same observable and unobservable factors. When unobservable factors, either time-variant or time-invariant, are ignored estimates are biased due to endogeneity.

In this study, a two-step estimation method is used to control for both time-variant and time-invariant heterogeneity. In the first step the part-time/full-time employment decision is estimated via a fixed effects procedure. Based on the results of this estimation, a control function is constructed and added as an additional co-variate to the fixed effects OLS estimation of the wage equation. By employing fixed effects procedures in both steps, time-invariant unobserved worker heterogeneity is taken into account while the time-varying unobserved worker heterogeneity is addressed by including the control function in the wage equation. A recently developed estimation method is used to account for the incidental parameter bias arising in both steps.

The estimation results suggest that there is no hourly wage difference between part-time and full-time working comparable women in western Germany. The found unconditional part-time pay penalty disappears once observable and unobservable worker characteristics are controlled for.

CHAPTER 1

Occupational Affiliation Data and Measurement Errors in the German Socio-Economic Panel

This chapter shows that there are severe measurement errors regarding the occupational affiliations in the German Socio-Economic Panel. These errors are traced back to the survey structure: in years where occupational information is gathered from the entire employed population instead of only from those declaring job or labor market status changes, average occupational mobility is around five times higher. In order to construct reliable occupational affiliation data, a correction method based on related job or labor market status changes is proposed. The corrected occupational mobility patterns are then analyzed for different samples.

JEL classification. C41, C81, J62.

Keywords. Measurement Errors, Occupational Mobility, Panel Data.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

1.1. Introduction

Occupational affiliation data is important for two growing aspects of labor economic research. The first is the determination of wage growth. The second is the analysis of worker turnover. However, reliability of data on occupational affiliation is known to be an issue (e.g. Mellow and Sider (1983), Murphy and Topel (1987), Mathiowetz (1992), Polivka and Rothgeb (1993), Neal (1999), Kambourov and Manovskii (2004*a*), Moscarini and Thomsson (2008)). This chapter shows that measurement errors concerning occupational affiliations are severe in the German Socio-Economic Panel (GSOEP). Throughout, the focus will be on occupational mobility at the individual level as it allows for displaying data inconsistencies in the clearest way. After discussing the sources of the measurement errors, a correction method based on job or labor market status changes is presented. Finally, the corrected average occupational mobility measures for different samples and occupational classifications are discussed.

Reliable data on occupational changes is crucial to analyze the contributing factors of wage growth. While many studies (e.g. Neal (1995), Parent (2000) and Dustmann and Meghir (2005)) argue that human capital is specific to the industry of employment i.e. that industry tenure has significant explanatory power on wage growth, Kambourov and Manovskii (2009), using Panel Study of Income Dynamics (PSID), provide evidence of considerable returns to occupational tenure. In fact when an individual's occupational experience is taken into account, his/her tenure in an industry or with an employer is found to have little importance in explaining his/her wage. Hence, they conclude that human capital is occupation rather than industry or employer specific.

Occupational changes are also of interest in studies of worker turnover. After analyzing worker reallocation across employment states (e.g. Abowd and Zellner (1985), Blanchard and Diamond (1990)), across employers (e.g. Farber (1994), Fallick and Fleischman (2001)), across industries (e.g. Jovanovic and Moffitt (1990), Bils and McLaughlin (1992)), recent studies have focused on worker reallocation across occupations (see Moscarini and Vella (2003), Kambourov and Manovskii (2004*b*), Burda and Bachmann (2008), Moscarini and Vella (2008)). These studies argue that occupations at a detailed level provide the best information to the labor economist

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

about the career changes. To see the importance of changes across detailed occupations, consider, for instance, the broad title *Professionals [2]*, where the number in square brackets denotes the respective code of the International Standard Classification of Occupations (ISCO-88). This entry includes both *Meteorologists [2112]* and *Chemists [2113]*. Clearly, one would not be able to identify the important career change of becoming a chemist after having worked as a meteorologist if the classification is not considered at a disaggregated level. Even at the three-digit level both of these occupations are named under *Physicists, Chemists and Related Professionals [211]*.

Data on occupational affiliation is known to be subject to measurement errors. This is not surprising as occupational classifications may contain hundreds to thousands of units. Cross-sectional errors in coding that are overlooked may become apparent only when longitudinal dimension is considered. Therefore, one of the most obvious ways to investigate the reliability of occupational affiliations is to analyze occupational mobility patterns.

Plots of worker turnover across occupations using the data provided by GSOEP exhibit a suspicious pattern over the last two decades. The fraction of workers changing occupation at annual frequency alternates recurrently between around 7 and 45 percent. These percentages are for the four-digit ISCO-88, which is constituted of 390 distinct occupational units. Even at the one-digit level, which only has 9 different occupational groups, the percentages are around 5 and 25 respectively. In this study it is shown that this pattern is mainly driven by the survey structure: years with high average occupational mobility coincide with the years in which the occupational information is gathered from all workers. In the years with low values, the occupational information is gathered only from respondents who declare that they have experienced a job or labor market status change.

To obtain more accurate occupational affiliation data, a correction method based on other reported job or labor market status changes is used. The rationale is that an occupational change is likely to be accompanied by a change of employer, position in the company, industry etc. Similar filters are also used by e.g. Moscarini and Thomsson (2008). This method clearly corrects the unacceptably high average occupational mobility found in years where every worker was interviewed about their occupation. The alternating pattern in the average occupational mobility disappears

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

after correction which validates the claim that a substantial part of the measurement error stems from the structure of the survey.

Results are presented for two measures of average occupational mobility that are commonly used in the literature. The first measure considers a worker as a "mover" if he/she declares a different valid occupational code in two consecutive periods in which he/she is employed (see Moscarini and Thomsson (2008), Burda and Bachmann (2008), Moscarini and Vella (2008)). The second measure also considers switches after non-employment spells, i.e. if an individual is employed in the current period, but was not employed in the previous period, a switch in his/her occupation will be recorded if he/she reports a current occupation different from the one he/she reported when he/she was most recently employed (see Kambourov and Manovskii (2004*b*)).

Average occupational mobility at annual frequency is ranging from around 4.5 percent to 7 percent over the last two decades, depending on the sample and the classification choice. There is no trend, but strong procyclicality is found to be robust across different samples. Only when changes after non-employment spells are also considered females are more mobile on average than males. This is expected since females have more intermittent careers and after non-employment periods workers in general are more likely to change occupations. Interestingly, workers with at least a college degree are found to be more mobile on average in comparison with other educational groups. Not surprisingly, workers younger than 40 have a higher occupational mobility on average which is also driving the overall procyclicality. The inclusion of workers from the former German Democratic Republic raises the mobility levels significantly, especially when changes after non-employment spells are also considered. Adding government sector or self-employed workers to the sample does not have significant impact on the observed occupational mobility patterns. The average occupational mobility levels are increased slightly when part-time workers are included. This is found to be mainly driven by females joining the employment pool after non-employment.

There are other studies presenting findings on occupational mobility in Germany that, like this study, use individual level data and disaggregated occupational classifications. Very recently, Burda and Bachmann (2008) analyze the extent and the dynamics of structural change in western Germany using the Institute for Employment Research (Institut für Arbeitsmarkt und Berufsforschung (IAB)) dataset. They compute worker flows across occupations and sectors over the period 1975-2001. Occupational mobility levels and cycles presented in their study are very similar to this study's findings. Kambourov and Manovskii (2004*a*) also use the IAB dataset but for the period 1975-1995. For a sample with the same characteristics and for the common time span 1985-1995, albeit with a different occupational classification, it is found that the patterns of occupational mobility are very similar to the ones in this study. However, there is a difference in levels (11 percent versus 7 percent). In an other study, Zimmermann (1999) analyzes the period 1984-1991 using the GSOEP. An exhaustive set of tables on average job and occupational mobility is provided, however measurement errors are not discussed. A direct comparison of the results presented in Zimmermann (1999) and the current chapter is unfortunately not possible since the codes used in Zimmermann (1999) are no longer available in the GSOEP. However, the average occupational mobility is about twice as high of this study's findings when corrected affiliation data is used for the same sample.

As mentioned before, measurement errors in occupational affiliation data are also an issue for other datasets. For instance, Kambourov and Manovskii (2009) document in detail the measurement errors regarding the occupational and industry affiliations in the PSID for the period 1968-1993. They compare the original occupation and industry affiliation data, i.e. coded at the time of the survey, to retrospectively coded data. The latter data files, namely, the Retrospective Occupation-Industry Supplemental Data Files became available in 1999 and include a retrospective assignment of three-digit 1970 Census codes to the reported occupations and industries for the period 1968-1980. There is stark disagreement between the originally and retrospectively assigned codes for the same individuals. For the period 1976-1980 two-digit occupational mobility levels in the retrospective files are found to be twice as small than the ones obtained in the original files.

Similarly, for the annual March files of the Current Population Survey (CPS) dataset Murphy and Topel (1987) and Kambourov and Manovskii (2004*a*) show strong evidence for classification errors in occupations. In the CPS, each household is interviewed once a month for four consecutive months, then removed from the

 DOI: 10.2870/13357

sample for eight months and again interviewed for another four months. Thus, any household is present in the survey for eight (non-consecutive) months in total. Occupational and industry information is gathered monthly, regarding workers' labor force activity in the week prior to the survey. Additionally, in March of each year workers present in the CPS sample are given a supplemental questionnaire in which they are asked to describe their longest job last year. Kambourov and Manovskii (2004*a*) provide convincing evidence that annual data from the March files does not measure annual mobility correctly. Due to the rotation of the panel, this data merely measures mobility over a couple of months' period. Recently, Moscarini and Thomsson (2008) employ the CPS data at the monthly level in order to avoid this problem. They exploit the monthly longitudinal structure to derive more accurate occupational mobility data.

Although the focus of this study is the occupational affiliations, it should be pointed out that the industry affiliations in the GSOEP (two-digit Nomenclature des Statistiques des Activités Economiques de la Communauté Européenne (NACE) and two other codes in the Cross-National Equivalent Files) are also measured with error as the information on the industry the worker is in was gathered through the same procedure as for occupational information. Moreover, any measure derived from occupational or industry affiliations is also contaminated with measurement errors such as International Socioeconomic Index of Occupational Status (ISEI), Magnitude Prestige Scale (MPS), Treiman Standard International Occupational Prestige (SIOPS) and Erikson Goldthorpe Class Category (EGP).

The next section describes the characteristics of the GSOEP and the Section 1.3 provides information on the occupational classifications. Section 1.4 discusses the measurement errors and Section 1.5 explains the proposed correction method. Section 1.6 presents and discusses the corrected occupational mobility measures. Section 1.7 concludes. The Appendix provides a detailed description of the data correction and the related properties of the sample.

1.2. German Socio-Economic Panel

The GSOEP is a nationally representative longitudinal survey of persons and private households which started in the Federal Republic of Germany (FRG) in 1984 with around 12,000 respondents (SOEPGroup (2001)). The target population

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

represented in the GSOEP was the entire residential population of the FRG. Initially there were two samples, namely *Residents in the FRG* and *Foreigners in the FRG*. The first sample covers persons in private households with household heads who do not belong to the main foreigners groups of guestworkers, whereas the second considers the private households where the household head is from Greece, Italy, Spain, Turkey and former Yugoslavia. The GSOEP expanded to the former German Democratic Republic (GDR) in June 1990 and since then the residential population in the former GDR is also represented (Haisken-DeNew and Frick (2003)).

The GSOEP has various advantages and disadvantages for studying labor market transitions. The primary advantage is, next to transitions across the labor market status i.e. employment, unemployment or being out of labor force; transitions across firms, within firms, industries and occupations are also collected. Moreover information on the exact timing of these transitions is gathered either via explicitly asking for the month and year of the change or via questions based on a calendar.

A second advantage of the GSOEP is the consistency of the survey questions. The central aim of this panel study is to collect representative micro-data on persons and households in order to measure stability and change in living conditions. Hence, changes in the questionnaires are minimized.

An additional advantage of the GSOEP is that generated variables are also provided next to the direct responses from the surveys for some variables. These generated variables are more reliable since they are constructed using several cross-checks. As suggested by Haisken-DeNew and Frick (2003), generated variables are used instead of the direct survey responses in this study when both are available.

There are also disadvantages to using the GSOEP. Compared to other datasets, such as the IAB, representing a two percent sample from the German social security records, there are relatively few observations. Moreover, as the GSOEP is a survey, information is collected on a voluntary basis which makes it prone to suffer from attrition. The representativeness of the GSOEP sample is addressed in several ways. All household members are interviewed individually once they reach the age of 16. Hence, the next generation is automatically included. In case of residential mobility, the person is followed within the country. Although this might lead to over- or under-represented geographical areas, it does not affect other properties of the

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

sample such as gender, age and family distribution.[1] Third persons moving into an existing GSOEP household are surveyed even in case of subsequently leaving that household. Persons and households which could not be successfully interviewed in a given year are followed until there are two consecutive temporary drop-outs of all household members or a final refusal. In the case of a successful interview after a drop-out, there is also a small questionnaire including questions on central information which is missing for the drop-out year. Addresses are kept up to date by the field work agency throughout the entire year in order to be informed about residential mobility.

The analysis in this study is based on 21 waves that cover the period 1984-2004. The base sample consists of full-time employed males and females, aged between 18-65, members of the *Residents in the FRG* and *Foreigners in the FRG* samples, not receiving education or training, not dually employed, not self-employed or belonging to a household with a self-employed member, not working in the government sector. Observations for individuals who reported to be living in the former GDR in 1989 and who moved to the former GDR after the unification are also excluded.[2] Additional sample specifications will be discussed and analyzed in Section 1.6.

1.3. Occupational Classifications in the GSOEP

The GSOEP provides several occupational classifications. This study focuses on the "Klassifizierung der Berufe (KldB)", which is the national coding system of the German Federal Statistical Office, and the International Standard Classification of Occupations (ISCO-88). The KldB is provided at the four-digit level. The 2,287 occupational unit groups can be aggregated to units of 369, 88, 33 and 6. The ISCO-88 is a nested classification of occupations at the four-digit level. The one-digit distinguishes 9 major groups, which have 28 major subgroups, 116 minor groups and 390 unit groups. Classification at the four-digit level thus corresponds to 390 different occupations (ILO (1990)). The four-digit KldB and ISCO-88 classifications provide highly detailed occupational information. A third classification in the GSOEP is in the Cross-National Equivalent File (Burkhauser, Butrica, Daly and Lillard (2000)),

[1]Note that the panel structure together with the follow-up of individuals instead of addresses (where the latter is the case in the CPS used by Moscarini and Thomsson (2008)) allow taking into account occupational changes that are accompanied by geographical changes as well.

[2]See the Appendix for a detailed description of the employed sample.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

referred to as CNEF code in this study. Although less detailed (101 occupational units), this file consists of equivalently defined variables to allow for comparison of the PSID, the GSOEP, the British Household Panel Study (BHPS) and the Canadian Survey of Labour and Income Dynamics (SLID). As this code is derived from the other occupational classifications, it is not discussed separately here.

The KldB and the ISCO-88 are present in the GSOEP for all periods under the investigation. However, occupational information is not asked each year to the whole survey population. Instead, in 1985, 1986, 1987, 1988, 1990, 1992, 1994, 1996, 1999, 2001 and 2003 only respondents who declared a job or labor market status change was surveyed. In the rest of the years, the whole population was surveyed via a direct question:

> What is your current position/occupation? *Please give the exact title. For example, do not write "clerk", but "shipping clerk"; not "blue-collar worker", but "machine metalworker". If you are engaged in public employment, please give your official title, for example, "police chief" or "lecturer". If you are an apprentice or in vocational training, please state the profession associated with your training.*

In the years that the question is asked only to people who experienced a job or labor market status change, the previously declared occupation is coded in the absence of a job or labor market status change.

A recoding of the occupational affiliations based on the original survey responses took place in 2002 (see Hartmann and Schuetz (2002)). The main reason for the update was to replace the outdated ISCO-68 with the ISCO-88. Based on the original survey answers, occupational affiliations were recoded retrospectively according to various criteria. First, recoding was done using the national coding system KldB. These codes were then translated into ISCO-88 by an algorithm. If the respondent provided information referring to distinct occupations in his/her answers, the first mentioned occupation was taken unless information regarding the second occupation was more precise. When the respondent did not provide sufficiently specific information to distill an occupational affiliation, also information such as industry branch, training and the job position was taken into account to decide on what his/her occupation is. If this was still not informative enough to determine the occupational category of the respondent, then the following two rules applied according to the

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

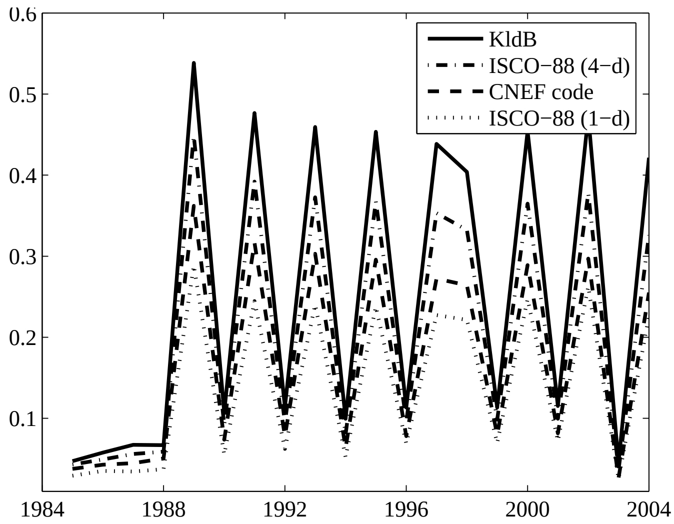

FIGURE 1. Occupational mobility based on the original data considering the national code (KldB), four-digit ISCO-88 (ISCO-88 (4-d)), CNEF code and one-digit ISCO-88 (ISCO-88 (1-d)).

source of ambiguity. If the information on the content of the occupation was not sufficiently specific to fit a single category, the category more frequently observed in the data was chosen. If the information was only sufficiently specific to determine the category of the occupation, the occupation in this category with the lowest qualification level was chosen. For 96.4 percent of the respondents the information was sufficiently specific to unambiguously generate occupational codes (87.2 percent without any additional information and 9.2 percent with additional information). Only for the remaining 3.6 percent of the cases, the last two rules had to be taken into consideration.

1.4. Measurement Errors

There are severe and unambiguous measurement errors in the occupational affiliations in the GSOEP. Figure 1 depicts the average occupational mobility over the last two decades for the base sample. Although in the figure only occupational transitions from employment-to-employment are considered, the picture is similar when changes after non-employment spells are also taken into account. Since the occupational changes are of interest, the first wave is lost.

Figure 1 is self-alerting as an evidence of measurement errors in the data. For all classifications available in the GSOEP, average occupational mobility changes from

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

5-7 percent one year to 25-55 percent for the next year and then back again to 5-7 percent and this in a repeated manner.

The measurement errors in occupational mobility arise regardless of the disaggregation level. As mentioned above, the KldB considers 2,287 different units where as the one-digit ISCO-88 considers only 9 units.[3] Although measuring occupational mobility with the one-digit ISCO-88 lowers the peaks from 35-45 percent to 20-25 percent, the dented pattern remains.

The reason for the observed average occupational mobility patterns is clear: most of the errors are generated by the structure of the survey. One can clearly see that the years 1989, 1991, 1993, 1995, 1997, 1998, 2000, 2002 and 2004 with a high occupational mobility are also the years in which all respondents, independent of whether they have experienced a job or labor market status change, are asked to declare their occupation in detail. Apparently, asking this question without any dependence on other changes is vastly generating spurious changes.

One could argue that the peaks reflect accumulated occupational changes over subsequent years. When individuals do change occupation but fail to report a job or labor market status change, the occupational change is counted in the following year in which all individuals are surveyed. This would imply that when occupational information is asked from all respondents in two subsequent years, average occupational mobility in the second year should be about half of the value found in the first year. In 1997 and 1998 all respondents are asked about their occupations regardless of any job or labor market status change, nevertheless, magnitudes of the average occupational mobility are found to be similar. This suggests that it is mostly the survey design instead of the accumulated occupational changes that drives the pattern.

There are other well-known sources of measurement errors in occupational affiliations. It could be the case that respondents are explaining their tasks in an unclear way or that the coder generates the error while coding. Mathiowetz (1992) presents an experiment in which coders are asked to assign occupations based on company records and respondent records independently for the same sample. The disagreement rate is found to be 48 percent at the three-digit level. Clearly, in

[3]When armed forces are also included, it becomes 10 units as armed forces are classified separately in the GSOEP.

DOI: 10.2870/13357

the GSOEP the scope of this kind of errors is larger in years when occupational information is collected from all respondents. Errors created at the coding stage can be minimized by retrospective checks on the same individual (see Kambourov and Manovskii (2004*a*)). However, the 2002 recoding of occupational codes in the GSOEP mentioned above did not take advantage of this. The 97 percent precision of the recoding thus only relates to cross-sectional inferences. At the longitudinal dimension, errors are not specifically addressed. This explains the fact that there are many instances in the data where respondents are coded in two different (quantitatively) but very similar (qualitatively) occupations. For instance, consider the ISCO-88 codes. Someone who had declared an occupational change in 1996 and hence had been asked for the new occupational information had been coded as *Secretary [4115]*. The following year, when the entire population was asked the question related to their occupations, although she did not declare any kind of job or labor market status change over the last year, she was coded as *Stenographer or Typist [4111]* and the year after, where again the whole population was surveyed, without experiencing any job or labor market status change, she was coded *Philologist, Translator, Interpreter [2444]*. These result is probably driven by the coding error. This special case is also a good example to explain the differences in levels among the different classifications in Figure 1. In the example, the first "highly likely spurious" change from *[4115]* to *[4111]* would not be observed if one was considering the occupational changes at the three-digit level, the latter change is observed at all levels. Clearly, the more detailed the occupational code, the more prominent the measurement errors are, although some of these coding errors occur at all levels.

Evidence from the PSID recoding suggests that the measurement errors would indeed have been less severe if the recoding of 2002 would have used retrospective checks on the same individual. The PSID used one-digit occupational codes in 1968-1975, two-digit occupational codes in 1976-1980 and three-digit codes after 1981. In 1986, the PSID analysts started working on the 1968-1980 files in order to maintain three-digit occupational codes for the whole period of the survey, including the years before 1981. To create three-digit codes, original material, which was also used to create the one- and two-digit codes in the past, was used. In 1999 the Retrospective Occupation-Industry Supplemental Data Files was released. Kambourov and Manovskii (2004*b*) find a considerable disagreement between the originally coded and the retrospectively corrected files. Occupational mobility in the latter for the period 1981-1994 is more than twice as small than the mobility obtained from the originally coded occupations. In the retrospectively corrected PSID files, all occupational information for each respondent across all required years was coded by the same analyst before moving to another respondent. In this way, the analyst also used the past and future information on the occupation of the respondent which obviously leads to a more consistent occupational history. In the next section, a similar approach is followed to correct occupational affiliation data in the GSOEP.

1.5. Identifying the Genuine Occupational Changes

To reduce the measurement errors in occupational affiliations in the GSOEP, a correction method that uses job or labor market status changes is followed. This is the most straightforward way of correcting the data since for almost half of the waves occupational information is asked only when respondents declare a job or labor market status change. Using such changes as a condition when correcting the data imposes the structure that is lost in years when the occupational coding is asked to all respondents. Moreover, it is unlikely to observe a genuine occupational switch without any other labor market situation change for a worker. Kambourov and Manovskii (2004*b*) show that in the PSID, 80 percent of the one- and two-digit occupational switches in the Retrospective Files are accompanied by either an employer or a position switch. The idea of the correction method is therefore to consider occupational changes genuine if they are accompanied by other job or labor market status changes.

Another justification of pursuing this approach follows from Polivka and Rothgeb (1993). Asking whether job or labor market status changes occurred starting from the beginning of the previous year is similar to using dependent coding. In the former, respondents are asked whether there were any changes, while in the latter respondents are confronted with their occupation in the previous period and are asked whether this is still their occupation. Polivka and Rothgeb (1993) analyze a proposed change in the survey structure of the CPS. When all respondents were asked to report their occupation, the average occupational mobility was 39 percent, whereas when dependent coding was used, it dropped to about 7 percent. In addition, an external consulting firm gave 5.9 and 7.4 percent as bounds for average

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

occupational mobility. The conclusion was that using dependent coding leads to more accurate estimates. The similarities of dependent coding and asking about job or labor market status changes suggest the use of the latter to identify genuine occupational changes.

In each wave of the GSOEP, the respondents are asked to state the changes in the "job situation" since the beginning of the previous year. If there is any change, they are asked to give information on the type and timing of the change such as whether the respondent has entered employment for the first time, started paid employment again after not being employed for a while, started a new position with a different employer, became self-employed or changed positions within the same company. Any occupational change accompanied by one of the job or labor market status changes above is considered as genuine. Without such a change the previous occupational code is kept. How this method is implemented in practice is discussed in the remainder of this section.[4]

To motivate the correction method, questions regarding job situation changes as well as questions regarding occupation and industry information are presented in Table 1. They are taken from the 2001 and 2002 surveys. Note that the latter year is a "peak year" and the former not.

From question 23, one can see that if the respondent declares in 2001 that he/she had not experienced a job situation change, then he/she is not asked for occupation or industry information. However, in 2002, regardless of the job situation change his/her occupational and industry information is asked.

The method to identify genuine occupational changes in the related calendar years consists of three steps. Before going into details, these steps can be summarized as follows. First, it is checked whether the respondents have changed their job or started a new job after the beginning of the previous calendar year (question 23 in Table 1). In case of no reported change, the previously coded occupation is kept. Second, if a change in job or labor market status took place, then the type and the exact timing of the change is retrieved (questions 24 and 25). Third, the occupational information regarding to that change in the data is kept unchanged but deployed to the relevant calendar year if necessary (question 30).

