Labor Market Inequality and Atypical Employment

by

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Dedicated to my brother, Jonathan, who inspired me to take on this crazy endeavor.

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Abstract

The four papers in this dissertation use various methods to examine the question of inequality in the labor market. The first three focus on the theme of "atypical employment," a term used to describe many forms of employment that are not full-time, permanent jobs through a single employer. In recent years, this theme has garnered attention as people perceive that these supposedly insecure and poorly compensated jobs are a growing part of the labor market that threaten all workers' security. The last chapter looks at organizational inequality among academic sociology departments.

The first paper takes a macro approach, examining country level determinants of three types of atypical employment (fixed term, part-time, and self employment) in 30 developed countries. Support is found for three hypotheses: atypical work arrangements are more prevalent i) when there is a strong entrepreneurial culture, ii) when there are legal constraints on firms, and iii) when economic constraints force workers to accept atypical employment. The paper also qualitatively examines countries' legislative and judicial histories with respect to atypical work, and future policy directions are suggested.

The second paper uses propensity score matching and regression analysis to show that fixed term workers' wages are lower than comparable permanent workers' wages. This can result from several mechanisms such as: firms in strict legislative environments using fixed term work as an extended probation period; unions advocating disproportionately for permanent workers; or inferior workers being sorted into fixed term work in a weak labor market. Results suggest that fixed term wages are relatively lower in those countries with strict employment protection legislation.

The third paper takes a theoretical turn, introducing a micro-simulation of job-worker matching with intermediaries (i.e. temp agencies). While many suggest that firms hire workers through intermediaries to save money on compensation, this paper finds that in a world of limited information and geographically limited job search, intermediaries' human resources ability could be a strong enough incentive, independent of compensation. The study also has some auxiliary findings showing that traditional fee structures encourage firms to use intermediaries for low-skill hires and that firms are more likely to use intermediaries when there is more worker heterogeneity. In the empirical analysis, it becomes clear that studies' estimates of indirect employment in the United States are inconsistent, partly because individuals are uncertain of their contractual status and their employer.

The final chapter continues from the organizational perspective, looking at inequality across organizations. This study is the first to consider the relationship between university departments' prestige rank and their centrality in the academic hiring network independent of department size and training. These new controls are important as the correlation between prestige rank and employment network centrality may result from the fact that highly ranked schools train more PhDs, their graduates are more likely to continue in academia, and that highly ranked schools have more faculty. Past research has characterized the correlation between academic departments' prestige rank and their centrality in academic hiring networks as indicative of a "caste" system. However, if academics move between institutions for assorted reasons like wages, location, and specialty areas, there should be no correlation between hiring network centrality and rank. This suggests that academics might prefer to make career switches to top ranked departments, creating the correlation between prestige and centrality, and giving top departments a competitive advantage. This would be one possible explanation why academic rankings are static.

In sum, this dissertation examines several instances of labor market stratification, considering how it is generated by the actions of individuals, firms, and countries, and its consequences.

Chapter 1

Introduction

This thesis examines individuals', organizations', and nations' characteristics and actions as determinants of labor market inequality. Labor market inequality is an important question in both public policy and sociology, and has been of particular interest in recent years as American and European wage inequality (before taxes and transfers) has increased. While there are many hypothesized causes, one that has drawn significant attention is the decline of "traditional" jobs in contrast to the perceived growth of "atypical employment" (work that is not full time and permanent). This kind of employment is often held culpable, as it is generally less well paid and more unstable. While the first three papers (chapters 2 to 4) focus on atypical employment and its effects on inequality, the last paper (chapter 5) takes a different perspective, examining how the labor market can generate inequality across organizations, in this case universities' sociology departments.

The first paper focuses on why countries have different levels of atypical employment. Three main hypotheses are tested for three types of atypical employment (self employment, part-time work, and fixed term work) including: Do entrepreneurial cultures encourage atypical employment? Do institutional constraints like employment protection legislation encourage atypical employment? And, do labor market conditions (e.g., unemployment) encourage atypical employment? This paper includes a historical narrative of the three European countries with the highest levels of atypical employment (fixed term workers in Spain, part-time workers in the Netherlands, and the self employed in Greece) and exam-

ines how well a quantitative analysis can capture the historical dynamics of these three exceptional cases. The paper ends with a review of policy approaches for protecting atypical workers.