[4]Detailed information on the changes made to the data and imputation methodology is presented in the Appendix.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

23. Did you change your job or start a new one after December 31, 1999? (2000 *in the 2002 questionnaire*)
- yes ☐
- no ☐, skip to question 37 (skip to question 30 *in the 2002 questionnaire*)

24. When did you start your current position?
2000, in the month ☐☐ (2001, in the month ☐☐ *in the 2002 questionnaire*)
2001, in the month ☐☐ (2002, in the month ☐☐ *in the 2002 questionnaire*)

25. What type of an employment change was that?
In the case that you have changed positions several times, please pick the appropriate reason for the most recent change.
- I have entered employment for the first time in my life ☐
- I have started up with paid employment again after not having been employed for a while ☐
- I have started a new position with a different employer ☐
- I have become self-employed ☐
- I have changed positions within the same company ☐

30. What is your current position/occupation?
Please give the exact title. For example, do not write "clerk", but "shipping clerk"; not "blue-collar worker", but "machine metalworker". If you are engaged in public employment, please give your official title, for example, "police chief" or "lecturer". If you are an apprentice or in vocational training, please state the profession associated with your training.

35. In which branch of business or industry is your company or institution active for the most part?
Please state the branch as exactly as possible, for example, not "industry", but "electronics industry"; not "trade", but "retail trade"; not "public service", but "hospital".

37. Since when have you been working for your current employer?
If you are self-employed, please indicate when you started your current work.
Since, month ☐☐ year ☐☐☐☐

TABLE 1. Questions used in identifying genuine occupational codes in the 2001 and 2002 surveys. Differences in the 2002 questionnaire are mentioned between parentheses. Note the different implications of a "no" to question 23.

In the first step, the occurrence of job changes are analyzed. To increase reliability, the generated variable is used. This variable shows whether the respondent is not employed, employed without a job change or employed with a job change. When the respondent is not employed the occupation is left missing, when the respondent is employed without a job change, the last reported occupation is kept. In case of a change in job status it is necessary to further analyze the occupational information.

The next step deals with identifying the calendar year of the change. This is important for two reasons. First, in contrast to other micro datasets where all interviews are held during a particular week or month, the GSOEP survey is conducted all over the year. To have a consistent overall picture, it is important that for all respondents the same 12 month period should be used and the calendar year is the obvious candidate. Second, deploying changes to exact calendar years makes it possible to relate worker reallocation with macroeconomic variables from other sources. Almost 90 percent of the survey is held in the first four months of the calendar year. Therefore, a large fraction of job situation changes reported in a given year correspond to the previous year. After this recoding of the job situation changes according to the exact year of the change, as expected some individuals have multiple job situation and thus occupational changes in a given calendar year.

As a result of allocating changes to their calendar years, there are cases in which the respondent is not employed at the time of the survey in a given year and the year after declares a change considering the "previous year". This raises the question whether to consider someone in the employment pool in a given calendar year when part of the year he/she is not employed. This choice obviously affects the occupational mobility. To reduce the scope for both under- and overestimation of occupational mobility, someone is considered "employed" if he/she works minimum 6 months in a given year. Respectively, relevant occupational codes and other variables, for instance, somebody becoming a government sector worker or self-employed with that job situation change, are also imputed. The results are not substantially altered when instead of 6 months, the minimum employed period is considered to be 3, 9 or 12 months.

Finally, after correcting the job situation change variables, new occupational codes are imputed. It is implicitly assumed that the occupational change took place when the job situation change took place. Double job situation changes is

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

translated to only 41 occupational changes. When there is a double occupational change observed for the same calendar year, they are both counted for the aggregate occupational mobility measures.

A slight change in the survey questions in 1994 may have affected the occupational mobility measures. Before 1994, respondents were asked to declare *all* the job situation changes they have experienced from the beginning of the previous year until the current date of the survey. However since 1994 they are asked to declare *only* the last change. The data suggests that the change in the survey can be ignored while identifying the job situation changes. Out of 72,482 observations before 1994, there are only 119 observations for which multiple job situation changes are declared. Hence, ignoring multiple job changes when considering occupational mobility at annual frequency seems not to be problematic.

Since a substantial part of the current year information becomes only available in the following year, the last (incomplete) wave for every respondent is ignored unless he/she already reports a change in the first few months. For instance in 2004, for the last wave of the survey, an implausibly low level of occupational change is observed. This is mainly due to the fact that data for individuals who will declare a job situation, and possibly an occupational change for 2004 in 2005 are not available.

A similar correction method which is also considering occupational changes genuine depending on other provided information is also followed by Moscarini and Thomsson (2008) for the monthly CPS files. They employ four consecutive months to identify valid occupational changes between the second and third month. They thus do retrospective and retroactive checks on the same individual to minimize spurious changes. Sequences of four consecutive occupations that involve two transitions forth and back to the initial occupation and that do not correspond to changes in industry or class of workers or to active job search in the past month are considered suspicious. Using these filters such as active job search is attractive since the data is provided on a monthly basis. A high rate of transitions on a monthly level is more suspicious than on a yearly level. For annual data, it is more acceptable when an individual has four different occupations in four consecutive years. This is also valid for the active job search filter. For the GSOEP data it is unfortunately not possible to employ industrial affiliation data as a filter. Note that information regarding the industry is asked in question 35 (see Table 1), i.e. in the same years as the question

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

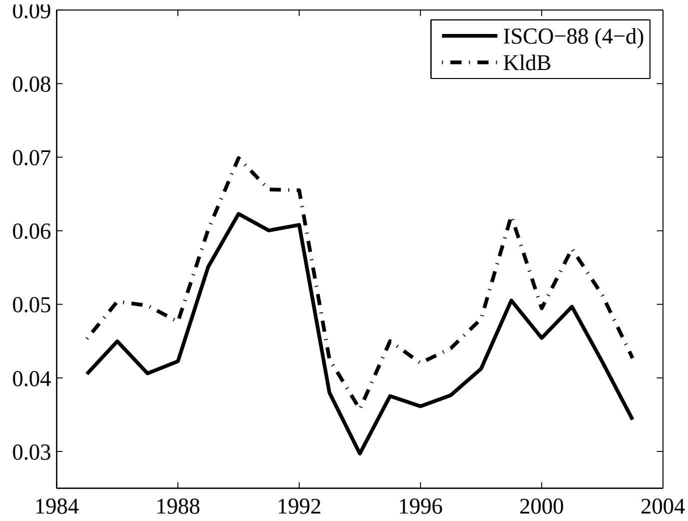

FIGURE 2. Occupational mobility considering employment-to-employment changes only.

regarding occupations. The industry changes exhibit the same dented pattern as the occupational changes. Using them in identifying occupational changes will only introduce more noise.[5]

1.6. Occupational Mobility in Germany 1985-2003

There are two measures of interest for occupational mobility. The first measure considers an individual as a "mover" if he/she is employed in two consecutive years and reporting different occupations (hereafter, employment-to-employment). The second measure also considers occupational changes after a non-employment period. For instance, if an individual is employed in the current year, but was not employed in the previous year, a switch in his/her occupation will be recorded if he/she reports a current occupation different from the one he/she reported when he/she was most recently employed.

The corrected occupational mobility patterns are plotted in Figures 2 and 3 for the base sample discussed in Section 1.2, respectively for the two definitions of mobility mentioned above.

From Figure 2 it can be seen that if one considers employment-to-employment changes only, occupational mobility averages to about 4.5-5 percent. As expected,

[5]Figures for NACE, and one- and two-digits codes provided in the CNEF files are available from the author upon request.

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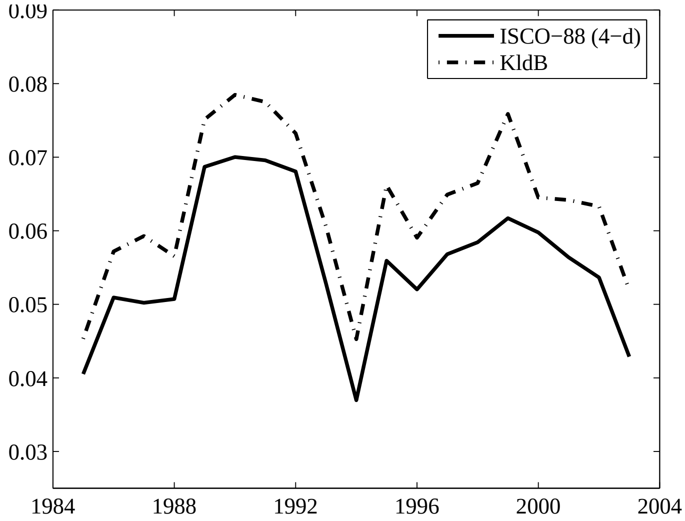

FIGURE 3. Occupational mobility including changes after non-employment spells.

the KldB which is more disaggregated leads to a higher average mobility than ISCO-88. Although there is no apparent trend, occupational mobility is clearly procyclical. Mobility was above average in 1989-1992, 1999 and 2001. The first period of high mobility is very likely to be related to the pre-unification economic boom and the unification itself. The trough in 1994 is expected to be the reflection of the 1993 recession.

If one also considers changes after non-employment spells, in general average occupational mobility rises to higher levels, see Figure 3. These higher levels reflect the fact that after being non-employed, individuals are more likely to find work in an occupation different than their last. This can be due to, for example, loss of skills or a changing economy in which certain occupation appear and disappear over time. As before, the KldB classification leads to a higher average mobility. There is no clear trend and the cyclical pattern remains unchanged. One might argue that relatively higher levels after 1993 compared to Figure 2 reflect increasing higher unemployment rates in Germany.

Similar results are found by Burda and Bachman in their recent work (Burda and Bachmann (2008)). They use the IAB dataset to analyze worker flows across sectors and occupations in western Germany during the time period 1975-2001. The analysis considers 16 broad economic sectors and 128 different occupations. Occupational mobility considering employment-to-employment flows for males aged 30-49 during the period 1985-2000 ranges between 2 and 5 percent. They find peaks around 1990 and 2000 which coincides with Figure 2. They also find that average occupational mobility decreases with age and the probability of changing occupation is higher after a non-employment spell. However, for women instead of a higher they find a lower average occupational mobility.

The only other study which analyzes occupational mobility at a disaggregated level using GSOEP is Zimmermann (1999). This study covers the period 1984-1991 for a sample of females and males, aged between 15 and 65. Individuals receiving vocational training and self-employed with their family members are dropped (see Page 311 and Table 12.3 of that study). The study presents general characteristics of the German labor market and partially deals with occupational mobility. An exhaustive set of tables containing information on job and occupational changes concerning different age groups, job status and educational levels is presented. The one- and three-digit ISCO-68 occupational codes are considered. Unfortunately, a direct comparison of the results presented in Zimmermann (1999) and the current study is not possible since the codes used in that study are not available anymore in the GSOEP after the recoding that took place in 2002. Although a direct comparison is not feasible, it is still interesting to have a closer examination of the findings of Zimmermann (1999). In the study, measurement error issues are not addressed. As can be seen from Figure 1, there are only two years with "suspicious" spikes in the period 1984-1991. Since the average occupational mobility over time is not plotted and as only averages for the whole period are presented, the spurious changes in 1989 and 1991 might very well not be discovered. The reasonable occupational mobility levels of the first years further conceal what is going on in 1989 and 1991. Average occupational mobility is reported to be around 13 percent. When now four-digit ISCO-88 codes are considered for the same sample with the same characteristics, average occupational mobility for the uncorrected data is about 14 percent (17 percent for the KldB). With the corrected data the numbers are 5 and around 6 percent respectively. Since the three-digit ISCO-68 (1,506 occupational units) is more detailed than four-digit ISCO-88 (390 occupational units) and less detailed than KldB (2,287 occupational units), occupational mobility for that period is expected to be between 5 and 6 percent which is less than half of the reported value.

In Figure 4 the occupational mobility patterns are shown for different groups

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

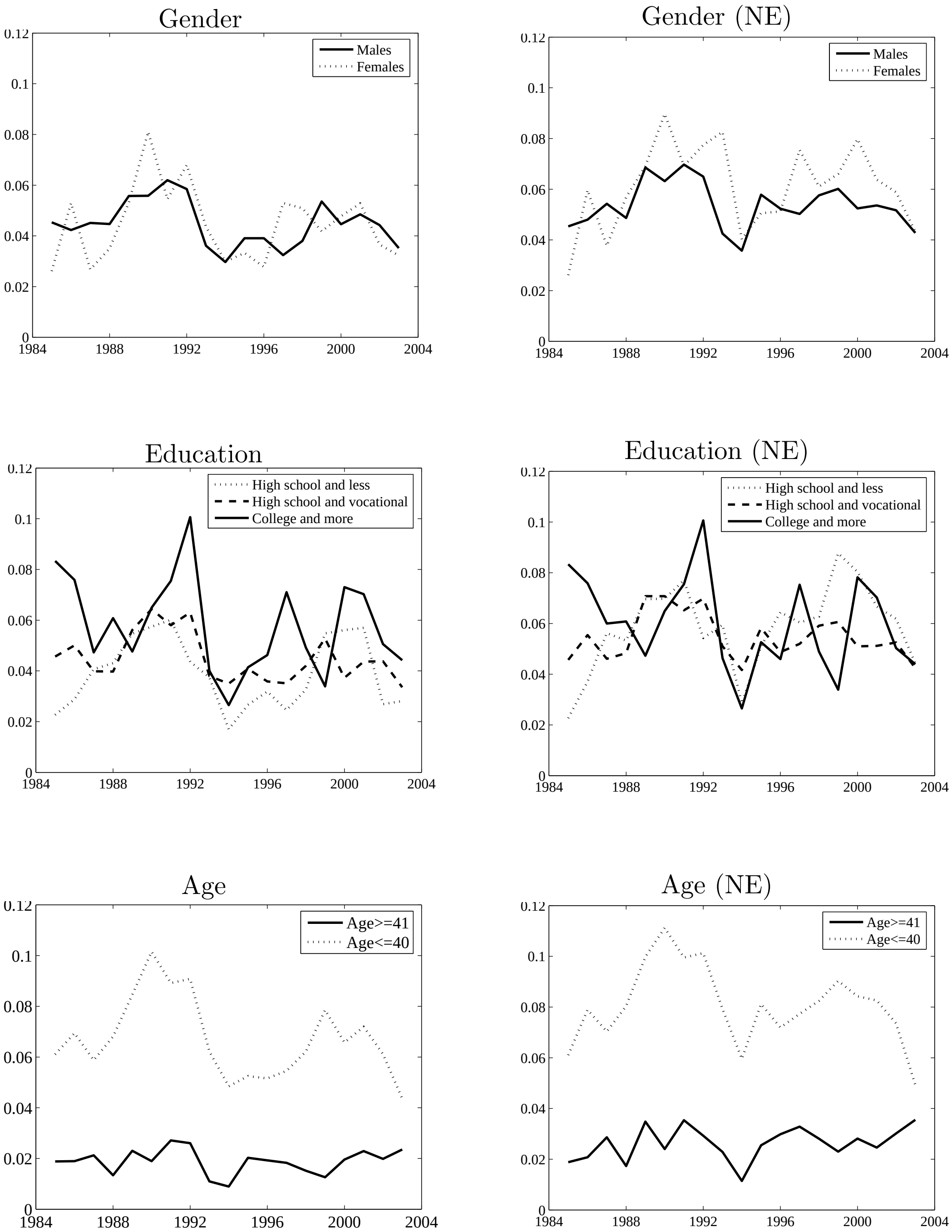

FIGURE 4. Occupational mobility for groups with different characteristics when considering employment-to-employment changes only and when including occupational changes after non-employment spells (NE).

of gender, education and age using the base sample with four-digit ISCO-88 codes.

DOI: 10.2870/13357

The figures on the left hand side concern occupational changes with employment-to-employment changes only, the figures on the right hand side also allow changes after non-employment spells. Average occupational mobility for females and males are found to be similar when only employer-to-employer changes are included. When changes after non-employment spells are added, occupational mobility for females is higher especially after the unification. Occupational mobility for females also seems to be more volatile in general.

Three broad educational groups are distinguished in the figures, namely “high school and less”, “high school and vocational” and “college and more”. The first group considers individuals who have no school degree or only high school degree without any vocational training. The second group consists of individuals who successfully completed both high school and vocational training. Individuals in the last group have at least a college degree. It is surprising to see that individuals with a college degree are more mobile on average. One might have expected a lower occupational mobility due to occupation specific education that colleges provide.

The lowest row of figures shows the occupational mobility patterns for different age groups, more specifically below or above 40, the average age in the sample. As expected, older workers are less often changing occupations. The group with younger workers also shows clearer cyclical patterns. Apart from a pronounced drop around 1994, the occupational mobility of the older group seems to be unsensitive to macro-economic fluctuations.

To analyze the effect of the sample choice on occupational mobility patterns, different samples are used in Figures 5 and 6. For all figures, the base sample considered until now is extended with a particular group of workers, namely workers from the former GDR, government sector workers, self-employed and part-time workers. Again, occupational mobility is shown for both mentioned measures using four-digit ISCO-88 codes. First, the base sample is extended to include workers in former GDR. For sake of consistency, the previously dropped individuals who were living in the GDR prior to 1989 and the individuals who move there after unification are included. Although the GSOEP started collecting data in the former GDR already in 1990, job and occupational information are only collected since 1992 so there is no data on occupational mobility for this sample before 1992. The difference in observed occupational mobility levels is stark after the inclusion of this sample.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

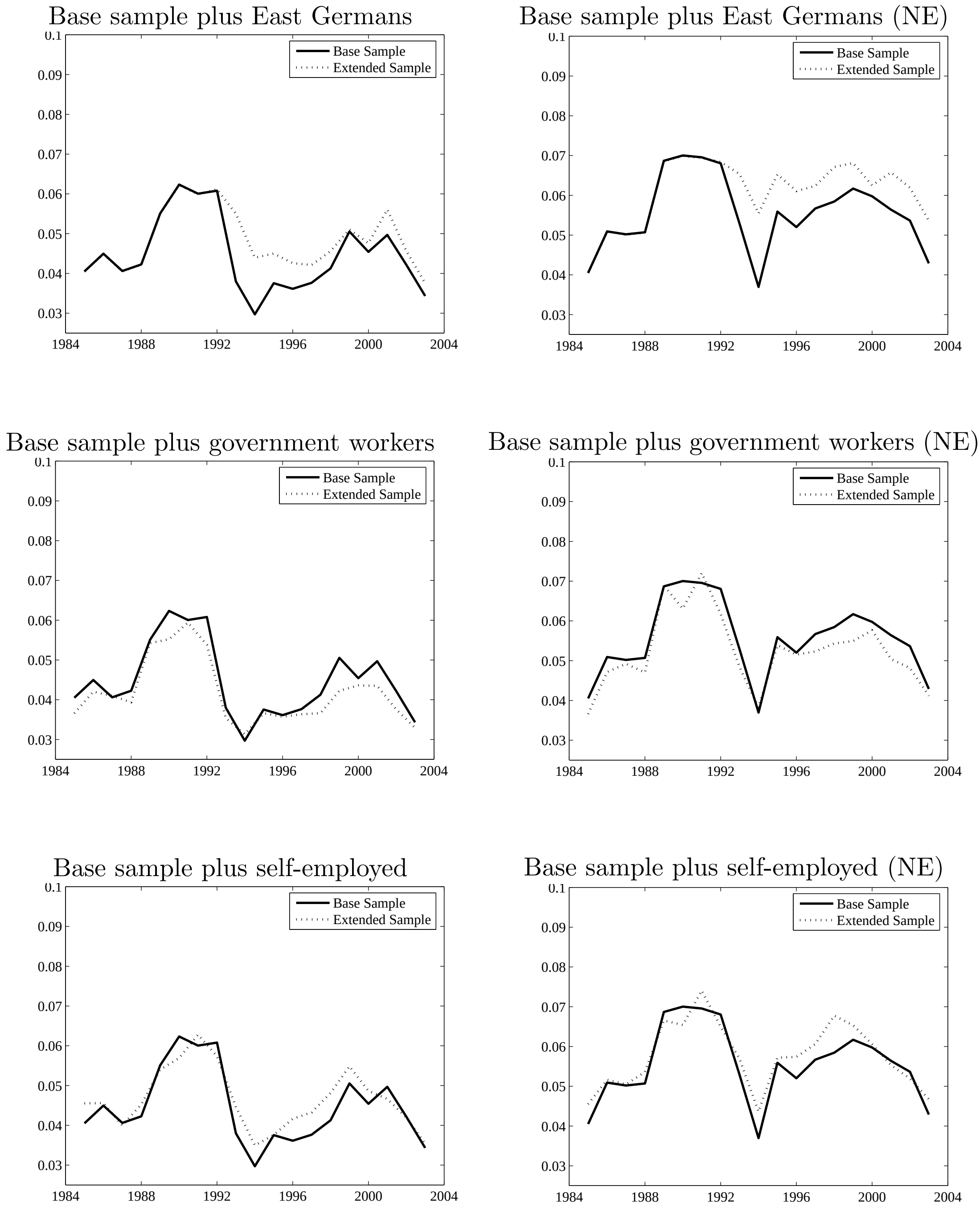

FIGURE 5. Base sample plus different groups when considering employment-to-employment changes only and when including occupational changes after non-employment spells (NE).

Especially when changes after non-employment spells are taken into account, consideration of workers from the former GDR translates to an almost constant level

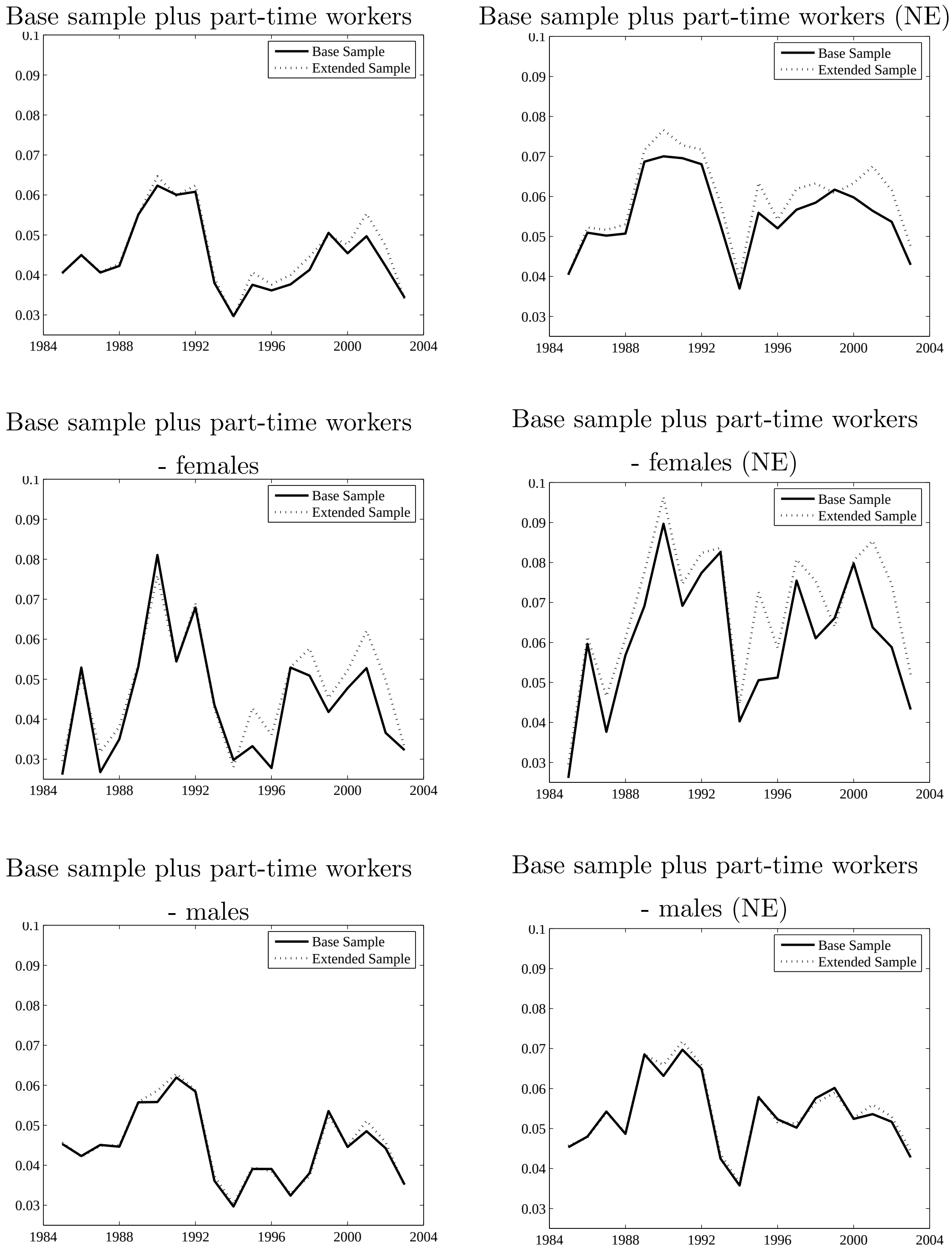

FIGURE 6. Base sample plus part-time workers when considering employment-to-employment changes only and when including occupational changes after non-employment spells (NE).

increase of one percent. The drastic decrease in 1994 is also mitigated considerably with the high occupational mobility levels of workers from the former GDR.

The higher occupational mobility of workers from the former GDR suggest that this group is in the process of occupational sorting.

The inclusion of government sector workers slightly decreases occupational mobility, while the inclusion of self-employed leads to a slight increase. Although these sectors have quite different characteristics with respect to labor contracts, the occupational mobility patterns are not affected by the inclusion of either group.

Finally, in Figure 6 part-time workers are added to the base sample. Levels of occupational mobility are not affected much, except when changes after non-employment spells are also considered. Since a large proportion of part-time workers in Germany are known to be females, it is interesting to distinguish occupational mobility according to gender. It follows that the observed higher occupational mobility levels is due to the inclusion of part-time female workers. For male workers there is almost no effect.

1.7. Conclusion

This chapter first presents unambiguous evidence for the existence of measurement errors in occupational affiliation data in the GSOEP. These errors are caused by the structure of the survey. More specifically, gathering occupational information from all respondents independent of any other job or labor market status changes in certain years in addition to coding errors generates an unacceptable spurious flows. Secondly, in order to minimize the measurement errors, the occupational data is corrected. The proposed method is based on considering occupational changes genuine only if they are accompanied with other job or labor market status changes. Thirdly, using the corrected codes, average occupational mobility patterns are presented for the last two decades in Germany. Depending on the disaggregation level of the used classification and the sample, occupational mobility averages to 4.5 to 7 percent. The pattern is found to be consistently procyclical for all samples and occupational classifications.

The particular survey structure does not only contaminate the occupational affiliation data. Industry affiliations and several social economic indices derived from occupation and industry information are also affected.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
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In the next chapter, corrected occupational mobility patterns are analyzed in more detail. The panel structure of the GSOEP and the corrected occupational mobility measures are exploited to identify the factors explaining the found patterns.

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Appendix 1.A. Details of the Data Cleaning Procedures

There are 134,182 observations of females and males aged between 17 and 65, not currently receiving education and/or vocational training, that belong to the "Residents in the FRG" or "Foreigners in the FRG" samples of the GSOEP for the period 1984-2004.

Corrections regarding occupational affiliations in the data that are explained in detail in Section 1.5 are implemented after the "job situation change" variable is constructed.

Job situation changes are identified through the variable "erwtyp$$"($$ is the symbol used for the year in the GSOEP, i.e. "erwtyp95" refers to the year 1995). It is a *generated* variable, so created after several cross checks, and it provides more consistent and reliable information than the direct survey responses. Unfortunately, this variable provides information only on whether there is a job situation change and not on the "type" and the "timing" of the change. The information on the "type" and "timing" of the changes from direct survey responses are combined with the information from the "erwtyp$$" variable. As can be seen from the survey questions in Section 1.5, "type" refers to information whether the respondent is in the labor market for the first time, comes back to employment after a non-employment period, changes job between employers, becomes self-employed or changes job within the same company. "Timing" refers to the exact month in a given year the change has realized. Accordingly, 769 observations are dropped as the "erwtyp$$" variable is missing.

As mentioned in Section 1.5, there was a change in the survey questions in 1994 which may affect the occupational mobility measures. Before 1994, respondents were asked to declare *all* the job situation changes they have experienced from the beginning of the previous year until the current date of the survey. However since 1994 they are asked to declare *only* the last change. The data suggests that the change in the survey can be ignored while identifying the job situation changes. Out of 72,482 observations before 1994, there are only 18 that show more than one job situation change for the same year. For these 18 observations, in order to be consistent with the data after 1994, only the last changes are considered. There are 119 observations for which two different changes are declared; one for the "current year" and one for the "previous year". In 38 of those cases the respondent did not participate in the survey or was not employed at the time of the previous year's survey, hence the occupational information regarding to that specific change is missing.

There are two obvious situations in which job situation change variables are suspicious. When there is a "change in the previous year" declaration and a "change in the current year" declaration made in the previous year that correspond to the same type of job situation change in the same month they are almost surely referring to the "same" change. In the whole sample there are 168 of such cases. There are also 82 cases where respondents declare two different job situation changes for the same month of the same year. Those are possibly but unlikely referring to two different changes. For instance, 63 of them refer to "come back to employment after a non-employment period" and "change job between firms" for the same month which suggest that respondents are simply providing extra information about their

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

job situation, i.e. they have come back to the employment pool with a new employer. Therefore "change in the previous year" declarations are ignored.

After correcting the job situation change variable, new occupational changes are generated. This is done in two steps; the first step considers an observed change in occupational coding genuine only if it is accompanied with a job situation change. Here, also another survey question is used which inquires information on whether the respondent has *left* his/her job since the beginning of the previous year and if so when, in order to increase reliability in identifying occupational changes. The second step allocates the occupational changes to the exact year of the change. For the respondents who do not declare any change for the "current year" and for the "previous year" in the consecutive year, occupational information is kept as it is. Also for respondents who declare a change in the occupation for the "current year" the occupational change is kept. If the worker declares that he/she has experienced a change in the "previous year" and no change in the "current year", then this information is deployed to the previous year.

Obviously, there are also cases in which the respondent is not employed at the time of the survey in a given year and the year after declares a change considering the "previous year" (2,120 observations). This raises the question whether to consider someone in the employment pool in a given calendar year when part of the year he/she is not employed. Considering someone employed when he/she works only a small part of the year would lead to an underestimation of occupational mobility. Therefore someone is considered "employed" if he/she works minimum 6 months in a given year. 914 of the observations are then considered as employed in a given year although they have reported that they are unemployed at the time of the survey. For the rest of the 2,120 observations, the change is considered to be realized in the current year. Following the same criteria, 1,141 respondents that are recorded as working in a given year but in the consecutive year declared that they have left their job the previous year, are recoded as unemployed if the total time they are employed that year is less than 6 months.

Then, relevant occupational codes and other variables, for instance, somebody becoming a government sector worker or self-employed with that job situation change, are imputed.

 DOI: 10.2870/13357

After this recoding of the job situation changes according to the exact time of the change, there are 402 double job changes in a given year. In these cases, a job situation change is reported and the year after there is another change reported corresponding to the previous year. The "erwtyp$$" variable showed a change for both years although in fact both changes are realized in the same year. These double job situation changes is translated to only 41 occupational changes (61 for the sample that also considers changes after non-employment spells).

Consecutively, 276 observations regarding the individuals who moved to the former GDR and 281 observations for who used to live in the former GDR before unification are dropped. 21,186 observations of government workers and 17,023 observations for self-employed and their family members are dropped. Furthermore, 2,037 observations for dual employed and 6,880 for part-time workers are dropped.

After generating the binary variable that identifies the occupational changes, i.e. after using all the information GSOEP provides, the first wave is dropped as the job situation change questions were not asked in 1984. However, information provided in 1984 on the occupation of respondents is used to find out the changes in 1985.

Moreover, the year that a respondent is not observed the consecutive year are not used unless he/she already declared an occupational change. The reason for that is again the fact that most of the job/occupational changes in a given year are declared in the consecutive year. That is also the reason why the last wave 2004 is dropped.

Finally, after deleting the 80 observations that are at age 17 (they were kept until this stage as they may provide information on occupations for some observations that are employed at age 18) the sample that is used for plotting occupational mobility consists of around 32,031 observations comprising employed individuals. There are small differences in the sample size depending on whether KldB or ISCO-88 is used. For the 19 years under consideration there is an average of 1,686 observations.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

CHAPTER 2

Worker Reallocation across Occupations in Western Germany

This chapter analyzes the determinants of annual worker reallocation across disaggregated occupations in western Germany for the period 1985-2003. Employing data from the German Socio-Economic Panel, the pattern of average occupational mobility is documented. Worker reallocation is found to be strongly procyclical. Its determinants at the individual level are then investigated while controlling for unobserved time-invariant worker heterogeneity. A dynamic fixed effects probit model is estimated to obtain coefficients and marginal effects. The incidental parameter bias is reduced by the method proposed in Hahn and Kuersteiner (2004). An interesting finding is that workers changing occupation are about 8 to 9 percent less inclined to experience occupational mobility in the subsequent year than workers who do not change. Except for workers with only compulsory education, the impact of age on the probability of occupational change is declining in the level of education. The unemployment rate has a negative effect on the probability of occupational changes, especially for female foreigners.

JEL classification. J24, J44, J62, C23, C25, C81.

Keywords. Dynamic Binary Choice Models, Fixed Effects, Incidental Parameter Bias, Occupational Mobility, Panel Data.

 DOI: 10.2870/13357

2.1. Introduction

This chapter studies the evolution and the determinants of worker reallocation across occupations in western Germany over the period 1985-2003. Worker reallocation across employment states, employers and industries has long been of interest to economists.[1] Movement of workers is an important labor market activity as human capital accumulation, wages and promotional gains/losses are mainly determined by worker's choice of sector, firm and labor market status. Moreover, a good understanding of worker flows at the aggregate level allows to analyze issues such as labor market flexibility and the effectiveness of job-worker matching processes i.e. allocation of workers to their most productive use in the economy. It also provides insight on the behavior of labor markets over the business cycle.[2]

Recently, worker reallocation across occupations defined at a very disaggregated level has become a focus of study.[3] A first reason is that occupations at a detailed level provide information about career changes. For instance the International Standard Classification of Occupations (ISCO-88), used in this study, has 9 occupational groups at one-digit, 28 at two-digit and 116 at three-digit. The four-digit level consists of 390 occupational units. Important career changes at this level can be easily missed even at the three-digit level. For instance, the three-digit group *Physicists, Chemists and Related Professionals* includes a variety of occupations such as *Astronomers*, *Meteorologists*, *Chemists* and *Geologists*.

Secondly, a change of occupation would imply a change of technology for the worker whereas this is not necessarily the case for a change of sector or employer. For example, a truck driver may perform the same tasks for different employers in different industries. Recent findings of Kambourov and Manovskii (2009) suggest that an important part of human capital is occupation specific. When occupational tenure is taken into account, tenure with an industry or employer has relatively little importance for the wage a worker receives. More specifically, everything else being constant, five years of occupational tenure is associated with an increase in wages

[1]See, for example, Abowd and Zellner (1985), Blanchard and Diamond (1990), Jovanovic and Moffitt (1990), Farber (1994), Schmidt (1999).

[2]See, for example, Altonji and Shakotko (1987), Topel (1991), Neal (1995), Parent (2000), Fallick and Fleischman (2001), Nagypal (2004), Cardoso (2005).

[3]See, for example, Parrado and Wolff (1999), Kambourov and Manovskii (2004*b*), Burda and Bachmann (2008) and Moscarini and Vella (2008).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

of 12 to 20 percent. This result implies that a substantial part of human capital is destroyed when the worker changes occupation.

Analyzing the levels, cycles, trends and determinants of occupational mobility is thus important for understanding various macro and labor economic phenomena. For Germany, a complete analysis has not been conducted. This is surprising as Germany is one of the world's major economies however also suffering from low employment growth and high unemployment rates. Unemployment is high and has been rising from 3.8 percent in 1980 to 11.6 percent in 2003 (see Statistisches Bundesamt). The high German unemployment rate is largely due to individuals suffering long unemployment spells whereas, for example, in the US unemployment is associated with people changing jobs as opportunities appear and dissolve and is of much shorter duration. Heckman (2002) states that one of the main reasons is the inability to rapidly respond to changes in Germany. The regulated German labor markets are characterized by centralized bargaining, high replacement rates (the percentage of earnings an unemployed worker can claim), and high union coverage. Employment protection laws that maintain the status quo make it difficult for firms to respond flexibly to changing market conditions. This study casts more light on the functioning of German labor markets by focusing on worker reallocation across occupations.

For western Germany, Zimmermann (1999) analyzes wage growth, worker movements between firms and within firms using the German Socio-Economic Panel (GSOEP) for the period 1985-1991. His study also briefly addresses occupational mobility and its determinants. As occupational mobility is only a part of a more general analysis, many interesting issues are necessarily left open. For instance, his study does not take into consideration the dynamic component of occupational mobility which is an important contribution of this study. Moreover, as discussed in Chapter 1, there are substantial measurement errors regarding occupational affiliations that are driven by the survey structure in the GSOEP. When instead of yearly averages, only the average occupational mobility for the entire period is presented, as in Zimmermann (1999), these measurement errors are concealed.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Very recently, Burda and Bachmann (2008) investigate the behavior of sectoral and occupational worker flows to assess both the extent and the dynamics of structural change in western Germany. They use the Institute for Employment Research (Institut für Arbeitsmarkt und Berufsforschung (IAB)) dataset for the time period 1975-2001. Their focus is on the gross and net worker flows involving a change of sector/occupation (for workers moving from one employer to another, from unemployment-to-employment and from nonparticipation-to-employment). Found occupational mobility patterns considering the employment-to-employment transitions have similar, level, cycle and trend as the ones presented in this study. Though they do not perform an econometric analysis to uncover the sources of these patterns.

In this study, individual level data from the GSOEP for the period 1984-2004 is used. GSOEP is ideal to study worker reallocation as it provides detailed information on the type and the time of the labor market transitions. Worker reallocation is considered according to ISCO-88 since this classification has several advantages for the purposes of this study. ISCO-88 was generated with the objective of considering occupational consequences of different technologies, incorporating new occupations and reflecting shifts in the relative importance of occupational groups. Occupations are grouped together and further aggregated mainly on the basis of the similarity of skills required to fulfill the tasks and duties of the jobs. Two dimensions of the skill concept are used: skill level, which is a function of the range and complexity of the tasks involved, and skill specialization, which reflects the type of knowledge applied, tools and equipment used, materials worked on or with, and the nature of the goods and services produced. Skills refer here to the skills required to undertake the tasks and duties of an occupation and not to the education level of the worker.

The analysis starts by discussing the patterns of *gross* and *net reallocation* and the difference between them, namely *churning*, during the sample period. Gross reallocation of employment is defined as the fraction of workers who are employed in two consecutive years and change occupation, at least once, in between. This provides a measure of average worker mobility at the annual level. Net reallocation is one half of the sum of the absolute changes in occupational employment shares. Churning can be seen as a measure of the turbulence in the labor markets. It represents the excess reallocation of employment not explained by the net distribution.

 DOI: 10.2870/13357

Gross reallocation is found to be strongly procyclical. It follows the Gross Domestic Product growth in western Germany. The expansion of the economy before and during the German Reunification (October 1990) and the aftermath recession of the 1993 and the following recovery is clearly observed in employment reallocation across occupations as well. Net reallocation is less procyclical. Another interesting finding is that in 1991 the churning is clearly higher than the net reallocation. This reflects the turbulence that the western German labor markets went through after the German Reunification. There is no trend in overall occupational reallocation over the last two decades.

To understand the determinants of gross reallocation, an empirical model of occupational mobility at the individual level is estimated. In such a model, unobserved time-invariant individual heterogeneity has importance as some covariates are decision variables and individual heterogeneity, most of the time, represents variation in tastes or technology. For instance, risk aversion may drive occupational choice. Moreover, individuals are also likely to make other decisions in life such as education or marriage under the influence of this trait. Estimation results may have incorrect implications if this kind of endogeneity is ignored.

Exploiting the panel structure of the dataset, a fixed effects approach is adopted to control for the unobserved time-invariant worker heterogeneity. Correlation between covariates and individual fixed effects is allowed. The model is estimated by maximum likelihood. Additionally, marginal effects can be computed since estimates of individual fixed effects are obtained.

There is a methodological problem involved in using the maximum likelihood method for nonlinear dynamic fixed effects estimation, namely the incidental parameter bias. As first highlighted by Neyman and Scott (1948), replacing unobserved fixed effects by inconsistent sample estimates leads to biased estimates of the other model parameters. This bias arises in maximum likelihood estimation of dynamic linear models as well as in static or dynamic nonlinear models with fixed effects. In this study, a method proposed by Hahn and Kuersteiner (2004) is implemented to address the incidental parameter bias.

Results from the econometric investigation can be summarized as follows. The lagged occupational mobility is found to be statistically significant and negative. Marginal effects suggest that workers who do change occupation are about 8 to

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

9 percent less inclined to change occupation in the subsequent year compared to workers who do not change occupation in a given year. Moreover, depending on the worker's characteristics, the effect varies from -14 to -2 percent. As expected, the probability of an occupational change decreases with age. For workers with more than compulsory education, the impact of age on the probability of occupational change is declining in the level of education, i.e. although workers become less inclined to change occupation with age, this effect is less pronounced for workers with high education levels. An increase in the regional unemployment rate has a negative impact on the probability of occupational change. Female foreigners are the most affected group by changes in regional unemployment rates with an average marginal effect of -7 percent. The effect for the rest of the population is only around -2 to -1.5 percent.

This chapter is organized as follows. The next section describes the dataset. Section 2.3 provides information on the occupational affiliations in the GSOEP. Section 2.4 documents and discusses the gross and net reallocation as well as churning and Section 2.5 presents the estimated model and the covariates. Section 2.6 presents the results from the econometric investigation and finally Section 2.7 concludes. The Appendix provides the summary statistics of the estimation sample and the estimation results.

2.2. German Socio-Economic Panel

GSOEP started in the Federal Republic of Germany (FRG) in 1984 as a nationally representative longitudinal survey of persons and private households with around 12,000 respondents (SOEPGroup (2001)). For this study, individual level data from the *Residents in the FRG* and the *Foreigners in the FRG* samples for the period 1984-2004 are employed. The latter sample covers persons in private households with a household head from the main foreigners groups of guestworkers, namely Greeks, Italians, Spaniards, Turkish and former Yugoslavians (hereafter *foreigners*), while household heads in the former sample are from German origin (hereafter *natives*). In June 1990, GSOEP expanded to the former German Democratic Republic (GDR). The *Residents in the GDR* sample is not employed in this study as the aim is to understand occupational reallocation in competitive labor markets. Observations for persons who moved to the former GDR states or persons

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

who were residing in the GDR before the reunification are therefore also excluded from the analysis.

Representativeness of the GSOEP is maintained in the following ways. Children within households of the original panel reaching age 16 enter the GSOEP. In case of geographical mobility, persons are followed within Germany. Split offs from the initial household remain in the panel as new households. When third persons move into an existing GSOEP household they are also surveyed and followed up even in case of subsequently leaving that household. Finally, when there is a successful interview after a drop-out year, respondents are also given a small questionnaire with questions regarding the drop-out year (Haisken-DeNew and Frick (2003)).

Furthermore, GSOEP provides detailed information on labor market transitions, e.g. transitions across the labor market states, across firms or within firms. Information on the exact time of these transitions is collected either via directly asking for the month and year of the change or via questions based on a calendar.

There are other German micro datasets that can be employed for analyzing worker reallocation, most notably the Microcensus and the IAB dataset. Microcensus has an ideal representative sample which considers 1 percent of all households in Germany. However, individuals are followed for a maximum of four consecutive years only. Moreover, for confidentiality reasons, the only available classification in the dataset, which is the national occupational classification (KldB), is provided at three-digit level instead of four.

The IAB dataset is a 2 percent random sample of all employees registered with the German social security system over the period 1975-2001. As the aim of the data collection is to provide a social insurance account for each employee, and as substantial legal sanctions are imposed for incorrect or missing notifications, the information provided is very reliable. Occupational information regarding employer changes is provided daily but occupational changes regarding internal mobility are registered late. Therefore, some occupational mobility is not recorded, such as when an employee changes his/her occupation and the match is destroyed before the next annual notification. Moreover due to confidentiality requirements, the IAB dataset is anonymized. The original data contains occupational information at the four-digit KldB level. In order to anonymize the occupational information, the IAB has cut these codes. For instance, Burda and Bachmann (2008) uses the

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

affiliation only with 128 different occupations. Another disadvantage of this dataset is that all civil servants and self-employed persons apart from apprentices as well as employees with earnings below a certain threshold-and therefore not subject to social insurance contributions-are excluded. In 1995, the employees registered with the social insurance system in western Germany accounted for around 80 percent of the total workforce, but the coverage varies over individual occupations and industries (Bender, Haas and Klose (2000)).

2.3. Occupational Information in the GSOEP

GSOEP provides three major classifications for occupations, namely KldB, ISCO-88 and CNEF code. The first is the national classification system of the German Federal Statistical Office, the second is the International Standard Classification of Occupations of the International Labor Office (ILO) and the third is the classification is of the Cross National Equivalent File (Burkhauser et al. (2000)).[4]

In this study ISCO-88 at the four-digit level is employed. The ILO of the United Nations produced the International Standard Classification of Occupations in 1958 for the first time and then revised it in 1968 and 1988 in order to make international comparisons of occupational statistics feasible and to provide an example for countries developing or revising their national occupational classifications. ISCO-88 is a nested classification of occupations at the four-digit level. It consists of 9 major groups at the one-digit level. Within these 9 groups there are three further levels: 28 major subgroups, 116 minor groups and 390 unit groups, i.e. classification at the four-digit level corresponds to 390 different occupations (ILO (1990)).

The main advantage of the ISCO-88 classification over the others is its structure. ISCO-88 at the four-digit level is based on two concepts: the *job* (kind of tasks and duties executed) and *skill*. Job is the statistical unit classified by ISCO-88 and a set of jobs whose main tasks and duties are characterized by a high degree of similarity constitutes an occupation. The characteristics of the job performed are the basis of any recent occupational classification whereas the logic of classification depending on skill requirements is a novelty of ISCO-88 compared to other classifications.

[4]This file contains variables that are generated according to the same definitions in order to allow comparative studies among the GSOEP, the Panel Study of Income Dynamics (PSID) of the US, the British Household Panel Study (BHPS) and the Canadian Survey of Labour and Income Dynamics (SLID).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Dependence on skill requirements does not mean that the skills necessary to perform the tasks and duties of a given occupation can be acquired only through formal education. The skills may be, and often are, acquired through informal training and experience. In addition, it should be emphasized that the focus in ISCO-88 is on the skills required to carry out the tasks and duties of an occupation and not on whether a particular worker having some occupation is more or less skilled than another worker in the same occupation.

This focus on skill requirements of ISCO-88 is important considering recent research finding evidence on the occupational specificity of human capital (Kambourov and Manovskii (2009)). They show that human capital is not primarily employer or industry but mostly occupation specific, e.g. when a truck driver switches industries, say, from wholesale trade to retail trade, or employers, he/she looses less of his/her human capital generated by the truck driving experience than when he/she switches his/her occupation and becomes a hairdresser.

Until 2002, GSOEP provided ISCO-68 codes. In 2002, Hartmann and Schuetz recoded the occupational and industrial affiliations retrospectively (Hartmann and Schuetz (2002)). The aim of this recoding was to update the ISCO-68 to ISCO-88. They went back to the original questionnaires and depending on the responses, recoded occupations first according to the KldB and then to ISCO-88.

To understand the factors driving occupational reallocation, it is important to have consistent and reliable occupational affiliation data. However, a vast literature documents measurement errors in occupational affiliations.[5] For the GSOEP, measurement errors in the occupational affiliations and a correction method are discussed extensively in Chapter 1.

2.4. Worker Reallocation across Occupations in Western Germany: Averages, Cycles and Trends

Before analyzing the determinants of worker reallocation at the individual level, further insights can be obtained from observing its aggregate patterns in terms of gross and net reallocation over the last two decades. Gross reallocation is a measure of average worker mobility at the annual frequency and considers the fraction of

[5]See, for example, Mellow and Sider (1983), Murphy and Topel (1987), Mathiowetz (1992), Polivka and Rothgeb (1993), Neal (1999), Kambourov and Manovskii (2004*a*), Moscarini and Thomsson (2008), Moscarini and Vella (2008).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

workers who are employed in consecutive years and who change occupation at least once. Net reallocation is one half of the sum of the absolute changes in occupational employment shares.[6] Due to technological progress, occupations continuously receive positive and negative shocks. As a result, some occupations are born and some die. Hence, net reallocation can be seen as representing labor demand. It is computed on the same sample as used for gross reallocation. Also of interest is churning, which is the difference between gross and net reallocation. It represents the excess reallocation of employment not explained by the net distribution and can thus be seen as a measure of the turbulence in the labor markets.

The sample under analysis is chosen such that it represents the workers in a competitive labor market. More specifically, it consists of native and foreigner females and males, aged 18-65, residing in western Germany, working full-time, not working in the government sector, not self-employed or living in the household of a self-employed, and not dually-employed. Table 1 summarizes the sample characteristics. Moves that make workers leave or enter the sample are not included since these occupational changes are typically accompanied by other decisions like starting ones own business or transiting into full-time employment from part-time employment when children start schooling. A more detailed analysis of gross worker reallocation across occupations for different samples regarding age, education, gender, residence etc., is already presented in Chapter 1.

As the aim of this study is to understand why workers change occupations rather than labor market status, only occupational changes from employment-to-employment without a significant period of unemployment are considered. The advantage of this approach is that decisions of changing occupation and decisions of participation in the employment pool are separated from each other. However this choice also implies that any result of this study hold for the employed workers only.

Figure 1 shows the gross and net reallocation as well as churning across four-digit ISCO-88 occupations for the period 1985-2003. Gross reallocation averages around 5 percent per year. Double changes in a year are also counted in this measure. Such cases are rare (around 2 percent) and they are considered as a single change in the

[6]This measure is used in Jovanovic and Moffitt (1990) for sectoral and in Kambourov and Manovskii (2004*b*) for occupational mobility.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

estimation. However, one should be aware that this figure may be an underestimation of the true average mobility as the occupational mobility at the individual level is identified conditioning on other types of job or labor market status changes as explained in Chapter 1. Net reallocation averages around 2.7 percent per year, which is an important proportion in explaining the total worker reallocation. Churning accounts for slightly less than half of the total reallocation.

Findings considering occupational mobility from other studies can be summarized as follows. Kambourov and Manovskii (2004*b*) analyzes the US with the Panel Study of Income Dynamics (PSID) dataset while defining occupational mobility as the fraction of currently employed individuals who report a current occupation different from their most recent previous report of an occupation. For the period 1968-1997, the average occupational mobility of male workers at the 1-digit level is found to be 13 percent. This figure increases to 19 percent at the 3-digit level. Mostly prior to 1984 mobility rates are increasing; in later years they are more stable. Their findings suggest a mildly procyclical average occupational mobility whereas net occupational mobility is countercyclical. Moscarini and Vella (2008) using monthly the US Current Population Survey (CPS) data for the period 1979-2004 present that reallocation of employed men across three-digit occupations averages about 3.5 percent per month and is strongly procyclical. For Germany, Burda and Bachmann (2008) document average occupational mobility, considering employment-to-employment transitions only during the period 1980-2000. For females and males between age 16 and 29, it amounts to 4.9 and 6.2 percent respectively. It decreases to 2.3 and 3.1 percent for mid-career females and males (age 30-49) and finally to and 0.8 and 0.9 percent for female and male workers in the period before retirement (age 50-64).

A comparison of gross and net reallocation with the Gross Domestic Product growth in western Germany over the last two decades reveals that gross reallocation of workers is strongly procyclical, see Figure 2. Similar analysis for the US also finds that worker reallocation is procyclical.[7] This behavior might seem at odds with a truly Schumpeterian view, in which recessions promote a more efficient allocation of resources by cleansing out bad investments with low productivity and by freeing up resources for more productive uses. This Schumpeterian view is confirmed for

[7] See, for example, Jovanovic and Moffitt (1990), Nagypal (2004) and Moscarini and Vella (2008).