The second paper expands on this theme, considering how one type of atypical employment, fixed term work, effects wages. The first question posed is whether empirically fixed term workers earn less than their permanent counterparts, controlling for the fact that less skilled workers tend to be selected into fixed term work. Second, the paper considers how the size of the wage gap between fixed term and permanent workers varies across countries based on institutional characteristics. The paper concludes with a review of recent policies directed at fixed term employment such as those limiting fixed term contract renewals.

The third paper is also on the topic of atypical employment, but takes a theoretical turn, using a micro simulation to examine the role of intermediaries in an artificial labor market. The model is a worker-job matching algorithm implemented in a spatial environment with limited information. The simulation tests whether intermediaries' better matching capabilities are sufficient motivation for firms to hire workers through intermediaries despite the additional cost incurred by the fees they charge. The model also suggests that when intermediaries are paid using a traditional percentage pricing scheme, firms tend to use them for their less skilled positions, sorting less skilled workers into indirectly hired positions. Finally, the model illustrates that intermediaries are more important in labor markets where the workers have more heterogenous skill levels. The policy implications of these findings are that removing the differences in legal protections between atypical workers and regular workers might not eliminate atypical employment or the sorting of low-skilled workers into atypical employment.

Up to this point, the dissertation treats organizations as actors contributing to labor market inequality. In contrast, the last paper looks at how organizational stratification is impacted by individuals' choices in the labor market. This paper uses network analysis to consider the relationship between universities' sociology departments' prestige rank (a

ranking developed from an index compiling various departmental attributes) and their positions in the sociology labor market. The position in the labor market is captured by a network connecting universities through professors' career moves. First, the analysis considers two important, but incomplete, explanations for the relationship between departmental prestige and network position: the fact that the most prestigious departments are larger in terms of faculty and that they provide the doctoral training for the vast majority of academic sociologists. However, the analysis shows that the centrality of departments in the employment network is a key predictor of academic prestige independent of these two factors. The structure of the employment network may result from a simple preference among professors to move to better ranked schools if they can or to remain in their current positions. The subsequent central position in the employment network can translate into an advantage in prestige through multiple mechanisms, thus contributing to static academic prestige rankings.

Taken alone, each paper addresses a very specific question, but taken together, they provide an overview of the labor market as a complex system with three main actors: countries, organizations, and individuals. Countries make several decisions that influence labor market inequality including their legal classification of workers, legislation protecting workers, and the design of social insurance programs. Organizations also make decisions influencing labor market inequality such as when to hire and fire workers and what type of contract to use. Workers, in turn, decide when to take a job, to leave a job, whether to take an atypical job, to organize, and to approach the courts to reclassify their contractual status. All of these decisions interact, changing conditions and outcomes for all three actors.

Even though this dissertation does not propose a complete policy strategy to address atypical employment, it does outline some policy directions. The first three papers suggest that governments need to remove a two-tier legal system for atypical and permanent workers and outline some legislative approaches as well as recent changes in union organizing and judicial intervention that influence atypical employment. The last paper has no pol-

icy conclusions beyond suggesting that some labor market dynamics are self-perpetuating and if, for example, a lower ranked academic department wished to alter the current conditions, a significant effort in resources, likely beyond that of the "top" schools, would be necessary.

Chapter 2

National Context and Atypical Employment

2.1 Introduction

"Atypical" employment is any type of employment that is not full-time and permanent with a single direct employer. It includes many diverse forms of work including part-time, self employment, fixed term contracts, temp work, free-lancing, piecework, unpaid family labor, and informal day labor. The label "atypical" lumps together arrangements that workers and employers choose for various reasons and with distinct consequences. This paper tests three hypotheses about how macro-context influences the total level of three types of atypical employment: fixed term, part-time, and self employment. Fixed term employment is similar to regular full-time work, with the exception that it has a specified end-date at the time of hire. In the United States, which has "employment at will," this is not theoretically different from regular employment, which can be terminated by both the employer and the employee at any time. Definitions of part-time work vary by country with thresholds normally between 30 and 35 hours a week. Workers working less than 10 hours per week are often considered "casual" workers rather than part-time. In the US part-time is employer-defined so that a worker working 38 hours a week in the US could be part-time, but in France the same worker is working overtime.

"Self employment" usually includes two distinct types of atypical employees: entrepreneurs and "dependent self employed" workers (free-lance or independent contractors who for all intents and purposes are employees although their contract is not an employment contract but a contract for services). In many studies the two groups are indistinguishable. Studies attempting to parse out the two groups have various estimates of how many workers are dependent self employed. In the UK, approximately 9% of the self employed (or 1.3% of the entire labor force) have no employees and only one purchaser of their services, likely dependent self employed workers. These workers are predominantly in construction, financial services, or skilled trades, are men, have less education than employees, and are likely to remain in positions as contractors (Böheim and Muehlberger, 2006). In Italy, estimates of the dependent self employed range from .88 to 5.3% of the labor force (Muehlberger and Pasqua, 2006; Alteri and Oteri, 2004), depending on the definition used. The dependent self employed in Italy differ from the average worker in that they are younger, more often single, more educated (in contrast to in other countries), in the service sector, and more often in Northern Italy. It is estimated that as many as 30% of Italian firms use these workers (Aris et al., 2001) and that most of these workers would prefer regular employment (Muehlberger and Pasqua, 2006). Their chances of transitioning to a standard job are higher the more they earn (Berton et al., 2005) and they are actually more likely to transition to unemployment than regular workers (Muehlberger and Pasqua, 2006). In Austria, the dependent self employed make up approximately 1.6% of the workforce (Heineck et al., 2004) and only 1% of the Greek workforce (EIRO, 2005). While the distinction between the two types of self employed workers is very important, this study is unable to distinguish between them. In surveys, workers generally self-report their status. Some dependent self employed (who generally commute to the same firm every day) misreport themselves to be employees (Bjelland et al., 2006). Part-time and fixed term workers are better able to correctly self-identify their status.

This paper proposes three main hypotheses regarding the macro level contexts influ-

encing the level of atypical employment. The first hypothesis is that firms might employ more atypical workers when permanent employment contracts are strictly regulated, the "free-market seeking hypothesis." The second hypothesis is that in a weak labor market, firms have more bargaining power and can successfully offer workers atypical jobs (which generally have lower salaries, benefits, and protections). Theoretically, a firm might also hire permanent workers at lower wages during downturns, although qualitative research suggests that there is less resentment when workers with different contracts receive different treatment (both better and worse) than when workers with the same contract receive different treatment. As such, it seems reasonable that when the market will bear lower wages, firms hire these new, lower paid, workers under atypical contracts. We call this the "constrained individual choices" hypothesis because this is when workers are forced to accept atypical employment, against their preferences, in a weak labor market. The third hypothesis, "entrepreneurial spirit," posits that workers prefer atypical employment when they have entrepreneurial goals. This is not only an individual-level cause, but it is also a macro-level hypothesis insofar as entrepreneurial motivation is time-culture specific. The entrepreneurial hypothesis is normally discussed in the context of self employment, although individuals starting their own businesses are likely to prefer to work in other forms of atypical employment as well. Finally, it is already a well-established fact that the proportion of women in the labor force strongly influences the overall level of part-time employment, and possibly other types of atypical employment, because women prefer flexible employment while raising children.

These three hypotheses are not mutually exclusive and can simultaneously influence atypical employment levels. For example, self employment might normally be driven by entrepreneurial spirit, but individuals might also chose self employment as a last resort in economies with high unemployment (constrained individual choices) or firms seeking to avoid regulations might use more independent contractors (free-market seeking hypothesis). Similarly, firms might seek flexibility from legal constraints by using less tightly

regulated part-time workers (free-market seeking hypothesis) but part-time work can also be a form of underemployment (constrained individual choices) and entrepreneurial spirit might also influence part-time work, as it enables individuals to start their own businesses in their free time. The primary motivation for fixed term employment is likely that firms seek flexibility by avoiding regulations (free-market seeking hypothesis), although again, workers preferring full-time work might be forced into these positions in a weak economy or workers with entrepreneurial aspirations (e.g. artists) might prefer fixed term assignments, working on their own projects between assignments.