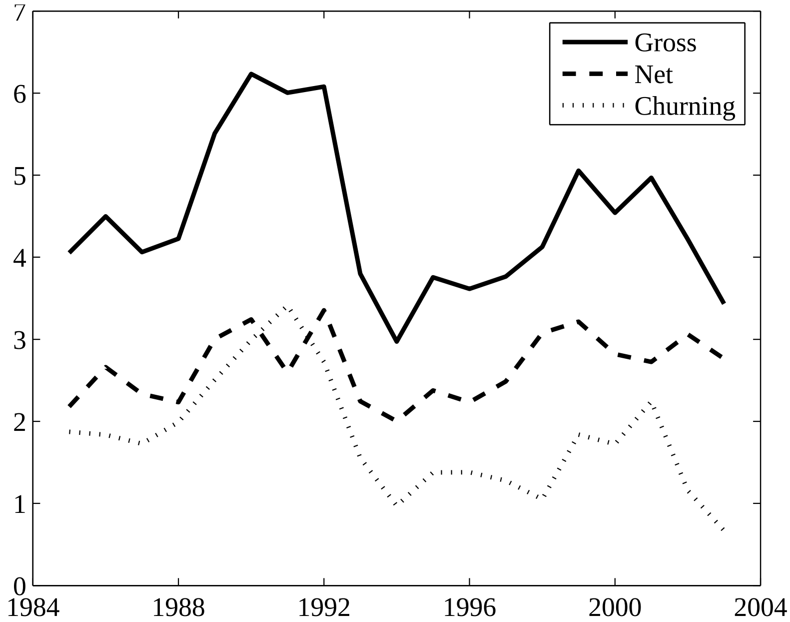

FIGURE 1. Occupational reallocation at the four-digit ISCO-88 level (percentages).

FIGURE 2. Gross domestic product growth in western Germany (percentages).

job reallocation in the manufacturing sector by the work of Davis, Haltiwanger and Schuh (1996), however not for worker reallocation. In fact, Barlevy (2002)

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

allows workers to search on the job as well as through unemployment in his model and shows that during recessions workers reallocate more slowly into their most productive uses. Even though the economy cleanses out its most inferior matches, most workers are stuck in mediocre matches and fewer high quality matches are created. This is because employers create fewer vacancies during recessions which makes it difficult for workers to move.

From the figures it is clear that net reallocation is also procyclical, although it is far less pronounced. Another interesting finding from these figures takes place during the reunification period. In 1991, just after the Reunification, the turbulence clearly surpasses the net reallocation. However, the effect is distributed over the period 1990-1992 for the gross reallocation due to the 1990-1991 economic boom and its effect in 1992. The economic crisis that took place in 1992-1993 is reflected as a huge drop in gross reallocation in 1993-1994. There appears to be no trend in overall occupational mobility.

2.5. Estimating the Determinants of Occupational Mobility

2.5.1. Model and Estimation Method. Consider the following empirical model of occupational mobility at the individual level:

$$MOB_{it} = \mathbb{1}\left\{MOB_{it-1}\gamma_0 + x_{it}'\beta_0 + \alpha_i + \epsilon_{it} > 0\right\}, \qquad \begin{array}{l} i = 1, \ldots, N, \\ t = 1, \ldots, T(i), \end{array} \tag{1}$$

where N denotes the total number of individuals and since the sample is an unbalanced panel, $T(i)$ the number of periods for person i. MOB_{it} is the binary dependent variable which takes value 1 in a given year if the worker changes occupation and 0 otherwise, $\mathbb{1}[.]$ is the indicator function, MOB_{it-1} is the lagged dependent variable, x_{it} is the vector of other covariates, γ_0 and β_0 are the parameters of interest, α_i is the individual fixed effect and ϵ_{it} is a time-individual specific random shock.

This is an error component model where the error term, $\alpha_i + \epsilon_{it}$, is composed of a permanent individual specific term α_i and a transitory shock ϵ_{it}. This framework has a particular advantage as it controls for unobserved time-invariant individual heterogeneity. Such heterogeneity is important as labor market outcomes of observably equivalent individuals are markedly different in terms of compensation and employment histories as it is described in the seminal model of Roy (1951). More recently, Abowd, Kramarz and Margolis (1999) using an employer-employee

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

dataset find that individual effects are statistically more important than firm effects in explaining compensation and performance outcomes. They show that the entire inter-industry wage differential is explained by the variation in average individual heterogeneity across sectors. It is individual effects, not firm effects, that form the basis for most inter industrial salary structure.

If not accounted for, unobserved individual heterogeneity can result in misleading inferences especially when it is correlated with the covariates. In many economic applications, this is the case as covariates are decision variables and individual heterogeneity usually represents variation in tastes or technology. For instance, Guiso and Paiella (2001) show that risk aversion plays an important role in occupational choice. More specifically, they find that it influences the choice of becoming self-employed or public sector employee. Risk averse individuals are also found to choose occupations where large negative income events occur with a relatively low probability. Similarly, it is likely that risk aversion also is important for decisions regarding education and marital status. In order to control for such endogeneity, a fixed effects approach exploiting the panel structure of the data is followed. The individual effect α_i is allowed to be correlated with the covariates x_{it}. The transitory error ϵ_{it}, however, is assumed to be independent of x_{it} and independently and identically distributed over time.

One can expect a negative correlation between job separations and tenure, simply because lower probabilities to change jobs/occupations imply longer periods at the same firm/occupation. On top of this purely statistical relationship, Jovanovic (1979) and Pissarides (1994), among others, find evidence for true state dependency i.e. the probability of change is partially explained by tenure. Thus, one might expect that the probability of occupational change depends on previous changes. For this reason, lagged occupational mobility is included in the estimation as an additional covariate.[8]

There are several models and methods of controlling for unobserved heterogeneity in using panel data (see Chamberlain (1994), Arellano and Honore (2001)). Though, in the specific model presented above, the discrete choice character with the dynamic component restricts the possibilities considerably. A feasible method is

[8]Ideally, one would like to include occupational tenure, however, GSOEP does not provide this variable (nor can it be constructed).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

random effects as it bypasses the incidental parameters problem by integrating out the individual effects. This method, however, requires strong assumptions: both α_i and ϵ_{it} need to be normally distributed and uncorrelated with the covariates. Although in a recent study Vella and Verbeek (1999) propose a more flexible approach, the distributional assumption of normality cannot be relaxed. Other available estimators usually have some practical limitations, most notably only providing estimates for the primary slope parameters which precludes the computation of the marginal effects (see e.g. Chamberlain (1985), Honore and Kyriazidou (2000)). This is a major drawback as in nonlinear models the objects of interest are in general the effects averaged over individuals rather than the parameters.

In this study, a dynamic fixed effects maximum likelihood approach, where individual effects α_i, $i = 1, 2, \ldots, N$, are considered as parameters to be estimated, is followed. Greene (2002) presents a practical solution that allows estimating nonlinear models with possibly thousands of dummy variable coefficients.[9]

There is a methodological difficulty associated with maximum likelihood estimation of nonlinear and/or dynamic models with fixed effects. In these models, parameter estimates suffer from the incidental parameters problem when individual heterogeneity is left completely unrestricted (Neyman and Scott (1948)). The problem arises because unobserved fixed effects are replaced by inconsistent sample estimates, which in turn leads to biased estimates of the other model parameters. Recently, many studies proposing methods to overcome this problem became available.[10]

To get some intuition for the incidental parameter bias, suppose for the moment that the time horizon is identical for all individuals, so $T(i) = T$ for all

[9]Newton's iterative method is used to find the parameters for which the derivative of the loglikelihood function is zero; the estimates are updated using the inverse of the Hessian and the deviation from zero. When K denotes the number of covariates, the Hessian is an $(N+K)\times(N+K)$ matrix, which makes direct inversion very slow, if at all possible. Computing the inverse is simplified by taking advantage of the sparse nature of the Hessian. The resulting computation than involves matrices of at most size $K \times K$.

[10]See, for example, Lancaster (2000), Hahn and Kuersteiner (2002), Hahn and Kuersteiner (2004), Hahn and Newey (2004), Carro (2003), Fernandez-Val (2007), Fernandez-Val and Vella (2007).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

$i \in \{1, \ldots, N\}$. Let $g(y_{it}, x_{it}; \theta, \alpha_i)$ be the likelihood of obtaining dependent variable y_{it} for covariates x_{it}, when the coefficients are θ and the fixed effect is α_i.[11] The true parameters θ_0 and α_{i0} then satisfy

$$(\theta_0, \{\alpha_{i0}\}_{i=1}^N) = \arg \max_{\theta, \{\alpha_i\}_{i=1}^N} E\left[\frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T} g(y_{it}, x_{it}; \theta, \alpha_i)\right]. \tag{2}$$

The sample analogue can be written as follows:

$$\hat{\alpha}_i(\theta) = \arg \max_{\alpha_i} \frac{1}{T}\sum_{t=1}^{T} g(y_{it}, x_{it}; \theta, \alpha_i), \tag{3}$$

$$\hat{\theta} = \arg \max_{\theta} \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T} g(y_{it}, x_{it}; \theta, \hat{\alpha}_i(\theta)). \tag{4}$$

Hence, for a candidate maximizer θ first the likelihood maximizing fixed effects $\hat{\alpha}_i(\theta)$ are computed which are then used in the maximization problem of θ. However, these sample estimates of α_i are inconsistent since there are relatively few observations of each individual in the data, so $\hat{\alpha}_i(\theta_0) \neq \alpha_{i0}$. Since these inconsistent estimates of the fixed effects are used while estimating θ, the coefficients are biased. To see this better, suppose that $N \rightarrow \infty$ with T fixed, then the best estimate θ_T of the true parameter is

$$\theta_T = \arg \max_{\theta} \lim_{N \rightarrow \infty} \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T} g(y_{it}, x_{it}; \theta, \hat{\alpha}_i(\theta)). \tag{5}$$

However, since $\hat{\alpha}_i(\theta_0) \neq \alpha_{i0}$, the estimate θ_T will not be equal to the true parameter θ_0. Only when the number of periods T becomes arbitrarily big, it holds that $\theta_T \rightarrow \theta_0$.

There are several ways of addressing the incidental parameter bias. Hahn and Newey (2004) and Hahn and Kuersteiner (2004) consider bias correction of the estimator either by panel jackknife or deriving analytical bias formulas; Woutersen (2002) proposes a correction of the estimating equation and Lancaster (2000) by modifying the maximum likelihood function. In this study the analytical bias correction approach designed for dynamic nonlinear models proposed by Hahn and Kuersteiner (2004) is employed. This method uses that $\theta_T = \theta_0 + \frac{\mathcal{B}}{T} + \mathcal{O}(T^{-2})$ for some $\mathcal{B}$ under smooth moment conditions. For $N \rightarrow \infty$, the difference between the

[11] Obviously $y_{it} = MOB_{it}$ and $\theta_0 = (\gamma_0, \beta_0)$ in the current model.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

real coefficient and its estimate becomes

$$\theta_T - \theta_0 \xrightarrow{p} \frac{\mathcal{B}}{T} + \mathcal{O}\left(\frac{1}{T^2}\right). \tag{6}$$

Hence, when $\mathcal{B}$ is known, the estimator $\theta_T - \frac{\mathcal{B}}{T}$ would be a bias corrected estimator of θ_0. The difference between the static and dynamic bias corrections is that the latter also corrects for covariances over time arising while computing the estimate of the bias.[12]

The main advantage of fixed effects maximum likelihood estimation is that marginal effects can also be computed. However, due to the incidental parameter bias these effects will be biased as well. Using the bias corrected coefficients, Hahn and Newey (2004) also derive a bias corrected estimator for the marginal effects, which is extended for the dynamic case by Fernandez-Val (2007). An additional advantage of this method is that the initial conditions problem discussed in Heckman (1981) is avoided. Hence, there is no need for imposing restrictions on the initial values of the process.

2.5.2. Covariates. To estimate the determinants of occupational mobility, covariates that represent worker characteristics and macro economic situation are selected. More specifically the employed covariates are dummies for lagged occupational mobility and marital status, a year dummy for 1991, workers' age interacted with their educational attainment, regional unemployment rates interacted with origin-gender background of the worker and dummies for one-digit ISCO-88 occupational groups.

The lagged occupational mobility dummy is employed to investigate the presence of the dynamic effects. The estimation method allows the identification of the true state dependence and serial persistence arising from individual heterogeneity. State dependence refers to the effect that past outcomes might have on the current outcome. Heterogeneity refers to unmeasured variables that influence the current outcome but are themselves not influenced by past outcomes.

The direction in which lagged occupational mobility affects the probability of a current occupational change is not obvious. A positive effect of the lagged occupational mobility dummy is suggested by the job-matching theory. Jovanovic

[12]To estimate these covariances, an average of the sample covariances is computed with the variables at periods $t-1$, t and $t+1$, as advised by Hahn and Kuersteiner (2004).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

(1979) argues that separation brings separation. The underlying reasoning is that job separations may force some workers to accept jobs in new occupations, wasting some accumulated occupation specific knowledge, and thus raise expected subsequent separations and mobility. Due to the occupational matching component in productivity, the same mechanism is also relevant for occupational mobility. On the other hand, a negative effect can also be expected due to successful matches. The argument is straightforward: when a worker changes an occupation, he/she thinks that the new occupation is the best available match. Unless the job is not according to expectations, the worker is thus expected to be satisfied with the new occupation. Hence, workers who have changed occupation recently are expected to be less likely to change in the following year. The empirical evidence will cast light on the relative importance of these opposing influences.

To assess the importance of family considerations on the probability of occupational change, a marital status dummy is included in the estimation. Family considerations can be of high importance for various reasons. For instance, having a spouse might limit occupational mobility which necessitates geographical mobility. A potentially interesting job which is far away from the current residence might not be taken when the spouse's own activities/career plans block any residential change.

To see the impact of educational attainment four different levels are distinguished, namely *no degree* (only compulsory education of 7 years), *high school* (secondary education but no further vocational training), *high school with vocational training* (secondary school with apprenticeship or other vocational training) and *college* (college and more). The German Apprenticeship System is a vocational training programme, based on the dual system of *on the job training*, which is provided by the firm, and *school education*, which is provided by the state and takes on average 1 or 2 days a week. In school, apprentices receive not only general education but also schooling specific to their occupation. Apprenticeship is completed in between 2 and 3.5 years. Today, around 60 percent of each cohort in Germany undertake apprenticeship training. In 1990, there were approximately 370 recognized apprenticeship occupations which included both blue and white collar professions. These cover many occupations which require college attendance in the UK and the US (Dustmann and Meghir (2005)). Hence, there is a considerable difference between workers having a high school degree only and those having a high school degree

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

with apprenticeship/vocational training. As the latter have more occupation-specific training background, it is important to consider them as separate groups.

Due to the estimation method, time-invariant variables are not identified as they cannot be isolated from the individual fixed effects. However, one might expect that occupational mobility decisions are affected by workers' education/experience levels as well. Although it would have been optimal to relate the educational attainment levels with actual labor market experience, unfortunately this comes at a cost. GSOEP does not provide a readily available experience variable. In theory, this variable can be constructed using biography and calendar files. However, this implies a further drop in the number of observations as biography and calendar information is missing for some individuals. Therefore, age is used instead of experience to see the impact of experience on occupational mobility. Educational attainment dummies are interacted with age to allow the impact of age to differ across the four educational background groups.

Through labor market attachment, origin and gender are expected to have an impact on occupational mobility decisions. DiPrete and Nonnemaker (1997) find for instance that in the US women and non-whites are more affected by labor market turbulence than men and whites. For Germany, it is important to distinguish between natives and foreigners in addition to gender. To not impose equal effects of gender for both natives and foreigners, four origin-gender dummies are employed, namely foreigner female, foreigner male, native female and native male. Due to their different characteristics, regional unemployment is expected to affect these groups differently. A high regional unemployment rate will probably decrease the probability of voluntary occupational changes: workers are less inclined to change occupation since there are fewer vacancies available. So, regional unemployment is taken as a measure of labor market tightness affecting workers' career choices. To measure the extent of its effect, the four origin-gender dummies interacted with regional unemployment rates are included in the estimation. It should be pointed out that regional unemployment rates may not be fully exogenous. There might be some simultaneity bias, i.e. it could be the case that not only occupational mobility depends on unemployment rates but also that unemployment rates depend on the occupational mobility. When occupational mobility is high, i.e. individuals with jobs easily migrate to new jobs, this suggests a high number of vacancies. Eventually

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

this can decrease the average unemployment rate. Although this may have some impact on the results, this type of endogeneity is not addressed in this study.

As discussed above, there was considerable turbulence in the German economy due to the Reunification which is also suggested by the high level of churning in 1991 (see Figure 1). Thus, a dummy variable is included in the analysis to account for this specific event.

Finally, one may suspect that the occupation itself may have a role in determining mobility decisions. To control for these effects, dummies for the one-digit ISCO-88 occupational groups are included as covariates. These groups are *Professionals*, *Technicians and Associate Professionals*, *Clerks*, *Service Workers and Shop and Market Sales Workers*, *Skilled Agricultural and Fishery Workers*, *Craft and Related Trades Workers*, *Plant and Machine Operators and Assemblers*, *Elementary Occupations* and *Legislators, Senior Managers and Officials*. However, these variables can be endogenous as they are decision variables. More specifically, a time-variant effect can have an impact on the choice of occupation. In this study, this kind of endogeneity is not taken into consideration.

2.6. Estimation Results

2.6.1. Fixed Effects Probit Estimates with Hahn-Kuersteiner Bias Correction. Table 2 presents coefficients and marginal effects of the fixed effects probit model where the bias is reduced by applying the method of Hahn and Kuersteiner (2004). Four different specifications are considered to observe the impact of various variables and to see the sensitivity of the estimates. The first column of Table 2 focuses on the impact of worker characteristics on the probability of occupational mobility abstracting from macroeconomic variables. It includes lagged occupational mobility and marital status dummies and four age and educational attainment interaction terms. In the next columns the following variables are subsequently added: four origin-gender variables interacted with regional unemployment rates (Column (2)), the 1991 dummy (Column (3)), and the one-digit ISCO-88 occupational dummies (Column (4)).

The lagged dependent variable has a statistically significant negative effect in all specifications. Results suggest that, compared to workers who do not change occupation in a given year, workers who do change are about 8 to 9 percent less

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

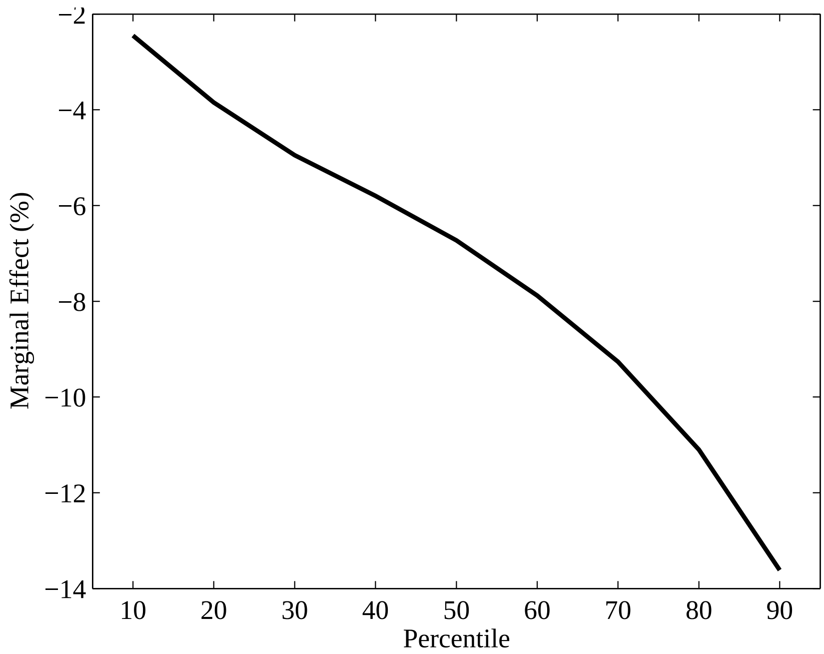

FIGURE 3. The marginal effects of lagged mobility according to percentiles.

inclined to change occupation in the subsequent year. This result is found to be robust across all specifications. The found negative effect contrasts with findings of some recent studies. For example, Moscarini and Vella (2008) construct a pseudo panel based on cohorts to deal with endogeneity and find a positive effect of lagged occupational mobility for the US. This issue will be discussed in detail in the next subsection.

Figure 3 shows the impact of lagged mobility for workers with different probabilities of occupational change. On the horizontal axis individuals are ranked according to their propensities to change occupation; on the vertical axis are the marginal effects. This figure uses the findings of Column (3), which as discussed below, is the preferred specification. The impact of lagged occupational mobility is changing considerably depending on the propensity to change occupation. Workers with the lowest propensity are about 2 percent less likely to change occupation if they have changed occupation in the previous year. This number becomes 14 percent for workers who are most inclined to experience occupational mobility. Therefore, the more a worker is inclined to change occupation based on his/her unobserved fixed effect and other observables, the more important it is whether or not he/she changed occupation in the previous period.

The married dummy is statistically insignificant in all specifications. Other variables that might measure family considerations, such as the number of children in the household, children in the household dummy, home ownership and head of household dummy are all statistically insignificant (results not presented here).

The age of the individual has a different impact on the probability of an occupational change for different educational groups. For workers with only compulsory education, the *no degree* group, there is no statistically significant effect of age. This suggests that these workers with very low educational formation mostly perform tasks for which it does not matter how long they have been in the labor market. In contrast, for the other educational groups, namely *high school*, *high school with vocational training* and *college*, there is a statistically significant negative effect. Between these three educational groups there are differences. In all specifications, age has the most negative effect for high school graduates, then for workers having high school with vocational training and finally for college graduates. So, when workers have more than compulsory education, the impact of age on the probability of occupational change is declining in the level of education: although a higher age makes one less inclined to change occupation, this effect is smaller the higher one's education is. This result is not surprising although one may initially think that workers with high educational attainment do change occupations less often as they receive on average more occupation-specific formal education. Apparently, for aging workers with higher education, their formal background is adapting more easily to new technologies in new occupations so that mobility is relatively higher.

Figure 4 shows the impact of age on workers in different parts of the distribution for each educational group. This figure also uses the findings of Column (3). Clearly, for workers without any degree the effect of age is close to zero over the entire distribution. For other educational groups the order is preserved over the distribution, although there is divergence for the higher percentiles. Moreover, the age effect is becoming more negative. The more a worker is inclined to move, the bigger the impact of age and educational background.

In the second specification, the four origin-gender dummies interacted with regional unemployment rate are added. For all groups, an increase in the regional unemployment rate (measured in percentage points) has a statistically significant negative impact on the probability of occupational change (although the coefficient

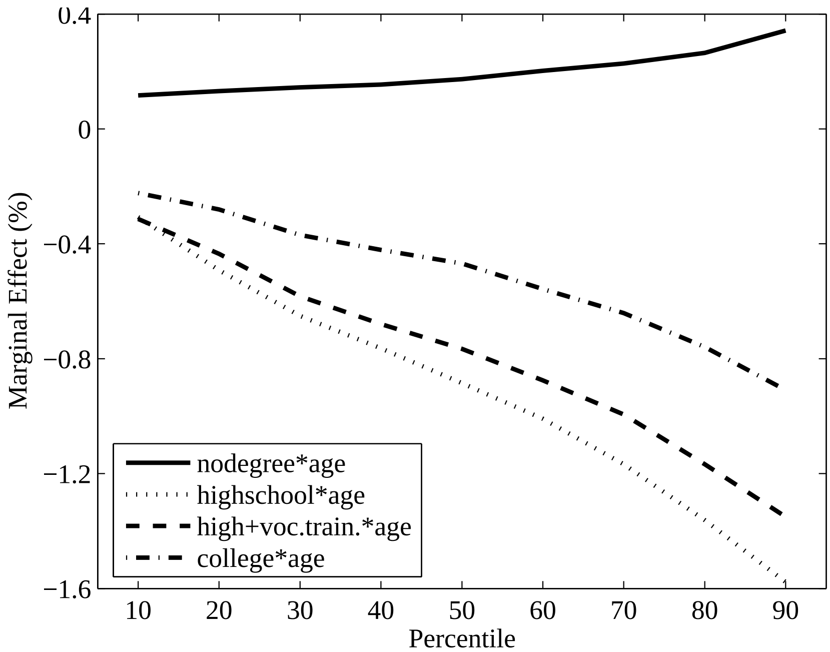

FIGURE 4. The marginal effects of age according to percentiles for the four educational groups.

for *native female*regional unemployment* is not always statistically significant). This is in line with expectations: the higher regional unemployment the lower the number of vacancies so the smaller the probability of changing occupations. The effects depend highly on ones origin and gender. Female foreigners are the group most affected by changes in regional unemployment rates. The average marginal effect is around -7 percent, whereas for the other groups it is around -2.1 to -1.3 percent. Inspection of the data shows that female foreigners are less educated. Although they have no specific reason to be committed to current occupations, their low formal skills may limit the tasks that they can undertake. As the results show, regional unemployment rates affect males less than females, and natives less than foreigners. Note also that the effect of gender depends on the origin and that likewise the effect of the origin depends on the gender. Employing only a gender and an origin dummy would not have captured these distinct effects.

In Figure 5 the effect of origin and gender is shown over the distribution of whole sample. For female foreigners, the effect of regional unemployment rates on the probability of changing occupation becomes more negative when a worker is more inclined to change occupation. This effect ranges from -1 percent to -12 percent depending on the characteristics of the worker. For the other three groups,

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

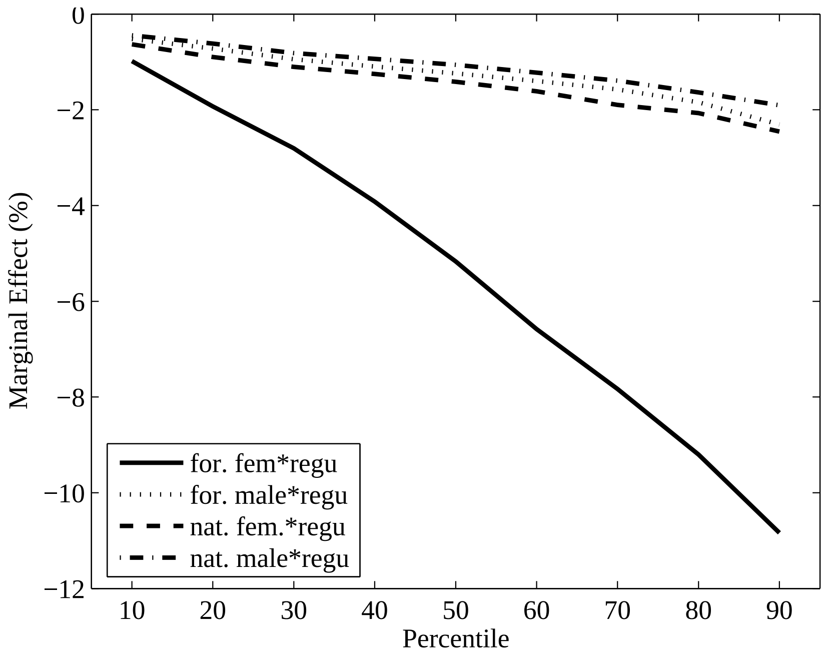

FIGURE 5. The marginal effects of regional unemployment according to percentiles for the four origin-gender groups.

the effect is also becoming more negative, albeit in a much less pronounced way, namely from -0.5 percent to -2 percent. Apart from female foreigners, the effect of regional unemployment rates does not depend on the individual's unobserved fixed effect and other characteristics.