This paper proceeds with a review of the literature on the causes of atypical employment, a description of current trends in atypical employment, followed by research design, findings, a discussion of policy trends related to atypical employment, and finally a conclusion discussing both the quantitative findings as well as policy implications.

2.2 Literature

The most commonly cited explanation for atypical employment is the free-market seeking hypothesis. Several authors suggest that countries' employment protection legislation (EPL) influences the incidence of fixed term and temporary work (OECD, 2003; Kalleberg, 2000b; Kahn, 2007). Often, using atypical workers allows firms to increase external flexibility and to extend screening periods in an environment where it is difficult to sever employment relationships (Kalleberg, 2000b).[1] Strict regulations can also encourage firms to shift towards internal flexibility (as has been shown in Germany (Keller and Seifert, 2005)), which would not affect atypical employment. Even in liberal labor markets like the United States, it has been shown that firms use atypical work to avoid legal constraints. For example, US federal tax code encourages firms to provide health insurance to all their employees by offering a tax deduction for firms offering a certain share of their workers

[1] "External flexibility" is adjusting labor inputs by hiring and firing workers in contrast to "internal flexibility" where firms adjust the workers' hours or switch workers' functions within the firm.

benefits. This encourages firms to buy services rather than hire employees for those positions for which they do not want to provide health insurance for, thus qualifying the firm for the tax deduction while still saving on non-wage compensation. Similarly, the post-1970 increase in part-time work in America may be partially attributed to increases in full-time benefit costs following the Federal and Family Leave Act of 1993 (Kalleberg, 2000b).

The constrained individual hypothesis is highly contested for all types of atypical employment except self employment. Some studies find that in a weak economy workers are forced into part-time, fixed term, and self employment (Grip et al., 1997; Blau, 1987) while others find that part-time employment does not increases in a bad market (Grip et al., 1997), and still others find an ambiguous relationship between economic conditions and atypical employment (Blanchflower, 2000). Grip and Basardi find that for couples, husbands' wages have no effect on women's decisions to work part-time, suggesting that, at least for women, economic constraints are not a consideration in choosing part-time work (Grip et al., 1997; Bardasi and Gornick, 2000). In contrast, assuming that boom times are accompanied by unexpected demand for labor, unemployment rates could have the opposite effect with firms more likely to use fixed term workers during *unanticipated* periods of high economic activity (Pfeifer, 2005). This hypothesis contradicts the constrained worker hypothesis, since it posits that in a booming economy firms seek atypical workers, increasing the share of atypical employment, while the constrained worker hypothesis posits that in a booming economy workers can pressure employers to offer permanent employment, decreasing the share of atypical employment. The uncertain relationship between economic conditions and atypical employment become clear in the next section, where we explore the high levels of part-time work in the economically robust Dutch labor market and the high levels of self employment in the weaker Greek labor market. Economic conditions can also impact workers in different ways—in a bad economy workers desiring regular jobs can be forced into atypical jobs, but in a good economy, workers can choose atypical employment to engage in other activities. In contrast to fixed term and part time work, self employment

has been consistently shown to be positively correlated with a weak economy.

The third hypothesis, entrepreneurial spirit, should be related to many types of atypical employment, as part-time, fixed term, and temp work all provide the flexibility for workers to start their own enterprizes while guaranteeing a secondary source of income. The concept of "entrepreneurial spirit" is hard to operationalize. The proportion of workers in self employment or the flows into self employment (or some combination of them, such as the TEA index) are the standard entrepreneurial variables (Chandler and Lyon, 2001; Gartner and Shane, 1995; Iversen et al., 2005). Of course, using self employment to operationalize entrepreneurship is not a solution when predicting self employment. Further, self employment is not really a direct measure of entrepreneurship since it includes independent contractors and casual workers (for example selling food off a street cart) who would rather be employed. While it is difficult to capture empirically, theoretically entrepreneurship should encourage other forms of atypical employment.