Although suggested by a higher churning than the net reallocation in Figure 1, the additional turbulence in 1991 after the Reunification is not confirmed by the estimation results (dummies for the fall of the Berlin Wall in 1989 and the Reunification in 1990 were also statistically insignificant). However, note that the inclusion of the 1991 dummy mainly affects the origin-gender-regional unemployment interaction variables. For these variables both the coefficients and the marginal effects are less negative. Clearly, taking account of the higher turbulence in 1991 and the accompanying high growth rate reduces the impact of regional unemployment on occupational mobility.

The last column of Table 2 also includes the one-digit ISCO-88 dummies to see the impact of the occupational groups on mobility. The comparison group is *Legislators, Senior Managers and Officials*. Only the *Service Workers and Shop and Market Workers*, *Skilled Agricultural and Fishery Workers*, *Craft and Related Trades*, *Plant and Machine Operators and Assemblers* and *Elementary Occupations* are statistically significant. Although there is no clear ranking, these are the occupations of which the education level is likely to be lowest and most distant from the occupation level of *Legislators, Senior Managers and Officials*. The effect of these group dummies is positive, so, everything else being constant, workers belonging to these occupations have a higher probability of changing occupation. The size of the marginal effects shows that, compared to *Legislators, Senior Managers and Officials*, *Craft and Related Trades* are 17 percent more inclined to change occupation, whereas *Plant and Machine Operators and Assemblers* 18 percent, *Elementary Occupations* 22 percent, *Service Workers and Shop and Market Workers* 23 percent and *Skilled Agricultural and Fishery Workers* 53 percent. The statistically significant different impact can be explained by the intense occupation specific educational investment that workers in the comparison group *Legislators, Senior Managers and Officials* have undertaken which makes changes to other occupations much less likely.

The findings are robust to the inclusion of occupation dummies as results are not considerably affected. However given that these dummies may still be contaminated by some measurement error and because of potential bias stemming from unobserved time-variant worker heterogeneity, the specification presented in Column (3) is the preferred one. The presence of fixed effects can be tested with a likelihood ratio test. The null hypothesis of no fixed effects is rejected (probabilities of less than 1 percent).

The results discussed above are obtained after correcting for the incidental parameter bias. To see the size and the impact of the bias correction, Table 3 presents the results from the uncorrected dynamic fixed effects probit estimations. Comparing the results with and without bias correction reveals that there are only minor differences in terms of statistical significance and no changes of sign for statistically significant variables. In general, there are small differences in the size of the coefficients and marginal effects. The exception is the effect on the lagged occupational mobility dummy. For all the specifications, the uncorrected marginal effects are around -11 percent while the corrected marginal effects are around -8 percent only.

2.6.2. Robustness. Table 4 presents the results from pooled probit estimation. This model is the most appropriate choice if unobserved time-invariant individual heterogeneity is ignored. To have comparable results, the time-invariant variables

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

are also included in the pooled probit estimation. The first two columns of Tables 2 and 4 are related specifications for bias corrected fixed effects probit and pooled probit respectively. In Column (3) the statistically insignificant variables of origin-gender dummies interacted with regional unemployment rates are removed; in Column (4) the 1991 dummy is added. The latter specification is the preferred pooled probit specification as it is closest to the preferred specification of the fixed effects probit estimation.

The most striking difference between the pooled probit estimates and the bias corrected fixed effects probit estimates is the opposite sign of the lagged occupational mobility dummy. The coefficient changes from about -0.4 to 0.3 and the marginal effect from about -0.085 to 0.025 between these two estimation methods. A further analysis of the data and the implications of the fixed effects method clarifies this puzzling finding.

The data consists of 4,230 individuals for whom both the occupational mobility variable and its lag exist. In fixed effects probit estimation, individual fixed effects are not identified for individuals who change occupation in each period or for individuals who do not change occupation in any period. The sample used for the fixed effects probit estimation consists of 640 individuals. For the remainder of the chapter, this sample is referred to as the fixed effects sample and the sample with all workers as the pooled sample. Intuition for the opposite signs of the lagged dependent variable can be obtained by inspecting the different samples.

Table 5 shows how the distribution of current mobility depends on lagged mobility. The upper panel presents this effect for the pooled sample and the lower panel for the fixed effects sample. For example, in 8.3 percent of the cases when a worker changed occupation in the previous year, he/she is changing again in the current year according to the pooled sample. For this sample, workers who changed occupation in the previous year are more likely to change occupation in the current year compared to workers who did not change in the previous year (8.3 and 3.0 percent respectively). This explains the positive effect found in the pooled probit results for this sample. However, for the fixed effects sample the effect is reversed. Workers who changed occupation in the previous year are less likely to change occupation in the current year compared to workers who did not change in the previous year (9.9 and 15.6 percent respectively).

 DOI: 10.2870/13357

Therefore, change of the sign of the lagged occupational mobility variable is due to the different samples. Of the individuals who are in the pooled sample but not in the fixed effects sample, 99 percent never change occupation. As many observations with no current and no previous occupational mobility are eliminated, there are relatively more workers who have not changed in a given year but changed in the consecutive year. This explains the increase from 3.0 to 15.6 percent for this group when the pooled sample is reduced. Hence, a worker who did not change occupation in a given year is more likely to change in the subsequent year compared to someone who has changed in that given year. In economic terms, there is a considerable group of individuals who are inherent non-movers, i.e. individuals who never change occupation in the sample. Although their non-moving behavior reflects an important feature of the German labor markets, this group is not of help to understand the contribution of true state dependence, worker characteristics and macroeconomic changes.

When comparing the bias corrected fixed effects probit results and the pooled probit results, it is more appropriate to use the same sample, i.e. the fixed effects sample. These results are shown in Table 6. The impact of lagged mobility is now also negative and statistically significant. The coefficient is around -0.35 to -0.37, the marginal effect is around -7 percent. Although more in line with the bias corrected fixed effects probit estimates, the impact of lagged mobility is slightly lower.

It can be argued that the negative effect is largely due to the distribution of workers with respect to the years in the sample. Workers who are in the sample should have at least one occupational change, but many have only one occupational change. Relatively speaking, individuals with fewer observations in the sample have more occupational changes. One might wonder whether this is driving the results. Table 7 shows the distribution of workers according to number of years in the sample. The average period is 8.3 years. In Table 8 results are shown for the same specifications as for the bias corrected fixed effects probit, but for a sample in which workers exist at least six years. Although the marginal effect of lagged mobility becomes about -5 percent, the effect is still statistically significant. The implications for the coefficients and marginal effects of the other covariates is relatively minor.

To see the sensitivity of other covariates to the inclusion of the lagged dependent variable, Table 9 presents the bias corrected fixed effects probit estimates for the static model. The bias corrections are done according to Hahn and Newey (2004). There are slight changes in the size and significance levels of the coefficients and marginal effects, but no changes in signs. Including the lagged mobility dummy has no considerable effects on other coefficients. However, comparison of the loglikelihood values with their counterparts when the lagged dependent variable is included, shows that the dynamic model provides a better specification.

2.7. Conclusion

In this study, evolution and the determinants of occupational reallocation of workers in western Germany over the period 1985-2003 are analyzed using individual level data from the GSOEP. The occupational mobility is considered at the most disaggregated level of ISCO-88 which consists of 390 occupational units. Using this level of disaggregation implies that a moving worker changes career and relocates to a different technology.

Annual average occupational mobility is found to be strongly procyclical. The expansions and recessions of the German economy in the last two decades are accompanied by similar changes in aggregate occupational mobility levels. No trend can be observed in gross reallocation patterns. Net reallocation is found to be procyclical as well, though less pronounced. More interestingly, the turbulence in labor markets that followed Reunification is clearly observed in the patterns of gross and net reallocation as well as in churning.

To analyze the sources of gross reallocation, a dynamic fixed effect maximum likelihood estimation taking into consideration unobserved time-invariant worker heterogeneity is considered. The incidental parameter bias is addressed accordingly. There are important new findings. The marginal effect of the lagged dependent variable suggests that workers who change occupation in the current year are 8 to 9 percent less inclined to change occupation in the subsequent year compared to workers who do not change occupation in the current year. This is interesting since lagged occupational mobility favors current occupational mobility when unobserved time-invariant worker heterogeneity is ignored. When one also controls for the unobserved individual heterogeneity through a fixed effects procedure, workers with identical moving decisions in all periods are excluded. If the interest is the sources of occupational changes not driven by unobserved individual heterogeneity,

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

lagged occupational mobility makes a current occupational change less likely. A higher age, as expected, decreases the probability of an occupational change. For workers with more than compulsory education, the impact of age on the probability of occupational change is declining in the level of education, i.e. although a higher age makes one less inclined to change occupation, this effect is smaller the higher ones education is. An increase in the regional unemployment rate has a statistically significant negative impact on the probability of occupational change. This effect is very profound for female foreigners and small for the other origin-gender groups.

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Appendix 2.A. Tables

The tables on the following pages present the sample characteristics and estimation results.

variables	mean	sd
age	37.14	9.07
male	0.81	0.39
foreigners	0.27	0.44
married	0.70	0.46
no degree	0.04	0.19
high school	0.17	0.37
high school and vocational training	0.65	0.48
college	0.15	0.35
legislators, senior officials and managers	0.06	0.25
professionals	0.09	0.29
technicians and associate professionals	0.18	0.38
clerks	0.11	0.31
service workers and shop and market sales workers	0.03	0.17
skilled agricultural and fishery workers	0.001	0.02
craft and related trades workers	0.27	0.45
plant and machine operators and assemblers	0.19	0.39
elementary occupations	0.07	0.25
number of observations	5,331	
number of individuals	640	

Table 1. Sample means and standard deviations for the fixed effects sample.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
lagged mobility	-0.4084^{***}	-0.4268^{***}	-0.4279^{***}	-0.4144^{***}
	(0.0723)	(0.0726)	(0.0727)	(0.0737)
	[-0.0840^{***}]	[-0.0868^{***}]	[-0.0869^{***}]	[-0.0844^{***}]
	(0.0187)	(0.0201)	(0.0209)	(0.0246)
married	-0.0996	-0.0947	-0.0930	-0.0880
	(0.0919)	(0.0928)	(0.0928)	(0.0939)
	[-0.0243]	[-0.0230]	[-0.0226]	[-0.0213]
	(0.0190)	(0.0191)	(0.0191)	(0.0194)
no degree*age	0.0002	0.0111	0.0087	0.0199
	(0.0313)	(0.0324)	(0.0324)	(0.0335)
	[0.0000]	[0.0030]	[0.0023]	[0.0053]
	(0.0072)	(0.0098)	(0.0092)	(0.0122)
high school*age	-0.0525^{***}	-0.0421^{***}	-0.0433^{***}	-0.0394^{***}
	(0.0138)	(0.0143)	(0.0144)	(0.0146)
	[-0.0133^{***}]	[-0.0106^{***}]	[-0.0109^{***}]	[-0.0099^{***}]
	(0.0021)	(0.0017)	(0.0018)	(0.0021)
high school with vocational training*age	-0.0462^{***}	-0.0393^{***}	-0.0397^{***}	-0.0352^{***}
	(0.0074)	(0.0076)	(0.0077)	(0.0078)
	[-0.0112^{***}]	[-0.0095^{***}]	[-0.0096^{***}]	[-0.0085^{***}]
	(0.0010)	(0.0012)	(0.0014)	(0.0016)
college*age	-0.0346^{**}	-0.0274^{*}	-0.0273^{*}	-0.0228
	(0.0154)	(0.0157)	(0.0158)	(0.0159)
	[-0.0080^{***}]	[-0.0063^{***}]	[-0.0063^{***}]	[-0.0052^{***}]
	(0.0012)	(0.0009)	(0.0009)	(0.0011)
foreigner female*regional unemployment		-0.3319^{***}	-0.3129^{***}	-0.3243^{***}
		(0.1134)	(0.1117)	(0.1121)
		[-0.0717^{**}]	[-0.0675^{***}]	[-0.0701^{**}]
		(0.0281)	(0.0254)	(0.0289)
foreigner male*regional unemployment		-0.0799^{**}	-0.0666^{*}	-0.0639^{*}
		(0.0357)	(0.0364)	(0.0366)
		[-0.0188^{***}]	[-0.0156^{***}]	[-0.0149^{**}]
		(0.0060)	(0.0059)	(0.0064)
native female*regional unemployment		-0.0766^{*}	-0.0657	-0.0592
		(0.0450)	(0.0455)	(0.0465)
		[-0.0205^{***}]	[-0.0176^{***}]	[-0.0157^{**}]
		(0.0055)	(0.0060)	(0.0067)
native male*regional unemployment		-0.0669^{***}	-0.0558^{**}	-0.0616^{***}
		(0.0216)	(0.0227)	(0.0231)
		[-0.0161^{***}]	[-0.0134^{***}]	[-0.0148^{***}]
		(0.0041)	(0.0040)	(0.0046)
1991			0.1213	0.1213
			(0.0853)	(0.0857)
			[0.0310]	[0.0309]
			(0.0214)	(0.0218)
professionals				-0.0016
				(0.1983)
				[-0.0004]
				(0.0401)
technicians and associate professionals				0.1993
				(0.1678)
				[0.0514]
				(0.0447)
clerks				0.2410
				(0.1912)
				[0.0639]
				(0.0538)
service workers and shop and market sales workers				0.7123^{***}
				(0.2643)
				[0.2275^{**}]
				(0.1036)
skilled agricultural and fishery workers				1.4670^{*}
				(0.7572)
				[0.5361^{**}]
				(0.2729)
craft and related trades workers				0.6201^{***}
				(0.1967)
				[0.1701^{**}]
				(0.0742)
plant and machine operators and assemblers				0.6354^{***}
				(0.2103)
				[0.1823^{**}]
				(0.0802)
elementary occupations				0.7145^{***}
				(0.2283)
				[0.2240^{**}]
				(0.0952)
Loglikelihood	-1965.4	-1950.7	-1950.1	-1938.7
LR test fixed effects	3015.33^{***}	3001.34^{***}	3005.65^{***}	3024.13^{***}
	(χ^2_{639})	(χ^2_{639})	(χ^2_{639})	(χ^2_{639})

Robust standard errors in parentheses, marginal effects in square brackets.
*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 2. Fixed effects probit estimates with Hahn-Kuersteiner bias correction (in Column (4) the comparison group is *Legislators, Senior Managers and Officials*).

	(1)	(2)	(3)	(4)
lagged mobility	−0.7507***	−0.7711***	−0.7717***	−0.7567***
	(0.0809)	(0.0812)	(0.0812)	(0.0819)
	[−0.1152***]	[−0.1170***]	[−0.1170***]	[−0.1147***]
	(0.0298)	(0.0325)	(0.0337)	(0.0394)
married	−0.1072	−0.1037	−0.1020	−0.0985
	(0.1035)	(0.1043)	(0.1043)	(0.1050)
	[−0.0216]	[−0.0208]	[−0.0204]	[−0.0196]
	(0.0211)	(0.0211)	(0.0211)	(0.0213)
no degree*age	−0.0025	0.0093	0.0072	0.0181
	(0.0358)	(0.0370)	(0.0371)	(0.0378)
	[−0.0006]	[0.0021]	[0.0016]	[0.0040]
	(0.0074)	(0.0106)	(0.0101)	(0.0130)
high school*age	−0.0582***	−0.0465***	−0.0475***	−0.0435***
	(0.0157)	(0.0161)	(0.0161)	(0.0163)
	[−0.0122***]	[−0.0097***]	[−0.0099***]	[−0.0091***]
	(0.0029)	(0.0021)	(0.0023)	(0.0024)
high school with vocational training*age	−0.0506***	−0.0425***	−0.0428***	−0.0385***
	(0.0080)	(0.0082)	(0.0082)	(0.0083)
	[−0.0102***]	[−0.0085***]	[−0.0086***]	[−0.0077***]
	(0.0013)	(0.0014)	(0.0016)	(0.0018)
college*age	−0.0378**	−0.0297*	−0.0296*	−0.0256
	(0.0168)	(0.0169)	(0.0169)	(0.0171)
	[−0.0072***]	[−0.0056***]	[−0.0056***]	[−0.0048***]
	(0.0016)	(0.0011)	(0.0012)	(0.0012)
foreigner female*regional unemployment		−0.3870***	−0.3694***	−0.3788***
		(0.1159)	(0.1162)	(0.1160)
		[−0.0674**]	[−0.0643**]	[−0.0662*]
		(0.0342)	(0.0321)	(0.0353)
foreigner male*regional unemployment		−0.0911**	−0.0795*	−0.0760*
		(0.0400)	(0.0412)	(0.0417)
		[−0.0177***]	[−0.0155**]	[−0.0147**]
		(0.0067)	(0.0065)	(0.0071)
native female*regional unemployment		−0.0826*	−0.0730	−0.0669
		(0.0492)	(0.0497)	(0.0502)
		[−0.0183***]	[−0.0161**]	[−0.0147**]
		(0.0059)	(0.0063)	(0.0069)
native male*regional unemployment		−0.0759***	−0.0661**	−0.0713***
		(0.0243)	(0.0257)	(0.0259)
		[−0.0152***]	[−0.0132***]	[−0.0141***]
		(0.0045)	(0.0044)	(0.0050)
1991			0.1076	0.1094
			(0.0952)	(0.0956)
			[0.0225]	[0.0228]
			(0.0228)	(0.0231)
professionals				0.0332
				(0.1977)
				[0.0067]
				(0.0411)
technicians and associate professionals				0.1923
				(0.1636)
				[0.0405]
				(0.0424)
clerks				0.2361
				(0.1826)
				[0.0512]
				(0.0502)
service workers and shop and market sales workers				0.7044***
				(0.2463)
				[0.1819*]
				(0.0952)
skilled agricultural and fishery workers				1.5353
				(0.9356)
				[0.4677]
				(0.3226)
craft and related trades workers				0.6319***
				(0.1898)
				[0.1422**]
				(0.0719)
plant and machine operators and assemblers				0.6470***
				(0.2039)
				[0.1522*]
				(0.0777)
elementary occupations				0.7423***
				(0.2148)
				[0.1902**]
				(0.0910)
Loglikelihood	−1955.7	−1940.9	−1940.2	−1929.0

Standard errors in parentheses, marginal effects in square brackets.
*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 3. Fixed effects probit estimates without bias correction (in Column (4) the comparison group is *Legislators, Senior Managers and Officials*).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
lagged mobility	0.3205***	0.2992***	0.2998***	0.2973***
	(0.0598)	(0.0601)	(0.0601)	(0.0602)
	[0.0281*]	[0.0257]	[0.0257*]	[0.0255*]
	(0.0150)	(0.0171)	(0.0144)	(0.0143)
married	−0.0668*	−0.0656*	−0.0651*	−0.0661*
	(0.0353)	(0.0365)	(0.0365)	(0.0365)
	[−0.0046]	[−0.0045]	[−0.0044]	[−0.0045]
	(0.0033)	(0.0038)	(0.0034)	(0.0034)
high school	−0.5571*	−0.5751*	−0.5625*	−0.5655*
	(0.3183)	(0.3211)	(0.3192)	(0.3200)
	[−0.0470**]	[−0.0486*]	[−0.0472**]	[−0.0475**]
	(0.0206)	(0.0258)	(0.0213)	(0.0214)
high school with vocational training	−1.0333***	−1.0651***	−1.0523***	−1.0538***
	(0.2962)	(0.2998)	(0.2978)	(0.2987)
	[−0.1498***]	[−0.1564***]	[−0.1533***]	[−0.1535***]
	(0.0251)	(0.0486)	(0.0301)	(0.0303)
college	−0.6407*	−0.7064*	−0.6884*	−0.6873*
	(0.3664)	(0.3691)	(0.3675)	(0.3683)
	[−0.0616*]	[−0.0701*]	[−0.0677*]	[−0.0675*]
	(0.0325)	(0.0397)	(0.0347)	(0.0347)
age	−0.0510***	−0.0498***	−0.0494***	−0.0496***
	(0.0076)	(0.0077)	(0.0077)	(0.0077)
	[−0.0035***]	[−0.0034*]	[−0.0034***]	[−0.0034***]
	(0.0013)	(0.0018)	(0.0013)	(0.0013)
high school*age	0.0154*	0.0147*	0.0145*	0.0147*
	(0.0085)	(0.0086)	(0.0085)	(0.0086)
	[0.0010***]	[0.0010**]	[0.0010***]	[0.0010***]
	(0.0003)	(0.0005)	(0.0003)	(0.0003)
high school with vocational training*age	0.0281***	0.0262***	0.0259***	0.0261***
	(0.0079)	(0.0080)	(0.0079)	(0.0080)
	[0.0019***]	[0.0018**]	[0.0018***]	[0.0018***]
	(0.0005)	(0.0009)	(0.0005)	(0.0005)
college*age	0.0228**	0.0219**	0.0215**	0.0216**
	(0.0096)	(0.0097)	(0.0096)	(0.0096)
	[0.0020***]	[0.0019**]	[0.0019***]	[0.0019***]
	(0.0006)	(0.0009)	(0.0007)	(0.0007)
foreigner male		0.0716	0.1767**	0.1764**
		(0.2474)	(0.0790)	(0.0792)
		[0.0038]	[0.0089]	[0.0089]
		(0.0142)	(0.0066)	(0.0066)
native male		0.1238	0.3072***	0.3057***
		(0.2361)	(0.0784)	(0.0785)
		[0.0079]	[0.0174]	[0.0173]
		(0.0174)	(0.0108)	(0.0108)
native female		−0.1223	0.2258***	0.2235***
		(0.2586)	(0.0841)	(0.0842)
		[−0.0087]	[0.0136]	[0.0135]
		(0.0171)	(0.0093)	(0.0092)
regional unemployment		−0.0567*	−0.0308***	−0.0273***
		(0.0298)	(0.0064)	(0.0066)
		[−0.0039*]	[−0.0021*]	[−0.0019*]
		(0.0021)	(0.0011)	(0.0010)
foreigner male*regional unemployment		0.0153		
		(0.0327)		
		[0.0009]		
		(0.0017)		
native female*regional unemployment		0.0449		
		(0.0331)		
		[0.0034]		
		(0.0023)		
native male*regional unemployment		0.0257		
		(0.0310)		
		[0.0019]		
		(0.0019)		
1991				0.1225**
				(0.0588)
				[0.0091]
				(0.0069)
Loglikelihood	−3473.1	−3451.4	−3452.9	−3450.8

Standard errors in parentheses, marginal effects in square brackets.
*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 4. Pooled probit estimates for the pooled sample (the comparison groups are *no degree*, *no degree*age*, *foreigner female* and *foreigner female*regional unemployment*).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	Mob_t	
Mob_{t-1}	1	0
1	8.3	91.7
0	3.0	97.0

	Mob_t	
Mob_{t-1}	1	0
1	9.9	90.1
0	15.6	84.5

TABLE 5. The effect of lagged mobility on the distribution of current mobility for the pooled sample (upper panel) and the fixed effects sample (lower panel).

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	(1)	(2)	(3)	(4)
lagged mobility	-0.3493^{***}	-0.3647^{***}	-0.3637^{***}	-0.3695^{***}
	(0.0680)	(0.0684)	(0.0684)	(0.0685)
	[-0.0686^{**}]	[-0.0710]	[-0.0708^{**}]	[-0.0717^{**}]
	(0.0338)	(0.0451)	(0.0358)	(0.0365)
married	-0.0902^{*}	-0.0783	-0.0771	-0.0772
	(0.0483)	(0.0505)	(0.0505)	(0.0505)
	[-0.0203]	[-0.0176]	[-0.0173]	[-0.0173]
	(0.0138)	(0.0149)	(0.0136)	(0.0136)
high school	-0.4284	-0.4997	-0.4960	-0.5119
	(0.4630)	(0.4679)	(0.4661)	(0.4688)
	[-0.1144]	[-0.1348]	[-0.1340]	[-0.1384]
	(0.1041)	(0.1100)	(0.1046)	(0.1048)
high school with vocational training	-0.7371^{*}	-0.8430^{*}	-0.8235^{*}	-0.8321^{*}
	(0.4306)	(0.4365)	(0.4346)	(0.4376)
	[-0.2187^{**}]	[-0.2559^{**}]	[-0.2488^{**}]	[-0.2515^{**}]
	(0.0958)	(0.1099)	(0.0989)	(0.0994)
college	-0.5808	-0.7636	-0.7288	-0.7426
	(0.5215)	(0.5288)	(0.5266)	(0.5293)
	[-0.1509]	[-0.2058]	[-0.1951]	[-0.1988]
	(0.1175)	(0.1296)	(0.1215)	(0.1219)
age	-0.0340^{***}	-0.0349^{***}	-0.0342^{***}	-0.0348^{***}
	(0.0115)	(0.0116)	(0.0116)	(0.0117)
	[-0.0076^{***}]	[-0.0078^{***}]	[-0.0077^{***}]	[-0.0078^{***}]
	(0.0010)	(0.0029)	(0.0013)	(0.0014)
high school*age	0.0083	0.0101	0.0097	0.0105
	(0.0128)	(0.0129)	(0.0129)	(0.0130)
	[0.0019]	[0.0023]	[0.0023]	[0.0024]
	(0.0024)	(0.0024)	(0.0023)	(0.0023)
high school with vocational training*age	0.0161	0.0177	0.0170	0.0176
	(0.0118)	(0.0120)	(0.0120)	(0.0121)
	[0.0036^{***}]	[0.0040^{**}]	[0.0038^{***}]	[0.0039^{***}]
	(0.0013)	(0.0018)	(0.0014)	(0.0014)
college*age	0.0118	0.0159	0.0147	0.0154
	(0.0140)	(0.0142)	(0.0141)	(0.0142)
	[0.0025]	[0.0034]	[0.0031]	[0.0033]
	(0.0022)	(0.0023)	(0.0021)	(0.0021)
foreigner male		-0.3556	0.1040	0.1060
		(0.3744)	(0.1143)	(0.1147)
		[-0.0860]	[0.0216]	[0.0219]
		(0.0826)	(0.0263)	(0.0264)
native male		-0.2850	0.1824	0.1797
		(0.3591)	(0.1122)	(0.1126)
		[-0.0709]	[0.0370]	[0.0365]
		(0.0826)	(0.0294)	(0.0293)
native female		-0.2188	0.2394^{**}	0.2367^{*}
		(0.3935)	(0.1213)	(0.1217)
		[-0.0553]	[0.0539]	[0.0532]
		(0.0926)	(0.0368)	(0.0367)
regional unemployment		-0.0916^{*}	-0.0277^{***}	-0.0216^{**}
		(0.0477)	(0.0087)	(0.0090)
		[-0.0206^{**}]	[-0.0062^{**}]	[-0.0048^{*}]
		(0.0096)	(0.0031)	(0.0027)
foreigner male*regional unemployment		0.0659		
		(0.0512)		
		[0.0143]		
		(0.0092)		
native female*regional unemployment		0.0654		
		(0.0520)		
		[0.0164]		
		(0.0107)		
native male*regional unemployment		0.0666		
		(0.0490)		
		[0.0148^{*}]		
		(0.0088)		
1991				0.2167^{***}
				(0.0813)
				[0.0532^{*}]
				(0.0312)
Loglikelihood	-2175.4	-2167.7	-2168.6	-2165.2

Standard errors in parentheses, marginal effects in square brackets.
*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 6. Pooled probit estimates for the fixed effects sample (the comparison groups are *no degree*, *no degree*age*, *foreigner female* and *foreigner female*regional unemployment*).