Some of the most important motivations for atypical employment happen at the individual-level. For example, age and gender are important determinants of self employment since older men are the most likely and able to start their own businesses (Blanchflower, 2000) and women with young children are more likely to work part-time (Carr, 1996; Grip et al., 1997; Bardasi and Gornick, 2000). Part-time work is also more common among the very old and very young during partial retirement or one's studies (Grip et al., 1997). Personal values and experiences are also important determinants of atypical employment and both religion (or the social support associated with it) and exposure to entrepreneurship in one's family are said to encourage entrepreneurship (Carrol and Mosakowski, 1987). Also, firm-level factors are important determinants of atypical employment; service sector firms and seasonal industries are more likely to use atypical workers (Grip et al., 1997; Kalleberg, 2000b).

2.3 Atypical employment

The OECD and Eurostat data used in this paper suggest that there is considerable variation in the level of atypical employment across countries, but that levels have been relatively stable and low since 1990. This contradicts several articles including the 2006 EU Green Paper on fixed term employment, which claims that up to 40% of the EU workforce was in atypical employment in 1995 (EU, 2006). The OECD and Eurostat data actually suggest that the biggest increases in atypical employment occurred in the 1980's. Of course, trends in atypical employment are particularly difficult to operationalize since they can vary depending on whether data are reported by employers or employees, by grouping together different types of atypical employment, and by analyzing shorter time periods, extrapolating from small blips in an otherwise stable trend (Grip et al., 1997; LeBlansch et al., 2000; Keller and Seifert, 2005; Magnani, 2003). Figure 2.1 shows the level of self, part-time, and fixed term employment in 2005 for 16 EU countries and the levels of self and part-time employment for the US, Canada, New Zealand, Switzerland, Japan, and Australia. There are three outliers, one for each type of atypical employment. Spain has very high fixed term employment, Greece (and to a lesser extent the other Mediterranean countries) have more self employment, and the Netherlands has more part-time employment. The US has less self and part-time employment than most other countries in the dataset.

The first panel of figure 2.2 shows the time trends for the average percent of the workforce in atypical employment across all countries illustrated in figure 2.1.[2] Part-time and fixed term work have increased slowly while self employment has declined. The second panel illustrates a few representative countries: UK (Anglo-Saxon), Czech Republic (former eastern bloc), Sweden (Nordic), and Italy (Mediterranean), as well as France and Ger-

[2]The average presented is just for Western Europe (Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom) and is weighted by population. Non-Western European countries have sparsely available time trends, so they are not included. Including all countries, the trends are mostly the same, but the self employment line shifts up. Using an unweighted average (where Luxembourg is equivalent to Germany) has a similar trend as what is depicted here.

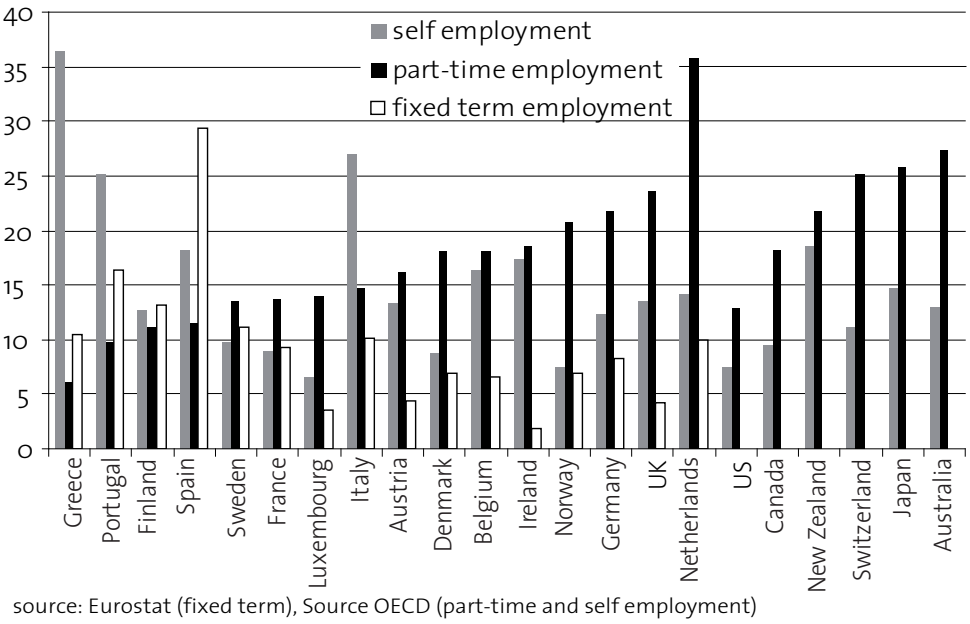

Figure 2.1: Levels of atypical employment by country, 2005

many. Across *all* countries (including those not illustrated in the graphic- see table 2.1 for a list of countries) from 1990 to 2006 self employment was relatively stable with higher levels in poorer countries and recent small declines in all countries except the former eastern-bloc countries like the Czech Republic and Romania. Fixed term employment increased slightly in Europe, with the exception of Ireland and Norway where it declined, and Poland and Portugal, where it grew rapidly. Part-time work has increased in most countries (particularly Germany) with the exception of Iceland and the United States.

Descriptions of the three extreme cases (fixed term employment in Spain, self employment in Greece, and part-time employment in the Netherlands) suggest which of the three hypotheses are relevant and whether the quantitative analysis will capture them.

Approximately 30% of Spain's workforce is in fixed term employment, about twice that of any other European country. The original growth in fixed term contracts (from 10 to 30% of the workforce) occurred in the 1980's and was the consequence of both labor market policies and economic conditions. Under Franco's regime, and in the first few years