 DOI: 10.2870/13357

years	observations	percent
2	59	9.2
3	54	8.4
4	58	9.1
5	53	8.3
6	53	8.3
7	52	8.1
8	45	7.0
9	43	6.7
10	30	4.7
11	32	5.0
12	25	3.9
13	18	2.8
14	23	3.6
15	15	2.3
16	14	2.2
17	20	3.1
18	46	7.2
total	640	100

Table 7. Distribution of the number of individuals for the years in the fixed effects sample.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
lagged mobility	-0.2423^{***}	-0.2663^{***}	-0.2689^{***}	-0.2493^{***}
	(0.0809)	(0.0816)	(0.0818)	(0.0829)
	$[-0.0466^{***}]$	$[-0.0504^{***}]$	$[-0.0508^{***}]$	$[-0.0474^{**}]$
	(0.0152)	(0.0160)	(0.0166)	(0.0188)
married	-0.1443	-0.1406	-0.1403	-0.1542
	(0.0988)	(0.0998)	(0.0999)	(0.1015)
	$[-0.0312^{*}]$	$[-0.0302]$	$[-0.0301]$	$[-0.0330]$
	(0.0189)	(0.0191)	(0.0192)	(0.0206)
no degree*age	-0.0196	-0.0100	-0.0126	-0.0025
	(0.0319)	(0.0331)	(0.0331)	(0.0344)
	$[-0.0048^{***}]$	$[-0.0024]$	$[-0.0030]$	$[-0.0006]$
	(0.0016)	(0.0042)	(0.0035)	(0.0063)
high school*age	-0.0574^{***}	-0.0474^{***}	-0.0488^{***}	-0.0459^{***}
	(0.0145)	(0.0150)	(0.0151)	(0.0153)
	$[-0.0127^{***}]$	$[-0.0104^{***}]$	$[-0.0107^{***}]$	$[-0.0100^{***}]$
	(0.0034)	(0.0026)	(0.0028)	(0.0031)
high school with vocational training*age	-0.0418^{***}	-0.0352^{***}	-0.0356^{***}	-0.0315^{***}
	(0.0076)	(0.0078)	(0.0079)	(0.0080)
	$[-0.0091^{***}]$	$[-0.0076^{***}]$	$[-0.0077^{***}]$	$[-0.0068^{***}]$
	(0.0011)	(0.0012)	(0.0014)	(0.0016)
college*age	-0.0330^{**}	-0.0262^{*}	-0.0262	-0.0207
	(0.0156)	(0.0159)	(0.0159)	(0.0161)
	$[-0.0066^{***}]$	$[-0.0052^{***}]$	$[-0.0052^{***}]$	$[-0.0041^{***}]$
	(0.0016)	(0.0011)	(0.0011)	(0.0011)
foreigner female*regional unemployment		-0.2898^{***}	-0.2708^{**}	-0.2842^{***}
		(0.1092)	(0.1074)	(0.1083)
		$[-0.0545^{**}]$	$[-0.0509^{**}]$	$[-0.0535^{*}]$
		(0.0269)	(0.0240)	(0.0282)
foreigner male*regional unemployment		-0.0641^{*}	-0.0503	-0.0487
		(0.0371)	(0.0379)	(0.0381)
		$[-0.0136^{**}]$	$[-0.0107^{*}]$	$[-0.0103^{*}]$
		(0.0057)	(0.0057)	(0.0062)
native female*regional unemployment		-0.0768	-0.0651	-0.0619
		(0.0514)	(0.0517)	(0.0531)
		$[-0.0171^{***}]$	$[-0.0145^{***}]$	$[-0.0137^{**}]$
		(0.0046)	(0.0050)	(0.0058)
native male*regional unemployment		-0.0654^{***}	-0.0534^{**}	-0.0586^{**}
		(0.0227)	(0.0240)	(0.0245)
		$[-0.0141^{***}]$	$[-0.0115^{***}]$	$[-0.0126^{***}]$
		(0.0040)	(0.0039)	(0.0047)
1991			0.1277	0.1291
			(0.0933)	(0.0937)
			[0.0292]	[0.0294]
			(0.0221)	(0.0227)
professionals				0.0015
				(0.2204)
				[0.0003]
				(0.0405)
technicians and associate professionals				0.2135
				(0.1869)
				[0.0494]
				(0.0479)
clerks				0.2826
				(0.2145)
				[0.0685]
				(0.0605)
service workers and shop and market sales workers				0.7248^{**}
				(0.3087)
				$[0.2161^{*}]$
				(0.1196)
skilled agricultural and fishery workers				-2.7790^{***}
				(0.7297)
				$[-0.1309^{**}]$
				(0.0589)
craft and related trades workers				0.6312^{***}
				(0.2202)
				$[0.1575^{*}]$
				(0.0826)
plant and machine operators and assemblers				0.6050^{**}
				(0.2389)
				$[0.1575^{*}]$
				(0.0880)
elementary occupations				0.6223^{**}
				(0.2603)
				$[0.1753^{*}]$
				(0.1016)
Loglikelihood	-1537.6	-1526.2	-1525.5	-1517.1
LR test fixed effects	3868.87^{***}	3891.65^{***}	3893.05^{***}	3909.86^{***}
	(χ^2_{415})	(χ^2_{415})	(χ^2_{415})	(χ^2_{415})

Robust standard errors in parentheses, marginal effects in square brackets.
*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 8. Fixed effects probit estimates with Hahn-Kuersteiner bias correction for a sample with minimum 6 periods of observations per individual (in Column (4) the comparison group is *Legislators, Senior Managers and Officials*).

	(1)	(2)	(3)	(4)
lagged mobility				
married	−0.0962	−0.0943	−0.0932	−0.0903
	(0.1022)	(0.1029)	(0.1029)	(0.1038)
	[−0.0230]	[−0.0224]	[−0.0221]	[−0.0213]
	(0.0213)	(0.0214)	(0.0214)	(0.0216)
no degree*age	0.0018	0.0105	0.0091	0.0193
	(0.0359)	(0.0370)	(0.0371)	(0.0378)
	[0.0005]	[0.0027]	[0.0024]	[0.0050]
	(0.0087)	(0.0111)	(0.0108)	(0.0137)
high school*age	−0.0480***	−0.0392**	−0.0400**	−0.0360**
	(0.0157)	(0.0161)	(0.0161)	(0.0163)
	[−0.0120***]	[−0.0097***]	[−0.0099***]	[−0.0089***]
	(0.0020)	(0.0017)	(0.0018)	(0.0020)
high school with vocational training*age	−0.0415***	−0.0351***	−0.0354***	−0.0309***
	(0.0079)	(0.0082)	(0.0082)	(0.0083)
	[−0.0099***]	[−0.0083***]	[−0.0084***]	[−0.0073***]
	(0.0009)	(0.0012)	(0.0013)	(0.0015)
college*age	−0.0298*	−0.0238	−0.0237	−0.0189
	(0.0168)	(0.0169)	(0.0169)	(0.0170)
	[−0.0067***]	[−0.0053***]	[−0.0053***]	[−0.0042***]
	(0.0007)	(0.0009)	(0.0010)	(0.0013)
foreigner female*regional unemployment		−0.2982***	−0.2840**	−0.2945**
		(0.1144)	(0.1147)	(0.1146)
		[−0.0630**]	[−0.0599**]	[−0.0622**]
		(0.0260)	(0.0241)	(0.0272)
foreigner male*regional unemployment		−0.0680*	−0.0587	−0.0567
		(0.0398)	(0.0409)	(0.0415)
		[−0.0155**]	[−0.0134**]	[−0.0129*]
		(0.0065)	(0.0065)	(0.0069)
native female*regional unemployment		−0.0604	−0.0528	−0.0492
		(0.0485)	(0.0491)	(0.0494)
		[−0.0162**]	[−0.0141*]	[−0.0131*]
		(0.0069)	(0.0074)	(0.0078)
native male*regional unemployment		−0.0639***	−0.0564**	−0.0623**
		(0.0240)	(0.0254)	(0.0256)
		[−0.0150***]	[−0.0133***]	[−0.0146***]
		(0.0044)	(0.0044)	(0.0049)
1991			0.0825	0.0843
			(0.0934)	(0.0939)
			[0.0204]	[0.0207]
			(0.0222)	(0.0224)
professionals				−0.0008
				(0.1906)
				[−0.0002]
				(0.0391)
technicians and associate professionals				0.2076
				(0.1581)
				[0.0527]
				(0.0434)
clerks				0.2740
				(0.1763)
				[0.0724]
				(0.0528)
service workers and shop and market sales workers				0.6962***
				(0.2383)
				[0.2219**]
				(0.0955)
skilled agricultural and fishery workers				0.9561
				(0.8953)
				[0.3330]
				(0.3251)
craft and related trades workers				0.6309***
				(0.1838)
				[0.1723**]
				(0.0726)
plant and machine operators and assemblers				0.6195***
				(0.1975)
				[0.1766**]
				(0.0771)
elementary occupations				0.6835***
				(0.2087)
				[0.2127**]
				(0.0890)
Loglikelihood	−2006.1	−1994.0	−1993.4	−1979.2
LR test fixed effects	366.54***	377.82***	380.51***	403.04***
	(χ^2_{639})	(χ^2_{639})	(χ^2_{639})	(χ^2_{639})

Standard errors in parentheses, marginal effects in square brackets.
*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 9. Static fixed effects probit estimates with Hahn-Newey bias correction (in Column (4) the comparison group is *Legislators, Senior Managers and Officials*).

DOI: 10.2870/13357

 DOI: 10.2870/13357

CHAPTER 3

Part-Time Work and Wages of Women in Germany

Abstract. This chapter studies the hourly wage differential between part-time and full-time working women in western Germany for the period 1996-2004. Economic theory provides arguments supportive of either a part-time wage penalty or premium and empirical evidence regarding German labor markets is diverse. The estimation method applied in this study not only accounts for selection into part-time employment but also for unobserved time-invariant worker heterogeneity. Moreover, it allows for an analysis of the dynamics regarding part-time employment. Based on data from the German Socio-Economic Panel, no evidence of a part-time wage differential is found.

JEL classification. C33, J22, J31.

Keywords. Panel Data, Part-Time Wage Penalty/Premium, Two-Step Estimation, Worker Heterogeneity.

3.1. Introduction

A high part-time employment rate among working women has become a prominent feature of German labor markets. According to the German Federal Statistical Office, 42 percent of female employees were working part-time in 2004, up from 34 percent in 1996.[1] Economic opportunities of many women in Germany are thus highly affected by the levels of pay, the types of jobs and the conditions that are available on a part-time basis. Most importantly, it is of interest whether a part-time worker receives the same hourly wage as what she would have been receiving while working full-time.

The German Parliament, recognizing the importance of part-time employment and its implications for job creation and flexible organization of work, passed a new Act on part-time work which came into force on 1 January 2001. The legal principle of equal treatment with respect to pay and all other kinds of benefits of full-time and part-time workers had existed since 1985. Apart from aligning this legislation to EU Directives, the new Act entitles an employee, even at the managerial level, to reduce his/her working time provided no internal company reasons prevent such a reduction. Although legislative regulations prohibit discrimination against employees working less than regular working hours, given the large fraction of women affected by the conditions of part-time work it is important to verify whether part-time workers are treated equally with respect to hourly wages.

This study investigates the determinants of the part-time/full-time employment decision and the potential part-time hourly wage differential using data from the German Socio-Economic Panel (GSOEP). The few existing studies on part-time wage differentials in Germany obtain contrasting results. Bardasi and Gornick (2000) find a wage penalty for part-time workers in Germany, whereas Manning and Petrongolo (2004) find a wage premium and Wolf (2002) finds no pay differential for women who work part-time for more than 20 hours a week.[2] These diverse findings reflect differences in the measure of part-time employment status, the employed data set and the way both unobserved time-invariant worker heterogeneity and selection into part-time employment are addressed. Among the mentioned studies, only Wolf (2002) focuses exclusively on German labor markets. However, the

[1] Part-time employment among men is considerably lower with about 6 percent in 2004.

[2] These studies and their findings are discussed in detail in Section 3.2.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

objective of that study is to estimate a simultaneous model of wages, working hours and the selection into employment. The findings suggest that hourly earnings of part-timers working less than 20 hours per week are lower than those of part-timers working more hours, but that no wage differential exists between the latter and full-timers. Since this study only uses the 1995 wave of the GSOEP, it does not permit to control for unobserved time-invariant worker heterogeneity. Clearly, both the scarcity and the contradicting findings (premium, penalty or no effect) of the existing studies show the need for further research.

This study adds to the literature on wage differentials between part-time and full-time working women in western Germany by using a method to rigorously deal with the methodological problems inherent to the topic. These problems arise since typically the decision to work part-time and the wage are affected by the same factors. When not all these factors are observable, the estimates are biased due to endogeneity. Ideally, one would like to address the two sources of this potential endogeneity, namely unobservable time-variant and time-invariant heterogeneity, simultaneously.

Unobservable time-invariant worker characteristics that play a role in the part-time employment decision, can also have an impact on the worker's wage. Without accounting for unobservable time-invariant worker heterogeneity in both estimations, the part-time employment decision would be correlated with the error term of the wage equation. Hence, when simply including a part-time dummy in the wage estimation, the impact of part-time employment on wage, the focus of interest, would not be estimated correctly via OLS.

However, even when the estimation accounts for unobserved time-invariant worker heterogeneity, the endogeneity problem continues to affect the results as individual specific unobservable and time-variant characteristics might affect both the part-time employment decision and the wages. Ignoring unobserved time-variant individual heterogeneity might thus lead to biased estimates.

A two-step estimation method is employed in this study to control for both sources of endogeneity. In the first step the part-time/full-time employment decision is estimated via a fixed effects procedure. A control function is then constructed based on these results. Accordingly, the wage equation is estimated by fixed effects OLS using the control function as an additional covariate. Employing fixed

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

effects procedures in both steps account for the unobserved time-invariant worker heterogeneity. The time-varying unobserved worker heterogeneity is addressed by the inclusion of the control function in the wage equation. Although accounting for fixed effects is straightforward when estimating the wage equation with OLS, this is not the case for the nonlinear part-time employment equation due to the resulting incidental parameter bias. Moreover, when the fixed effects in both equations are correlated, also the wage equation is affected by the incidental parameter bias through the included control function. A method proposed in Fernandez-Val and Vella (2007) is used in this study to address the incidental parameter bias in both steps.

An additional advantage of the applied method is that it allows to analyze the dynamic effects of the part-time employment decision. This is relevant since it is likely to observe persistence in the employment status. The two possible explanations of this observed persistence, namely true state dependence and unobserved worker heterogeneity, can be isolated with the employed estimation method.

In addition to widely used covariates reflecting human capital and household related controls, occupational dummies are also included in the analysis. Recently, Manning and Petrongolo (2004) report that a 10 percent part-time wage penalty for the UK after controlling for worker heterogeneity, decreases to only 3 percent once the occupation of the worker is also taken into account. This observation suggests that the penalty is arising due to sorting of part-time workers into specific occupations. Also, O'Dorchai, Plasman and Rycx (2007) stress the importance of including occupation and sector related information in their cross-country analysis. A similar effect can be expected for Germany. The first chapter of this dissertation, though, shows that seizable measurement errors exist regarding occupational affiliations in the German Socio-Economic Panel. Accordingly, here the 'corrected' occupational affiliations are used.

The plan of this chapter is as follows. In the next section both the theoretical and empirical literature on part-time/full-time wage differentials are summarized. In Section 3.3 the model and estimation method are discussed. In Section 3.4 the data and sample are described in detail. The estimation results are presented in Section 3.5. Tables are in the Appendix.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

3.2. The Part-time Wage Differential in the Literature

3.2.1. Theoretical Arguments for a Part-time Wage Penalty or Premium. Economic theory suggests several reasons for the existence of an hourly wage differential between part-time and full-time workers with similar skills. Some of these theories argue that part-time work is associated with a wage penalty, while others suggest it might endow a wage premium. The reasons for an hourly wage differential can be summarized under five groups which overlap and support each other.

Firstly, labor supply and demand interactions can be a cause of hourly wage gaps. Some individuals may prefer to work part-time instead of full-time, such as young people during their studies, older people at the end of their career and people with time consuming household related responsibilities. A preference for part-time work cannot be a reason for a part-time wage differential on its own. However, a worker's preference for part-time work might result in a lower hourly wage due to its effect on the worker's human capital accumulation. Human capital theory suggests that individuals who expect to work part-time in the future invest less in formal education compared to those who aim to work full-time. Moreover, part-time workers accumulate tenure and labor market experience at a lower rate. Empirical studies indeed show that compared to full-time employment spells, lower returns to tenure and labor market experience are accumulated during part-time employment spells (see e.g. Swaffield (2000), Manning and Robinson (2004) and Hirsch (2005)).

In addition, a worker's preference for part-time work can also result in a lower hourly wage when the employer's preferences regarding the schedule of working hours differs from the worker's preferences. For example, due to the nature of the work, the employer might have strong preferences for certain working schedules and might want to remunerate different time slots accordingly. When part-time workers are restricted in choosing their working hours, like women with children, and can only work during times relatively unattractive for the employer, their hourly wage may be lower than if they would have been working full-time.

Moreover, Ermisch and Wright (1993) stress the importance of geographical mobility for explaining the part-time wage penalty. The same reasons that drive individuals to work part-time might also make them wanting to work close to home.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Since they then have fewer job options, employers have more bargaining power to adjust part-time wages downwards.

On the other hand, supply-demand factors can also cause a part-time wage premium. For instance, if employers have seasonal or fluctuating demand for labor, such as in the tourism sector or in restaurants with mealtime peaks, their optimal working schedules may require employing part-time workers. If in general employees prefer full-time work, it can be difficult to find part-time workers with a part-time wage premium as a result.

Secondly, the fixed costs of employment incurred by firms can explain the wage gap. Employers face several employee related fixed costs concerning hiring, firing, administration, while also the provision of fringe benefits, such as health insurance, can be independent of hours worked. Clearly, the cost per worker is relatively higher for part-time workers, which explains why part-time workers may receive lower wages (see e.g. Oi (1962) and Montgomery (1988)). A similar reasoning is related to the training costs. Lindbeck and Snower (2000) argue that following the fundamental changes in production technologies in the past decade, such as computerized information and communications systems, especially firms in advanced industrialized countries reorganize themselves more frequently. These reorganizations promote continuous learning and employees' direct involvement in decision making. The increased emphasis on individual responsibilities requires training, which comes at a cost for the firm. Although the training costs may differ depending on the occupation and the position of the worker, it is unlikely that they differ based on the hours worked by the employee. Hence, training costs are relatively higher for part-time workers.

Thirdly, various theories associate the number of hours worked with worker productivity. Considering that the hourly wage rate should be equal or at least related to marginal productivity of labor, wages may respond to changes in the number of hours worked. Barzel (1973) argues that due to 'start-up' effects, worker's productivity rises slowly at the beginning of a working day, then increases more rapidly and finally levels off at the end of the working day. Therefore, the productivity of the last hour of a 'normal' day exceeds average productivity. So, although full-time workers are not inherently more able than part-time workers, they receive a higher hourly wage because their average hourly productivity during the day is higher.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Opposite to this finding, Moffitt (1984) and Tummers and Woittiez (1991) present evidence that productivity arguments imply a wage premium for part-time workers. They have shown that the negative 'fatigue' effect which causes marginal productivity to drop in case of long working hours or overtime is avoided by part-time work. Furthermore, less hours may reduce unproductive time. As the peak of the average productivity is found to be at 34 working hours a week, part-time workers can have a higher productivity and will then be paid accordingly. On top of this, Scandura and Lankau (1997) and Baltes, Briggs, Huff, Wright and Neuman (1999) argue that flexible part-time work schedules decrease absenteeism while increasing work performance and commitment to the job.

A fourth group of theories is related to the prevailing institutional settings. The tax structure, for example, has large implications on the pay that the worker takes home. In Germany, for instance, low-paid jobs with few hours, the so-called 'marginal jobs', do not have social security coverage and are taxed by a lump sum tax fully paid by the employer (15 percent of the gross wage rate in 1995). Wolf (2002) concludes on the basis of an empirical analysis by Schwarze (1998) that employers shift the entire tax burden onto the marginal employees, resulting in a wage penalty of nearly 15 percent compared to full-time employees. Furthermore, a progressive tax structure increases the net hourly wage of part-time workers relative to full-time workers. Hence, part-time workers might accept lower gross hourly wages when the net wages are comparable. Other relevant institutional factors include anti-discrimination legislation and lower rates of union membership among part-time workers.

Finally, a part-time wage differential can arise from the existence of dual labor markets. This theory argues that there is a primary labor market for 'good' jobs and a secondary labor market for 'bad' jobs. Full-time jobs are usually good jobs with high wages and generous fringe benefits. Part-time jobs on the other hand are mostly bad jobs with a lower reward, causing a part-time wage penalty. Lettau (1994) finds that compensation per hour is substantially lower for part-time jobs than for full-time jobs, even for jobs within the same firm and occupation.

As this summary above shows, economic theory does not provide a clear prediction about the sign and size of the part-time/full-time wage differential. Since

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

many factors are at play, empirical findings for labor markets of different countries are expected to vary.

3.2.2. Empirical Findings in the Literature. There are several economic studies focusing on the wage differential between part-time and full-time female workers. This subsection presents an overview of various studies covering different countries. Apart from documenting the found wage differentials for several labor markets, the impact of the assumptions underlying the estimation method is also discussed.

Blank (1990) investigates the wage differentials for part-time workers in the US using data from the March 1988 Current Population Survey(CPS). The results from a simple regression suggest a wage penalty of 21 percent for female part-time workers. To account for the unobserved worker heterogeneity, estimation using instrumental variable approach as well as estimation controlling for selection bias are carried out. The instrumental variable approach suggests an extraordinary part-time wage penalty of 62 percent. On the other hand, controlling for selection into employment and part-time/full-time work suggests a wage premium of 17 percent. Due to the widely varying results of different estimation methods, the author refrains from drawing firm conclusions and suggests that unmeasured worker and job heterogeneity are important in determining wage differentials.

More recently, Hirsch (2005) analyzes wage differentials also using CPS data from 1995 till 2002. The definition of part-time employment status is based on hours of work. Apart from the standard covariates, the used data set allows to assess the impact of occupational skill requirements and working conditions which makes it possible to analyze the wage difference between similarly skilled workers. Reduced-form wage equations are estimated with a part-time employment dummy though not accounting for selection into part-time employment. The longitudinal dimension of the data allows to include lagged part-time employment status to capture acquired human capital and other unobserved worker characteristics to some extent. The data suggest an unconditional part-time wage penalty of 20 percent for women. When accounting for observable worker characteristics and industry and occupation dummies, the penalty decreases to 10 percent. Including occupational

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

skill dummies reduce the wage penalty further to a mere 4 percent. When the longitudinal structure is exploited to partially address unobserved worker heterogeneity, little if any evidence of a part-time wage penalty is found.

Ermisch and Wright (1993) use data from the 1980 Women and Employment Survey to assess the wage differences for part-time working women in the UK. Part-time employment is based on the self-assessment of the worker regarding her job. The analysis includes the decision whether or not to work as well as the decision to work part-time or full-time. An unconditional wage penalty of 15 percent is found. After controlling for differences in observable worker characteristics such as education and work experience and also controlling for selection into part-time employment, the wage penalty is reduced to 2-8 percent.

Manning and Petrongolo (2004) also study wage differences for women in the UK. The analysis is based on data from the Labour Force Survey for the period 2001-2003. Different estimation methods are employed (including methods accounting for sample selection). For comparison, both the self-assessment measure and the hour-based measure of part-time employment are used. The results are found to be very similar for both measures and across estimation methods. The unconditional part-time wage penalty is 25 percent. Accounting for observable worker characteristics reduces this gap to 10 percent. The authors stress the importance of including the worker's occupation as a covariate, which reduces the part-time wage penalty to around 3 percent only.