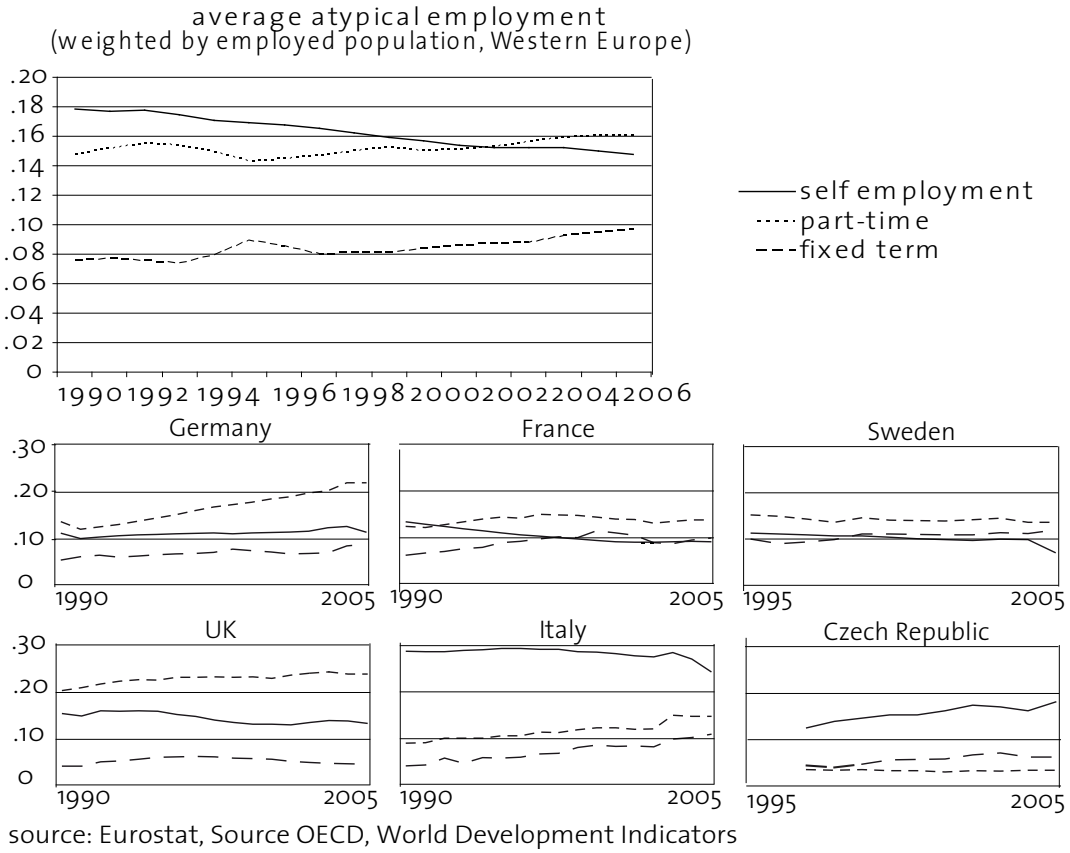

Figure 2.2: Trends in atypical employment

following it, employment policy was centralized and employment protection was strict. Employment policy was dominated by Instituto Nacional de Empleo (INEM), a central clearinghouse that matched jobs and workers, and managed unemployment benefits, vocational training programs, and employment records. Originally, unemployed workers and firms with vacancies were obliged to register with INEM although by 1980, 90 percent of vacancies were filled independently. Centralized administration and strong worker protections were liberalized in 1980 under the pressure of rising unemployment rates (Dolado et al., 2002). The "Ley Basica de Empleo" or " Ley del Estatuto de los Trabajadores" deregulated fixed term contracts, allowing them for temporary activities or as preliminary contracts for young workers. The law mandated equal wages for fixed term workers, reinforced private temporary work agencies' illegal status, and reaffirmed INEM's place as

the central placement organization. This legislation allowed firms the first legal means to circumvent strict employment regulations, while at the same time reinforcing most constraints. In another attempt to reduce unemployment, fixed term contracts were liberalized in 1984 under the Worker's Statute Reform which allowed firms to use fixed term workers for permanent activities and created a new form of contract that endured a minimum of 6 months, and was renewable up to 3 years. Under this contract, after three years the worker had to be either permanently hired or replaced with 12 days of severance pay. The final step towards liberalizing atypical employment was legalizing temporary work agencies under Royal Decree 18 (1993), although in fact, temporary work agencies already existed in practice. Strict limitations on temporary work agencies exist to this day, as they must be officially registered and authorized as non-profits and are generally run by local governments, unions, or employers' associations.

In the early 1990's, when it became apparent that the liberalization of fixed term contracts had divided the labor market into separate and unequal sectors, the government began to relax the strict EPL governing regular employment contracts and increased constraints on fixed term employment, equalizing their legal status. In 1992, the typical 6 month-3 year renewal contract was changed to a 1 year contract, again renewable up to a total of 3 years. In 1994, this contract was restricted to hard-to-employ workers including those over 45 years old and the long-term unemployed. Finally, in 1997 the contract was entirely eliminated. In 1997 and 1998, laws 8/1997, 63/1997, and 15/1998 made small adjustments to the difference in EPL for fixed term and permanent employees and finally in 2001, dismissal costs for fixed term workers were introduced (8 days per year of service) (Izquierdo et al., 2005). The most recent limitations on fixed term employment were passed in law number 43/2006, "Reforma Laboral," a direct response to the 1999 EU directive demanding limits on either the number of fixed term contract renewals or their cumulative duration (MTAS, 2006). This law requires fixed term contracts to be justified by the employer as "training" or fulfilling "short-term production needs" such as specific projects or replac-

ing employees on leave. The law specifies that contracts cannot endure beyond 2 contract cycles for a maximum of 24 months in a 30 month period, after which the worker automatically becomes a permanent employee. The reform also set tax benefits for firms converting fixed contracts to permanent ones, offering 850 euro for women, 1,200 euro for people over forty-five, 600 euro for the long-term unemployed, and 6,300 euro for the disabled with all bonuses annual and renewable for up to four years of employment, except the disabled bonus which endures indefinitely. According to the Spanish government, this legislation was successful: from 2005 to 2006, there was 108% growth in the rate of turnover from fixed to permanent contracts and in 2007 a full 42% of permanent contracts were initiated as indefinite contracts, compared to only 30.1% in December 2006 (MTAS, 2007b). This method can be strongly biased by the possibility that in recent years more people have started their jobs in fixed term employment. Correcting for this error Guell and Petrongolo (2007) finds there has been no increase in the hazard of transitions to permanent contracts.