In Norway with its strict rules against discrimination between full-time and part-time work, Hardoy and Schøne (2006) find no statistically significant difference in hourly wages between part-time and full-time working women. The data comes from the Level of Living Surveys for the period 1997-1998. The definition of part-time is based on the respondents declaration of weekly working hours. Cut-off points for part-time employment at 25 hours and 32 hours are considered. Using an endogenous switching regressions model, the part-time employment decision is estimated first. The results are used to control for selection into part-time employment in the two separate wage equations estimated for the two subsamples of part-timers and full-timers. Finally, the difference in estimated wages is included as a covariate in the part-time employment equation to estimate the impact of wage

 DOI: 10.2870/13357

differentials on a worker's choice for part-time work. Their findings suggest an unconditional wage penalty of approximately 5 percent. However, after controlling for observed worker characteristics and non-random selection into part-time employment the wage penalty disappears, even though the results do not provide evidence of selection. As expected after the finding of a non-significant wage differential, the difference in payments between part-time and full-time jobs does not have an impact on the decision whether to work part-time or not.

For the Netherlands, Russo and Hassink (2005) investigate the part-time wage differential with a focus on the possible negative effects of part-time work on a worker's career. Since human capital accumulation is slower in part-time employment, the incidence of promotion among workers in part-time employment is expected to be lower. This suggests that for young workers who recently entered the labor market, no part-time wage penalty should exist. The study uses employer-employee matched data covering the years 1997-2000. Conditional upon labor force participation three employment states are distinguished: short part-time (less than 20 hours per week), part-time (between 20 and 36 hours per week), and full-time (36 hours or more per week). Findings suggest a part-time wage penalty for women depending on the type of the part-time work. Women in short part-time jobs suffer a wage penalty of about 2.4 percent relative to comparable full-time employees, whereas this number decreases to 1.7 percent for women in part-time employment. Using lagged part-time status as a proxy for unobserved individual characteristics, both found part-time wage penalties disappear and no statistically significant effect of part-time on wage is found. For young workers no wage penalty is found even without including the lagged part-time dummy in the analysis.

To estimate part-time wage differentials for Australia, Booth and Wood (2008) use panel data for the period 2001-2004 from the Household, Income and Labour Dynamics in Australia Survey. Part-time employment status is based on the individual's hours of work, where workers reporting less than 35 hours of work per week are considered as part-timers. When only observable characteristics are taken into account, no evidence of a wage differential is found for women. However, once unobserved time-invariant worker heterogeneity is taken into account through a fixed effects estimation, a wage premium of 10 percent is found. This premium is higher

DOI: 10.2870/13357

for casual workers, i.e. for workers who are not eligible for sick and holiday pay entitlements and as a compensating differential they are paid a wage premium. Vella (1993) estimates a simultaneous model of wages and hours for young Australian women between ages 15 and 26 using the 1985 panel of the Australian Longitudinal Survey. The method employed accounts for the sample selection and endogeneity of hours. The findings suggest that although the hourly gross and net wage rates are indeed decreasing in hours, worker's total hourly remuneration including both the gross wage and fringe benefits, is constant.

Few studies perform a cross-country analysis. Bardasi and Gornick (2000) analyze the wage differential of part-time employed women across Canada, Germany, Italy, the UK and the US using data from the Luxembourg Income Study for 1994 or 1995 depending on the availability. The part-time employment definition is based on the self-assessment of the worker. The unconditional wage penalties are found to be around 8 percent in Germany, 12 percent in Canada, 15 percent in the UK and 22 percent in the US and Italy. In the empirical analysis, the first stage focuses on whether to work or not and if working whether to work part-time or full-time. In the second stage, the wage gap between part-time and full-time workers is analyzed and an Oaxaca decomposition of the wage differential is performed. For the UK, 90 percent of the unconditional wage penalty is accounted for by observed individual characteristics. For the US, Italy and Canada selection into part-time employment seems to drive the observed unconditional wage penalties. Only in Germany, both the observables and the selection into part-time have a relatively minor role in explaining the wage penalty reducing it from 8.4 percent to 7.7 percent. The authors suggest two reasons for this pay differential. Firstly, the pay penalty in Germany may reflect 'discrimination' against part-time workers. Alternatively, unobservable factors such as aptitude and motivation may drive pay differences. In their study they cannot differentiate between these two explanations.

Hu and Tijdens (2003) analyze and compare the part-time wage differentials in the Netherlands and the UK for men and women using data from the European Community Household Panel for the year 1998. Two types of part-time employment are distinguished: depending on whether an employee works between 12 and 21 or between 22 and 29 hours per week he/she is considered to be a short or a long part-timer. A worker is treated as a full-timer if he/she works 30 hours or more per week.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Hu and Tijdens (2003) allow for sample selection using a two-step method. However as three categories of employment status are defined, i.e. short part-time, long part-time and full-time, an ordered probit model is estimated. For the Netherlands, the wage gap between full-time and short part-time employees is 10 percent, while 27 percent in the UK. Regarding long part-time workers, the numbers are 3 percent for the Netherlands and 26 percent for the UK. While for the UK a wage penalty is found for both types of part-time workers, in the Netherlands only those who are in short part-time employment have a statistically significant lower hourly wage than comparable full-time workers.

3.3. A Model of Wage Determination and Part-Time Employment

The previous section showed the importance of the estimation method when analyzing the impact of part-time employment on wages. Considerably different results are obtained depending on whether unobserved time-invariant worker heterogeneity and/or selection into part-time employment is controlled for. Observable worker characteristics explain a part of the unconditional wage gap. The potential endogeneity of the part-time employment status can be addressed by controlling for unobservable worker heterogeneity. This further reduces, or even eliminates, the wage gap between part-time and full-time workers. The unobserved worker heterogeneity can be time-variant as well as time-invariant. A two-step method with fixed effects addressing both sources of endogeneity is used in this study. In the first step, the reduced form of the time-variant worker heterogeneity is estimated including fixed effects. The wage equation is then estimated including both the constructed control function and fixed effects. Part-time employment is thus treated as a binary choice outcome and is considered to be endogenous to wages. As selection into employment is not taken into account, results should be interpreted as conditional on being employed.

Consider the following empirical model:

$$PT_{it} = \mathbb{1}[PT_{it-1}\beta_1 + x_{it}^{'}\beta_2 + z_{it}^{'}\beta_3 + \alpha_{1i} + \epsilon_{1it} > 0], \tag{1}$$

$$wage_{it} = PT_{it}\theta_1 + x_{it}^{'}\theta_2 + \alpha_{2i} + \epsilon_{2it}, \tag{2}$$

$$i = 1, \ldots, n, \; t = 1, \ldots, T,$$

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

where $\mathbb{1}[.]$ is an indicator function equal to one if its argument is true, and zero otherwise. PT_{it} is 1 if individual i is working part-time in period t and 0 otherwise. PT_{it-1} is the lagged part-time employment status and $wage_{it}$ is the log of the individual's hourly wage. Covariates that appear in the conditional mean of both equations are denoted by x_{it}. PT_{it-1} and z_{it} are covariates that appear only in the part-time equation. Therefore, the parameter identification does not rely on distributional assumptions of the first step when $\beta_1 \neq 0$ and/or $\beta_3 \neq 0$. Both the part-time employment and the wage equation have an additive unobserved time-invariant individual component, α_{1i} and α_{2i} respectively. To allow these components to be correlated with the covariates and each other a fixed effects approach is followed.

Fernandez-Val and Vella (2007) suggest a procedure to estimate this model. First, the reduced form of the time-variant heterogeneity underlying the endogeneity bias, i.e. part-time employment decision, is estimated by a fixed effects procedure allowing for dynamics. Second, the wage equation is estimated including a control function constructed by the estimates of the first step and including fixed effects. In both steps, incidental parameter bias arises and is addressed by appropriate corrections.

This method allows for a flexible error structure. The idiosyncratic disturbances ϵ_{1it} and ϵ_{2it} are assumed to be jointly normally distributed with variances σ_1^2 and σ_2^2 respectively and a potential nonzero covariance σ_{12}, while

$$E[\epsilon_{jit}|x_i^t, z_i^t, PT_i^{t-1}, \alpha_{1i}, \alpha_{2i}] = 0, \tag{3}$$
$$j = 1, 2;\ i = 1, \ldots, n;\ t = 1, \ldots, T,$$

where $x_i^t = [x_{i1}, \ldots, x_{it}]$, $z_i^t = [z_{i1}, \ldots, z_{it}]$ and $PT_i^t = [PT_{i0}, \ldots, PT_{it}]$. No condition on the joint distribution of α_{1i} and α_{2i}, given x_i^t, z_i^t and PT_{it-1} is imposed. This structure indicates that the endogeneity of the part-time status in the wage equation arises both through the correlation in the unobserved individual fixed effects and also through the contemporaneous correlation in the idiosyncratic errors.

The part-time employment equation is estimated by a dynamic fixed effects probit method and the incidental parameter bias due to nonlinear estimation is reduced by the method described in Hahn and Kuersteiner (2004). A discussion of the source of the incidental parameter bias and the used correction method is

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

provided in the second chapter of this dissertation. After obtaining the estimates of the part-time employment equation, estimation results are used to construct the control function λ_{it}. As the model is estimated by probit, the control function is equal to the generalized residual. So,

$$\lambda_{it} = \begin{cases} \frac{\phi(\xi_{it})}{\Phi(\xi_{it})} & \text{if } PT_{it} = 1, \\ \frac{-\phi(\xi_{it})}{1-\Phi(\xi_{it})} & \text{if } PT_{it} = 0, \end{cases} \tag{4}$$

where $\xi_{it} = PT_{it-1}\beta_1 + x'_{it}\beta_2 + z'_{it}\beta_3 + \alpha_{1i}$, while $\phi(.)$ and $\Phi(.)$ respectively denote the probability density function and cumulative distribution function of the standard normal distribution.

In the second step the following equation is estimated by fixed effects OLS

$$wage_{it} = PT_{it}\theta_1 + x'_{it}\theta_2 + \delta\hat{\lambda}_{it} + \alpha_{2i} + \epsilon_{2it}, \tag{5}$$

where $\hat{\lambda}_{it}$ is the estimated control function. Note that this equation accounts for time-invariant endogeneity by including fixed effects and time-variant endogeneity as an estimate of the control function is also included as a covariate.

When estimating the wage equation by taking deviations from means, the unobserved time-invariant individual effects α_{2i} drop out. However, since the estimate of the control function is a nonlinear function of the individual effects of the part-time employment equation, estimates of α_{1i} enter the wage equation. Hence, the incidental parameter bias is carried over to the wage equation through the control function. Fernandez-Val and Vella (2007) propose a way to address the bias in this case. Their large-T bias correction method provides an analytical expression of the bias with which the incidental parameter bias in the OLS estimates can be reduced accordingly.

3.4. Data and Sample Description

The data is drawn from the German Socio-Economic Panel for the period 1996-2004. The first wave is used for constructing the lagged part-time employment dummy variable. Native and foreigner[3] women between ages 20 and 65, living in western Germany, having completed formal education, being employed during the analyzed period, not being self-employed, and not having missing data for this period

[3] Only foreigner women with Greek, Italian, Spanish, Turkish or former Yugoslavian origins are included.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

are kept in the analysis. A woman is considered working if she declares that she has been employed in the month preceding the survey interview. As the objective of this study is to measure whether women changing from full-time to part-time employment experience any pay differentials, women who are unemployed in the analyzed period are excluded as well as the ones exiting labor force. Women working in eastern Germany are also excluded due to the considerable differences in the part-time employment rates between western and eastern Germany (45 and 28 percent in 2004 respectively, see Federal Statistical Office). Workers with more than one job are not included in the analysis since they are likely to choose part-time work for other reasons than the part-timers with only one job.

Gross monthly wages in euros are reported for the month preceding the survey interview. They are first divided by 4.33 to obtain weekly wages and then by the number of actual weekly hours worked for pay to obtain hourly wages. Women with an hourly wage of less than 1 euro and more than 100 euros are excluded. To make wages comparable over time, they are deflated by the consumer price index.

The variable of interest - the part-time employment dummy - is based on the self-assessment of the worker in response to the question: “Are you currently engaged in paid employment? Which of the following applies best to your status?”. Part-time and full-time employment are possible answers next to others such as being in training or doing military service. Self-assessment is one of the two most commonly used measures in the literature. The alternative measure is based on hours worked and classifies individuals working below a certain number of hours as part-time workers. This cut-off point depends on the labor market conditions. For instance, in the UK the standard cut-off point is 30 hours whereas it is 35 in the US (see Blank (1990) and Manning and Petrongolo (2004)). The main difference between the two measures of part-time employment is the reference number of hours for a full-time worker. The hours-based measure uses a single cut-off point, usually suggested by legislation, that is expected to hold for the entire working population. On the other hand, with the self-assessment measure, workers declare their part-time status taking into account comparable workers in their companies, occupations and/or industries. This subjectivity may be advantageous as studies on the part-time wage gap usually discuss whether part-timers experience a wage penalty or premium relative to comparable full-timers having the same job. Moreover in

Germany, the statutory definition of a part-time worker is subjective. According to the legislation, any employee whose regular weekly working time is shorter than that of a comparable full-time employee is working part-time.

Table 1 summarizes the sample characteristics. The first column considers the observations used for estimating the part-time employment decision. This sample consists of women who work part-time in some years and full-time in others during the analyzed period. The characteristics in the second column are of women who are working part-time during the entire period, while those in the third column are of women who are never working part-time. These three columns form a partition of the sample used for the wage equation which is presented in the fourth column. For the part-time employment equation the sample consists of 752 observations for 94 women; for the wage equation there are 3,264 observations for 408 women.

The sample characteristics show that women who are working full-time in each period are the youngest among all groups. In the sample used for estimating the wage equation, women who work full-time have a higher hourly wage than those working part-time. This suggests that when not controlling for other factors, there is a part-time wage penalty. The difference between the hourly wages of full-time workers (12.85 euros) and part-time workers (12.39 euros) implies around 4 percent unconditional part-time penalty. Similarly, the sample used in estimating the part-time employment decision suggests evidence for a 1 percent pay penalty for part-time workers.

Women who are always employed part-time work around 17 hours less than those who always work full-time (23.36 and 39.87 hours respectively). The average number of working hours in the wage equation sample is 33. In all four samples, women have around 11.5 years of schooling on average. Given that the years of schooling are very similar for these four groups, the differences in pseudo experience (age minus years of schooling minus 6) mimics those in age. Interestingly, only 2 percent of the women always working part-time are foreigners whereas for women working full-time in all periods this number is 14 percent. The fraction of foreigners in the sample of women who sometimes work part-time is in between these numbers.

Women who are always working part-time are more likely to be married, especially compared to those who always work full-time. Compared to the women who

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

always or sometimes work full-time, there are surprisingly few women with disabilities who always work part-time. Public sector workers are relatively present in the always working part-time sample with a fraction of 42 percent. They are equally represented in the other samples with about 32-35 percent.

Since usually mothers take care of the children during working hours in Germany (see Federal Statistical Office (2006)), part-time work is especially attractive for mothers who have to, or want to, participate in gainful employment. Although a kindergarten place is statutorily guaranteed and usually available for every child of three years and older, most of these services are available only for part of the day and private child care is expensive (see Wolf (2002)). Hence, only when children reach school age, working becomes more feasible and attractive for most women. From Table 1 it can be seen that women who work are much more likely to have children at school age than very young children, reflecting that women with young children are less likely to work and thus be included in this analysis. Interestingly, women with young children who work are not considerably more engaged in part-time than in full-time employment. Though, when the children reach school age, the preference for part-time work is very significant.

The distribution of women across one-digit ISCO-88 occupational codes shows that, independent of the sample, women included in the analysis work mostly as *technicians and associate professionals*, *clerks* and *service workers and shop and market sales workers*. The latter occupational group has a higher representation among women who occasionally work part-time.

Table 1 also presents information on the number of years women are working part-time. In the sample used for the part-time equation estimation, there are slightly fewer part-time than full-time observations (48 versus 52 percent). In the sample used for the wage equation, 36 percent of the women work part-time. The distribution of the number of years worked part-time is rather flat, except for 1 and 2 years which are slightly higher and lower respectively. For women working both part-time and full-time during the period under analysis, more insight can be obtained by looking at the direction of the changes. As shown in the table, changes from part-time to full-time employment and vice-versa occur with about the same frequency (with 12-13 percent in the part-time equation sample and due to the larger pool with 3 percent in the wage equation sample).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

During the period of analysis, the German Government introduced a new legislation to promote part-time work with the objective to reduce unemployment. The Act Teilzeit und Befristungsgesetz (TzBfG) came into effect as of 1 January 2001. The legal principle of equal treatment of part-time and full-time workers existed since 1985. Accordingly, a part-time worker was explicitly entitled to be paid or receive other dividable monetary benefits at least in an extent corresponding to the share of his/her working time relative to the working time of a comparable full-time worker. The new Act strengthened and replaced the previous legislation from 1985 while implementing the European Council's Directive (97/80/EC79) on part-time employment. The aim was to facilitate the development of part-time work on a voluntary basis and to contribute to the flexible organization of working time in a way that considers both the needs of employers and workers. In brief, the new Act provides the right to part-time work with a high level of employee protection. Accordingly, employers must enable employees - including those at managerial levels - to reduce the agreed working time. Any level of reduction can be claimed as no specific degree of reduction is defined. Only if the reduction of working time would be excessive in cost or have a negative effect on the company's work flow or safety, the employer may reject the demand. An employee must have been continuously employed for more than 6 months in a company with more than 15 employees to be eligible for the right to part-time work. The TzBfG is also applicable to marginal part-time workers excluded from the statutory systems of social security (see Schmidt (2002) and Schmidt (2006) for details on the Act). Hence, while the new Act is expected to have no impact on the monetary compensation of part-time workers, it is expected to increase the fraction of part-time workers in the German labor markets. Figure 1 shows the part-time employment rates during the analyzed period based on data from the German Socio-Economic Panel. Between 1997 and 2004 part-time employment is increasing from 34 to 42 percent, which is about 1 percent per year. A slightly higher increase can indeed be seen around 2001. Data from Microcensus and Eurostat suggest similar patterns and levels.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

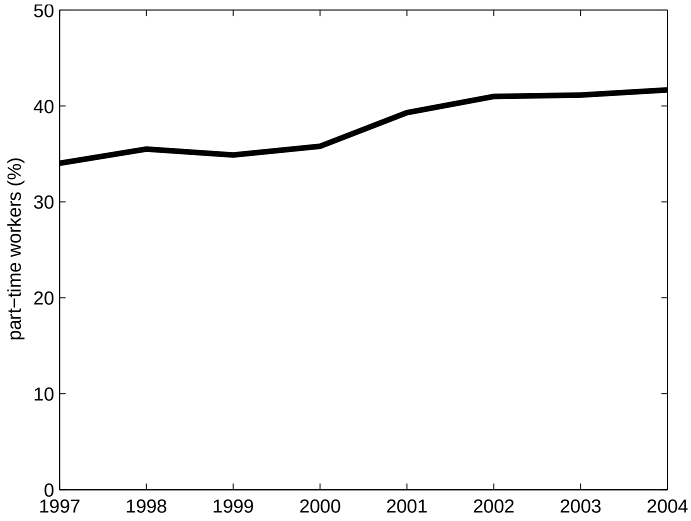

FIGURE 1. The proportion of working women in part-time employment.

3.5. Estimation Results

The following subsections present the estimation results for the part-time employment and wage equation respectively. Four specifications differing in the inclusion of fertility variables and occupation dummies are reported for each estimation. In addition to the estimation results of the preferred model, results from estimations differing in the assumptions regarding individual heterogeneity and dynamics are also presented.

3.5.1. The part-time employment equation. The analysis starts with the estimation results of the bias corrected dynamic fixed effects probit approach (Table 2). Then the pooled probit estimation results (Table 3) and the bias corrected static fixed effects estimation results (Table 4) are presented for comparison.

The following covariates are used for the part-time employment estimation: the *lagged part-time employment* dummy; *pseudo experience* and its square; the *married* dummy; the *disability* dummy; the *after 2001* dummy capturing the impact of the legislative change on part-time employment; and the *public sector* dummy indicating whether the worker is employed in the government sector or not. These variables constitute the base estimation presented in the first column of the part-time employment estimations. In the second column, three fertility variables are added, namely the *number of children of age 0 to 2*, *age 3 to 5* and *age 6 to 17*. In the third column, the fertility variables are replaced by occupational dummies.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Finally, the last column includes both fertility variables and occupational dummies. Since there are few observations in some occupational groups, *skilled agricultural and fishery workers*, *craft and related trades workers*, *plant and machine operators and assemblers* and *elementary occupations* are grouped together *(agr. + craft + oper. + elem.)*. Next to this combined group, *service workers and shop and market sales workers* are also included in the estimations *(service workers)*. The benchmark occupational group consists of workers with higher formal schooling, namely *legislators, senior managers and officials*, *professionals*, *technicians and associate professionals* and *clerks*. As only women who have completed formal education are kept in the analysis, the inclusion of fixed effects captures the effect of educational attainment. Accordingly, when an estimation procedure without individual specific fixed effects is carried out, a *years of schooling* variable is added as covariate and for the same reason, a *foreigner* dummy as well.

The *lagged part-time employment* dummy is included in the analysis to determine whether there is temporal persistence in part-time employment and if so to evaluate the relative importance of *state dependence* and *worker heterogeneity*. Several studies found that a worker's part-time employment choice exhibits persistence over time, i.e. the probability that a worker is observed to be employed part-time at time t given that she was observed to work part-time at time $t-1$, is higher than if she would have been observed working full-time at time $t-1$. However, persistence observed in the data does not directly translate into a statement about workers' behavior. There are two main factors contributing to the observed persistence in part-time employment.

Firstly, it is possible that working part-time at time $t-1$ makes the worker more likely to again work part-time at time t. Such a causal link between current and past behavior - known as *state dependence* - can exist for many reasons. For instance, it has been widely documented that many part-time jobs are 'bad' jobs which are in low-wage occupations, do not provide good career opportunities and become a trap resulting in reoccurring part-time work (Connolly and Gregory (2005)). A similar argument can be made with respect to human capital accumulation. A woman in part-time employment accumulates work experience at a lower rate and this can affect her labor market opportunities negatively.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

Secondly, it may be that the causality between past and current part-time employment does not fully explain the observed persistence. Differences in observable and unobservable characteristics across workers - known as *worker heterogeneity* - can influence their choices as well. If these worker characteristics are persistent over time, past history reflects individual heterogeneity and the occurrence of a given state makes the same state more likely to occur in the future. In this case, a worker who was working part-time at time $t-1$ is more likely to have characteristics leading to part-time work at time t than if she would have been employed full-time at time $t-1$.

Identifying the relative importance of state dependence and worker heterogeneity in the analysis is important. For instance, temporary government policies to promote part-time work will have a longer-lasting effect if there is strong state dependence, but the impact will disappear once the implementation of the policy stops if this is not the case. Accounting for both causes of observed persistence is also important regarding the reliability of the estimation results. If worker heterogeneity plays a role in the true model but is ignored in the estimation, the degree of state dependence will be overestimated. On the other hand, if the estimation incorrectly does not consider state dependence, the importance of worker heterogeneity will be exaggerated. For these reasons, an estimation method that allows for both state dependence and worker heterogeneity, observed and unobserved, is followed in this study.

Table 2 presents the estimation results of the part-time employment equation using the dynamic bias corrected fixed effects method. Several of the covariates are statistically significant and all of them have the expected signs. The impact of the lagged part-time employment is positive and statistically significant which, as unobserved time-invariant worker heterogeneity is controlled for, indicates the presence of state dependence. A woman who worked part-time in a given year is 5 percent more likely to work part-time in the following year than if she would have been working full-time in that year. This result is robust across all specifications. Further investigation shows more clearly the role of unobserved worker heterogeneity and the sample characteristics in explaining the dynamics of part-time work. For the pooled probit model which ignores the unobserved time-invariant worker heterogeneity (see Table 3), the effect of the *lagged part-time employment* dummy is found to be above 80 percent. However, this result is largely driven by workers who are always in either part-time or in full-time employment during the period under analysis. When the same pooled probit estimation is carried out for the sample consisting only of workers who change status during the analyzed period, the impact of the *lagged part-time employment* dummy decreases to 50 percent (estimation results not presented here). Considering the difference between this finding and the result from the dynamic bias corrected fixed effects estimation, it can be concluded that an important part of the observed persistence in part-time employment is due to unobserved time-invariant worker heterogeneity.

The effect of pseudo experience on part-time employment is a combination of the statistically significant first and second order terms. The impact of pseudo experience on the propensity to work part-time is increasing until 22 years and then decreasing afterwards.

To capture the impact of the worker's non-work related situation, variables related to marital status and health limitations are added to the analysis. Being married has no effect on the propensity to work part-time and this result is robust across all specifications. The *disability* dummy is statistically significant and suggests that having health limitations increases the probability of working part-time by 4 percent.

The legislative change that took place at the beginning of 2001 to promote part-time work in Germany had no effect on the part-time employment decision as indicated by the statistical insignificance of the *after 2001* dummy. Ignoring unobserved time-invariant worker heterogeneity does not change this result (see Table 3). Considering that the objective of the legislation was to promote part-time work, this finding is somewhat surprising. It might be the case that in practice either women already had the flexibility to work part-time on a voluntary basis or on the opposite that the legislation was not effective enough to stimulate additional part-time work. Graphical inspection of the part-time employment rate (see Figure 1) also does not suggest a considerable additional increase in part-time employment rate levels. On the other hand, as only women employed during the entire sample period are considered in the estimation, the effect of the legislation on women who were not employed prior to 2001 cannot be observed.

The *public sector* dummy has no statistically significant effect both when unobserved time-invariant worker heterogeneity is accounted for and not.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

The coefficients and marginal effects, as well as the level of their statistical significance, change only slightly when the fertility variables are included. Having very young children (ages 0-2) increases the propensity to work part-time compared to full-time around 4 percent whereas no statistically significant effect is observed for children at older ages. However, when ignoring unobserved time-invariant worker heterogeneity (see Table 3), school age children are also found to have a statistically significant positive effect on the propensity to work part-time. This effect though is mainly driven by women who always or never work part-time as no statistically significant effect is observed for the pooled probit estimation using the sample of women who change part-time/full-time status (estimation results not presented here).

As certain professions might be more suitable for part-time jobs than others, it is important to control for occupations. However, occupations are chosen every period and the estimation method only controls for unobserved time-invariant worker heterogeneity, hence the results should be treated carefully due to possible endogeneity with a time varying nature. Having an occupation in the group *agr. + craft + oper. + elem.* increases the propensity to work part-time by 3 percent compared to the benchmark group consisting of legislators, professionals, technicians and clerks. Including the occupational dummies does only have a minor effect on the results of the base estimation, though no impact on their statistical significance is observed.

Finally, the presence of fixed effects is tested by a likelihood ratio test. The result suggests that the null hypothesis of no individual specific fixed effects can be rejected. To account for the aging of the sample during the period of analysis, one would ideally include time dummies. Although not fully supported by the estimation method, these estimations were also carried out and no major differences regarding the variables of interest were found (estimation results not presented here).

To see the impact of the dynamic component in the part-time employment decision, Table 4 shows the estimation results of the static part-time employment equation with individual specific fixed effects. The incidental parameter bias is corrected according to the method presented in Hahn and Newey (2004). The signs of the coefficients are unaffected. The most important change in significance levels is that the *number of children age 3-5* variable has become statistically significant. Although to a lesser extent than the younger children, children in this age group tend to increase the propensity to work part-time.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

3.5.2. The wage equation. The estimation results of the wage equation are reported in Tables 5, 6 and 7. Table 5 presents the results from the preferred model, i.e. the bias corrected fixed effects OLS estimation where the control function, based on the bias corrected dynamic fixed effects estimation of the part-time equation (Table 2), is added as a covariate. Table 6 presents pooled and fixed effects OLS estimations of the wage equation ignoring selection into part-time employment. Finally, in order to assess the importance of unobserved time-invariant worker heterogeneity, two-step estimation results with pooled data are presented in Table 7.

As the part-time employment equation has a binary dependent variable, observations of women who always or never work part-time during the sample period are excluded from the first step estimation. In the second step, however, observations for these women are also included.

The base wage equation includes as covariates the *part-time* dummy - the variable of interest; *pseudo experience* and its square; the *married* dummy; the *public sector* dummy; and the control function. In addition, occupation dummies are also included in some specifications. As in the first step, a *years of schooling* variable and a *foreigner* dummy are added as covariates for pooled estimations. Note that in the case of two-step estimations, the difference between columns 1 and 2, and likewise between columns 3 and 4, stems from different specifications of the part-time employment equation.

Two exclusion restrictions are employed in the first step estimation to ensure that the identification of the wage equation does not depend on distributional assumptions regarding the part-time equation. The exclusion restrictions are assumed to explain selection into part-time employment and not to have a direct impact on the received wage. The only effect on wage is thus expected to be indirect through the endogenous part-time dummy and the control function. These exclusion restrictions are the *disability* dummy and the *lagged part-time employment* dummy.[4] Any possible effect of these variables on wages is assumed to work through its impact on the current employment status. The *disability* dummy is expected to influence one's choice to work full-time or part-time, however, since pay discrimination based on disability is very unlikely to take place in developed labor markets, it should not

[4]Although the *after 2001* dummy is only included in the part-time equation, it does not serve as an exclusion restriction due to its statistical insignificance.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

have a direct effect on wages. The *lagged part-time employment* dummy can also be seen as an exclusion restriction. Although one can argue that it captures human capital investment of previous periods, its impact will already be largely picked-up by the inclusion of the current part-time employment dummy in the wage equation. The estimations of the second and the fourth columns of Tables 5 and 7 are based on part-time employment equations where fertility variables are also included. Although these variables are not included in the wage estimation, they can not be considered as exclusion restrictions as it is likely that employers offer different wages based on the worker's number of children.[5]

Estimation results based on the preferred model (see Table 5) suggest no hourly wage difference between comparable women working part-time employment and full-time. This result implies that there is no wage discrimination based on part-time employment in western Germany. Although the unconditional wage gap indicates a part-time pay penalty of around 4 percent, this penalty disappears once observables and unobservables are taken into account. The difference between the found impacts of part-time employment on wage needs more explanation. The effect of controlling for observed worker characteristics can be analyzed by performing a one-step pooled OLS estimation of the wage equation. The results are shown in the first two columns of Table 6. After controlling for observables, there is a wage penalty of around 8 percent regardless of the specification. This is even larger than the unconditional pay penalty. However, when also unobservable time-invariant worker heterogeneity is taken into account via a fixed effects estimation, see the third and fourth column, a wage premium of around 3 percent is found. Controlling for unobservable worker characteristics thus explains the improvement in the wage of part-time workers relative to full-time workers. To focus on the isolated impact of selection into part-time employment, a two-step procedure using the pooled data is used (see Table 7). For all specifications a statistically significant part-time penalty of around 11 percent is found. Hence, accounting for selection into part-time work increases the hourly wage penalty of part-time workers relative to full-time workers by 3 percent (from 8 to 11 percent). This impact is of a similar magnitude as controlling for selection when fixed effects are included (a pay premium of 3 percent turns into no wage

[5]The fertility variables are not included in the wage equation due to potential endogeneity and addressing this issue is outside the scope of this chapter.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

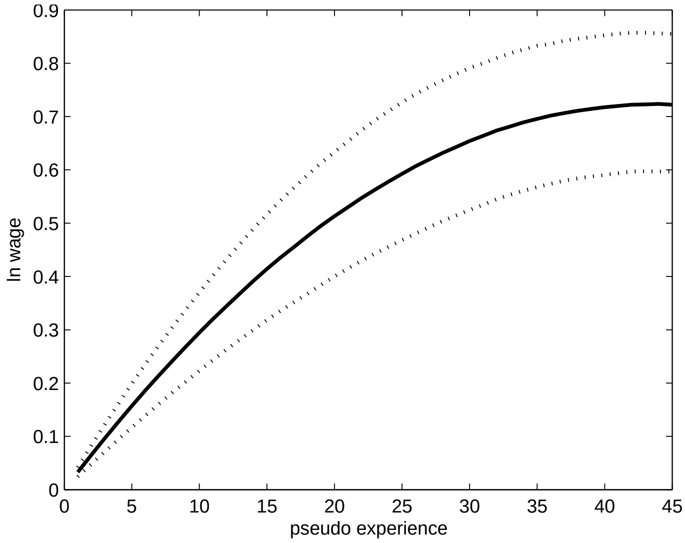

FIGURE 2. The direct effect of experience on log hourly wages with 95 percent confidence interval.

differential, see Table 5 and the third and fourth column of Table 6). Thus, while only controlling for observable variables increases the unconditional wage penalty regardless of whether selection into part-time employment is taken into account or not, addressing time-invariant worker heterogeneity more than offsets this increase and eliminates the wage gap fully when also selection is controlled for.

The impact of pseudo experience is positive and highly statistically significant. The direct contribution of pseudo experience on wage is shown in Figure 2, which abstains from the effect through selection. The direct effect is positive for all years of experience and is statistically significant. While for short periods of experience the effect on wage of an additional year of experience is considerable, close to retirement the effect almost disappears.

The remaining covariates are statistically insignificant, except for the *service workers* occupational dummy and the control function. The statistical significance of the latter can be interpreted as evidence of selection into part-time employment. Combined with the statistical significance of the fixed effects, this confirms the validity of the followed approach.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

3.6. Conclusion

This chapter sheds more light on the wage differentials between part-time and full-time female workers in western Germany for the period 1996-2004. Previous research on the part-time wage differential in western Germany has produced opposing findings as both a premium and a penalty are found (see Bardasi and Gornick (2000) and Manning and Petrongolo (2004)). Wolf (2002) finds a penalty for part-time employees working less than 20 hours but no wage difference for other part-timers. This study incorporates several of the factors that are identified as important drivers by economic theory. By exploiting the panel data structure, controlling for both time-variant and time-invariant unobservable worker heterogeneity and allowing for a dynamic decision making process, this study presents a detailed description of the mechanisms underlying wage differentials.

Although data from the German Socio-Economic Panel suggest an unconditional wage penalty of about 4 percent, using a recently developed econometric method to address the above mentioned issues no evidence for a part-time wage differential is found. The difference between the unconditional pay penalty and the found result is mainly explained by the unobserved time-invariant worker heterogeneity. The applied method also uncovers the relevance of state dependence for the decision to work part-time or full-time.

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Appendix 3.A. Tables

The tables on the following pages present the sample characteristics and estimation results.

	Part-time employment sample	Always part-time	Never part-time	Wage sample
age	45.03 (7.97)	44.96 (6.76)	42.85 (9.19)	43.88 (8.43)
wage	11.92 (5.61)	12.62 (5.05)	13.06 (4.72)	12.69 (5.04)
wage of full-timers	11.97 (5.30)	– (–)	13.06 (4.72)	12.85 (4.85)
wage of part-timers	11.86 (5.94)	12.62 (5.05)	– (–)	12.39 (5.34)
hours	28.22 (11.37)	23.36 (6.61)	39.87 (6.90)	33.06 (10.88)
pseudo experience	27.60 (8.01)	27.35 (6.93)	25.30 (9.44)	26.34 (8.61)
years of schooling	11.43 (2.40)	11.61 (2.44)	11.55 (2.30)	11.54 (2.36)
foreigner	0.12	0.02	0.14	0.10
married	0.76	0.87	0.57	0.69
disability	0.09	0.04	0.06	0.06
public sector	0.32	0.42	0.33	0.35
number of children age 0-2	0.02	0.00	0.01	0.01
number of children age 3-5	0.03	0.03	0.02	0.02
number of children age 6-17	0.56	0.81	0.20	0.44
legislators, senior officials and managers	0.01	0.00	0.02	0.01
professionals	0.10	0.10	0.08	0.09
technicians and associate professionals	0.24	0.33	0.30	0.29
clerks	0.20	0.27	0.30	0.27
service workers and shop and market sales workers	0.27	0.18	0.10	0.16
skilled agricultural and fishery workers	0.01	0.00	0.00	0.00
craft and related trades workers	0.02	0.02	0.07	0.04
plant and machine operators and assemblers	0.03	0.00	0.07	0.04
elementary occupations	0.11	0.10	0.06	0.08
part-time	0.48	1.00	0.00	0.36
number of years part-time				
0	0	0	212	212
1	23	0	0	23
2	8	0	0	8
3	11	0	0	11
4	15	0	0	15
5	11	0	0	11
6	10	0	0	10
7	16	0	0	16
8	0	102	0	102
part-time to full-time transitions	0.13	-	-	0.03
part-time to part-time transitions	0.35	1.00	-	0.33
full-time to part-time transitions	0.12	-	-	0.03
full-time to full-time transitions	0.39	-	1.00	0.61
number of observations	752	816	1,696	3,264
number of individuals	94	102	212	408

TABLE 1. Properties of the samples used in the wage and part-time employment equations, as well as those of women who work part-time or full-time in all periods. Standard deviations are in parenthesis.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
lag part-time	1.1667*** (0.1104)	1.1557*** (0.1122)	1.1561*** (0.1108)	1.1445*** (0.1128)
	[0.0527***] (0.0045)	[0.0518***] (0.0045)	[0.0521***] (0.0045)	[0.0511***] (0.0045)
pseudo experience	0.2002** (0.0893)	0.2242** (0.0901)	0.2002** (0.0896)	0.2248** (0.0905)
	[0.0075**] (0.0030)	[0.0084***] (0.0030)	[0.0075**] (0.0030)	[0.0083***] (0.0030)
pseudo experience^2	−0.0045*** (0.0015)	−0.0046*** (0.0015)	−0.0045*** (0.0015)	−0.0046*** (0.0015)
	[−0.0002***] (0.0001)	[−0.0002***] (0.0001)	[−0.0002***] (0.0001)	[−0.0002***] (0.0001)
married	0.3897 (0.2939)	0.3741 (0.2950)	0.4157 (0.2959)	0.4009 (0.2969)
	[0.0146] (0.0095)	[0.0139] (0.0094)	[0.0155] (0.0095)	[0.0148] (0.0095)
disability	1.2088*** (0.4677)	1.1767** (0.4727)	1.2115*** (0.4670)	1.1790** (0.4720)
	[0.0416***] (0.0139)	[0.0403***] (0.0139)	[0.0415***] (0.0139)	[0.0402***] (0.0140)
after 2001	0.0888 (0.2008)	0.0916 (0.2024)	0.0938 (0.2007)	0.0969 (0.2023)
	[0.0033] (0.0064)	[0.0034] (0.0065)	[0.0035] (0.0064)	[0.0036] (0.0064)
public sector	0.1149 (0.2776)	0.1138 (0.2800)	0.1532 (0.2828)	0.1538 (0.2850)
	[0.0043] (0.0089)	[0.0042] (0.0089)	[0.0057] (0.0091)	[0.0057] (0.0091)
number of children age 0-2		1.0506** (0.5231)		1.0559** (0.5233)
		[0.0391**] (0.0168)		[0.0392**] (0.0168)
number of children age 3-5		0.3985 (0.3920)		0.4094 (0.3924)
		[0.0148] (0.0125)		[0.0152] (0.0125)
number of children age 6-17		0.1674 (0.1485)		0.1730 (0.1483)
		[0.0062] (0.0048)		[0.0064] (0.0047)
service workers			0.2033 (0.3824)	0.2196 (0.3848)
			[0.0493] (0.0931)	[0.0529] (0.0931)
agr. + craft + oper. + elem.			0.9291* (0.4788)	0.9510** (0.4831)
			[0.0340**] (0.0149)	[0.0345**] (0.0149)
Loglikelihood	-358.88	-356.88	-358.08	-356.06
LR test fixed effects	690.08*** (χ^2_{93})	674.54*** (χ^2_{93})	688.33*** (χ^2_{93})	673.45*** (χ^2_{93})

Robust standard errors in parentheses, marginal effects in square brackets.

*** significant at 1%; ** significant at 5%; * significant at 10%.

Table 2. Hahn & Kuersteiner bias corrected estimates of the part-time equation.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
lag part-time	2.9973*** (0.0727)	2.9428*** (0.0750)	2.9932*** (0.0728)	2.9403*** (0.0752)
	[0.8472***] (0.0296)	[0.8301***] (0.0346)	[0.8455***] (0.0308)	[0.8288***] (0.0359)
pseudo experience	0.0624** (0.0249)	0.0640** (0.0270)	0.0653*** (0.0249)	0.0659** (0.0270)
	[0.0071**] (0.0032)	[0.0072**] (0.0035)	[0.0074**] (0.0032)	[0.0074**] (0.0035)
pseudo experience^2	−0.0011** (0.0005)	−0.0010** (0.0005)	−0.0012*** (0.0005)	−0.0010** (0.0005)
	[−0.0001**] (0.0001)	[−0.0001*] (0.0001)	[−0.0001**] (0.0001)	[−0.0001*] (0.0001)
married	0.3228*** (0.0859)	0.2623*** (0.0890)	0.3216*** (0.0860)	0.2626*** (0.0891)
	[0.0379***] (0.0106)	[0.0300***] (0.0105)	[0.0376***] (0.0105)	[0.0300***] (0.0105)
disability	0.1510 (0.1428)	0.1837 (0.1447)	0.1516 (0.1436)	0.1839 (0.1454)
	[0.0174] (0.0169)	[0.0209] (0.0171)	[0.0174] (0.0169)	[0.0209] (0.0171)
after 2001	−0.0018 (0.0743)	0.0088 (0.0753)	−0.0073 (0.0745)	0.0034 (0.0755)
	[−0.0002] (0.0084)	[0.0010] (0.0084)	[−0.0008] (0.0084)	[0.0004] (0.0084)
public sector	0.0787 (0.0785)	0.0895 (0.0797)	0.0745 (0.0786)	0.0871 (0.0798)
	[0.0090] (0.0089)	[0.0101] (0.0089)	[0.0085] (0.0089)	[0.0098] (0.0089)
number of children age 0-2		0.6904** (0.2766)		0.6609** (0.2773)
		[0.0774**] (0.0327)		[0.0739**] (0.0325)
number of children age 3-5		−0.1796 (0.2220)		−0.1911 (0.2226)
		[−0.0201] (0.0246)		[−0.0214] (0.0246)
number of children age 6-17		0.2120*** (0.0566)		0.2101*** (0.0566)
		[0.0238***] (0.0067)		[0.0235***] (0.0067)
service workers			0.1097 (0.0986)	0.0976 (0.0993)
			[0.0134] (0.0121)	[0.0121] (0.0124)
agr. + craft + oper. + elem.			−0.1162 (0.1104)	−0.1081 (0.1116)
			[−0.0131] (0.0123)	[−0.0120] (0.0123)
years of schooling	−0.0015 (0.0172)	0.0030 (0.0174)	−0.0032 (0.0181)	0.0013 (0.0184)
	[−0.0002] (0.0019)	[0.0003] (0.0020)	[−0.0004] (0.0020)	[0.0001] (0.0021)
foreigner	−0.3334** (0.1390)	−0.4048*** (0.1417)	−0.2908** (0.1435)	−0.3606** (0.1466)
	[−0.0382**] (0.0162)	[−0.0461***] (0.0168)	[−0.0331**] (0.0166)	[−0.0408**] (0.0171)
constant	−2.6770*** (0.3836)	−2.9301*** (0.4210)	−2.6951*** (0.3964)	−2.9322*** (0.4334)
Loglikelihood	-703.92	-694.15	-702.25	-692.78

Robust standard errors in parentheses, marginal effects in square brackets.

*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 3. Pooled probit estimates of the part-time equation.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
lag part-time				
pseudo experience	0.2312^{**} (0.0940)	0.2540^{***} (0.0979)	0.2272^{**} (0.0942)	0.2500^{**} (0.0981)
	$[0.0094^{**}]$ (0.0037)	$[0.0102^{***}]$ (0.0038)	$[0.0092^{**}]$ (0.0037)	$[0.0101^{***}]$ (0.0038)
pseudo experience^2	-0.0050^{***} (0.0015)	-0.0050^{***} (0.0016)	-0.0050^{***} (0.0015)	-0.0049^{***} (0.0016)
	$[-0.0002^{***}]$ (0.0001)	$[-0.0002^{***}]$ (0.0001)	$[-0.0002^{***}]$ (0.0001)	$[-0.0002^{***}]$ (0.0001)
married	0.4050 (0.3107)	0.3802 (0.3125)	0.4334 (0.3135)	0.4077 (0.3153)
	[0.0164] (0.0113)	[0.0153] (0.0113)	[0.0175] (0.0114)	[0.0163] (0.0113)
disability	1.2086^{**} (0.4762)	1.1548^{**} (0.4767)	1.2078^{**} (0.4768)	1.1538^{**} (0.4773)
	$[0.0447^{**}]$ (0.0176)	$[0.0426^{**}]$ (0.0176)	$[0.0444^{**}]$ (0.0177)	$[0.0424^{**}]$ (0.0177)
after 2001	0.0759 (0.2163)	0.0555 (0.2178)	0.0855 (0.2171)	0.0655 (0.2186)
	[0.0031] (0.0078)	[0.0022] (0.0078)	[0.0035] (0.0078)	[0.0026] (0.0078)
public sector	0.1998 (0.2846)	0.1951 (0.2867)	0.2245 (0.2892)	0.2223 (0.2912)
	[0.0081] (0.0104)	[0.0079] (0.0103)	[0.0091] (0.0105)	[0.0089] (0.0104)
number of children age 0-2		1.0396^{*} (0.5487)		1.0365^{*} (0.5485)
		$[0.0419^{**}]$ (0.0201)		$[0.0417^{**}]$ (0.0200)
number of children age 3-5		0.9276^{**} (0.4709)		0.9260^{**} (0.4708)
		$[0.0374^{**}]$ (0.0172)		$[0.0373^{**}]$ (0.0172)
number of children age 6-17		0.2727 (0.1737)		0.2730 (0.1738)
		$[0.0110^{*}]$ (0.0064)		$[0.0110^{*}]$ (0.0063)
service workers			-0.0487 (0.5151)	-0.0253 (0.5109)
			$[-0.0124]$ (0.1411)	$[-0.0056]$ (0.1372)
agr. + craft + oper. + elem.			0.9466 (0.6331)	0.9642 (0.6359)
			$[0.0369^{*}]$ (0.0213)	$[0.0372^{*}]$ (0.0212)
Loglikelihood	-385.15	-381.15	-383.37	-379.37
LR test fixed effects	3124.75^{***} (χ^2_{93})	2839.05^{***} (χ^2_{93})	3105.67^{***} (χ^2_{93})	2826.40^{***} (χ^2_{93})

Robust standard errors in parentheses, marginal effects in square brackets.

*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 4. Hahn & Newey bias corrected estimates of the part-time equation.

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
part-time	−0.0426 (0.0367)	−0.0413 (0.0362)	−0.0378 (0.0367)	−0.0370 (0.0362)
pseudo experience	0.0330*** (0.0045)	0.0330*** (0.0045)	0.0331*** (0.0045)	0.0331*** (0.0045)
	[0.0319]	[0.0317]	[0.0321]	[0.0320]
pseudo experience^2	−0.0004*** (0.0001)	−0.0004*** (0.0001)	−0.0004*** (0.0001)	−0.0004*** (0.0001)
	[−0.0004]	[−0.0004]	[−0.0004]	[−0.0004]
married	0.0108 (0.0211)	0.0107 (0.0211)	0.0109 (0.0211)	0.0109 (0.0210)
	[0.0086]	[0.0087]	[0.0088]	[0.0088]
public sector	−0.0118 (0.0182)	−0.0118 (0.0182)	−0.0129 (0.0182)	−0.0129 (0.0182)
	[−0.0125]	[−0.0125]	[−0.0137]	[−0.0137]
service workers			−0.1516*** (0.0405)	−0.1516*** (0.0404)
			[−0.1526]	[−0.1527]
agr. + craft + oper. + elem.			−0.0137 (0.0434)	−0.0138 (0.0434)
			[−0.0185]	[−0.0187]
control function	0.0493** (0.0214)	0.0486** (0.0211)	0.0452** (0.0215)	0.0449** (0.0212)
R^2	0.8821	0.8821	0.8826	0.8826
F test fixed effects	34.3651*** ($F_{407,2851}$)	34.3648*** ($F_{407,2851}$)	26.2926*** ($F_{407,2849}$)	26.2930*** ($F_{407,2849}$)

Standard errors in parentheses, marginal effects different from coefficients in square brackets.

*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 5. Fernandez-Val & Vella bias corrected fixed effects estimates of the wage equation (control function is based on the estimates of Table 2.)

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
part-time	-0.0826^{***} (0.0125)	-0.0782^{***} (0.0123)	0.0313^{**} (0.0155)	0.0300^{*} (0.0155)
pseudo experience	0.0263^{***} (0.0034)	0.0261^{***} (0.0034)	0.0319^{***} (0.0045)	0.0321^{***} (0.0045)
pseudo experience^2	-0.0004^{***} (0.0001)	-0.0004^{***} (0.0001)	-0.0004^{***} (0.0001)	-0.0004^{***} (0.0001)
married	-0.0196 (0.0134)	-0.0110 (0.0131)	0.0075 (0.0211)	0.0079 (0.0210)
public sector	0.1155^{***} (0.0124)	0.0998^{***} (0.0122)	-0.0134 (0.0182)	-0.0145 (0.0182)
service workers		-0.1491^{***} (0.0163)		-0.1494^{***} (0.0406)
agr. + craft + oper. + elem.		-0.1772^{***} (0.0171)		-0.0212 (0.0431)
years of schooling	0.0679^{***} (0.0026)	0.0567^{***} (0.0027)		
foreigner	-0.0227 (0.0197)	0.0241 (0.0200)		
constant	1.3448^{***} (0.0517)	1.5225^{***} (0.0527)		
R^2	0.5022	0.5340	0.8819	0.8824
F test fixed effects			22.5329^{***} ($F_{407,2853}$)	20.7712^{***} ($F_{407,2853}$)

Standard errors in parentheses.

*** significant at 1%; ** significant at 5%; * significant at 10%.

Table 6. Pooled OLS estimates of the wage equation (columns 1 and 2) and fixed effects OLS estimates of the wage equation (columns 3 and 4).

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357

	(1)	(2)	(3)	(4)
part-time	-0.1151^{***} (0.0145)	-0.1132^{***} (0.0145)	-0.1086^{***} (0.0143)	-0.1069^{***} (0.0143)
pseudo experience	0.0275^{***} (0.0034)	0.0274^{***} (0.0034)	0.0273^{***} (0.0034)	0.0272^{***} (0.0034)
	[0.0266]	[0.0266]	[0.0264]	[0.0264]
pseudo experience^2	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)	-0.0005^{***} (0.0001)
	[−0.0005]	[−0.0005]	[−0.0004]	[−0.0005]
married	−0.0128 (0.0134)	−0.0132 (0.0134)	−0.0048 (0.0131)	−0.0052 (0.0132)
	[−0.0174]	[−0.0167]	[−0.0090]	[−0.0084]
public sector	0.1164^{***} (0.0124)	0.1164^{***} (0.0124)	0.1006^{***} (0.0122)	0.1006^{***} (0.0122)
	[0.1153]	[0.1152]	[0.0996]	[0.0995]
service workers			-0.1460^{***} (0.0162)	-0.1462^{***} (0.0162)
			[−0.1474]	[−0.1474]
agr. + craft + oper. + elem.			-0.1780^{***} (0.0171)	-0.1779^{***} (0.0171)
			[−0.1765]	[−0.1766]
years of schooling	0.0678^{***} (0.0026)	0.0678^{***} (0.0026)	0.0566^{***} (0.0027)	0.0566^{***} (0.0027)
	[0.0678]	[0.0677]	[0.0567]	[0.0566]
foreigner	−0.0301 (0.0197)	−0.0297 (0.0197)	0.0177 (0.0200)	0.0181 (0.0200)
	[−0.0254]	[−0.0243]	[0.0215]	[0.0225]
control function	0.0595^{***} (0.0122)	0.0566^{***} (0.0125)	0.0553^{***} (0.0121)	0.0527^{***} (0.0124)
constant	1.3380^{***} (0.0516)	1.3384^{***} (0.0517)	1.5145^{***} (0.0526)	1.5150^{***} (0.0527)
R^2	0.5065	0.5060	0.5375	0.5371

Standard errors in parentheses. Marginal effects different from coefficients in square brackets.

*** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 7. Pooled OLS estimates of the wage equation (control function is based on the estimates of Table 3.)

Isaoglu, Aysen (2009), Empirical Essays on Occupations, Reallocation and Wage Differentials
European University Institute DOI: 10.2870/13357