In sum, Spain has come full circle, first supporting fixed term contracts as a solution to high unemployment, and then creating incentives for transitions to permanent employment after realizing they created a two-tier system of employment. Despite the policy reversal, fixed term contracts are still more common in Spain than elsewhere in Europe. While some (Toharia, 1999) argue that Spain naturally has a labor market with a core/periphery structure that lends itself to two-tier employment, it seems more likely that the high rate of fixed term contracts is a historical legal legacy of the earlier policies (Toharia, 1999; Casals, 2004; Royo, 2005; Amuedo-Dorantes and Serrano-Padial, 2005; MTAS, 2007a). The econometric analysis will partially capture this dynamic, measuring the strictness of regular and fixed term workers' employment protection, but it will fail to capture the historical legacy of the 1980's.

This policy reversal, first liberalizing atypical employment and then bringing its regulation closer to that of regular employment (either through making atypical EPL stricter or loosening permanent worker EPL) is typical, albeit normally not as dramatic as in the

Spanish case. For example, Germany, another country with strict EPL, liberalized atypical employment as an attempt to mitigate unemployment, legalizing temporary work agencies under the Loan Worker Employment Act (1972) and relaxing restrictions on fixed term employment in 1985. In 2000, the government tried to reverse course with the "Act of Part-time and Fixed Term Employment," which gave workers the right to switch to part-time work, required temp work to be used only for specific tasks, and limited the renewal of fixed term contracts to three years. In 2003, the Hartz Laws completed the reversal, increasing protections for atypical workers, forcing employers to pay health insurance and pension contributions for part-time workers (charging them an additional 2% wage tax), and promoting temporary work agencies as a *transition* to regular employment. As such, in Germany, as in Spain, there was a u-turn in policies, first promoting a two-tier system of employment as a solution to unemployment, and then attempting to equalize the two classes.

The second outlier in atypical employment is Greece, with 35% of its workforce self employed. Partly, the high self employment rate stems from the fact that the average firm size is only 2 employees compared to 6 in the EU (Mihail, 2003). Given that at least one worker in each small business is self employed, the predominance of small businesses should increase the proportion of self employed. In addition, Greece has strict EPL with high severance costs (higher for white collar than blue collar workers), strong minimum wage laws, and industry-wide collective agreements that all business owners in an industry must comply with regardless of whether they participated in negotiations (OECD, 2007; Kufidu and Mihail, 1999). This level of strict EPL and union power has been shown to encourage self employment (Cazes and Nesporova, 2003; Robson, 2003; OECD, 1999). In Greece, firms cannot circumvent strict EPL by using other forms of atypical employment since regulations on part-time, temporary, and fixed term work are also strict (Miaouli, 1998); self employment is the only way around the constraints. One caveat is that in practice small firms are able to circumvent EPL on overtime hours, dismissal policies, and to

negotiate pay and bonuses individually, in defiance of union contracts (Mihail, 2003; Kufidu and Mihail, 1999). Thus, these small firms do not need to resort to "dependent self employment."

For more than half of the period covered by this study, self employed workers were entirely free of the regulations governing both permanent and other atypical workers in Greece. This changed in August 1998 when the Law on Industrial Relations required that agreements between self employed persons and companies be reported to the ministry of labor within 15 days of the contract. If the contract is not registered, the relationship becomes that of regular employment in the eyes of the law (Kouzis, 2002). The goal of this initiative is to provide better estimates of how many self employed are actually self employed, as the self employment numbers prior to this law were exaggerated by employees masquerading as self employed. Since 1998 the courts have also enforced a more general definition of "employment." Currently, an employee is a worker who is subordinate, does not direct his or her work, does not determine his or her place of work or hours, and does not control his or her own performance. This new definition has been applied to reclassify dependent contractors as employees, giving them more protections, and thus equalizing their position with other workers. The full implications of reclassifying employees is elaborated on in the conclusion of this paper. There have been other moves towards closing the gap for contract workers. In March 2007, the Mediation and Arbitration Service demanded that any worker placed in a position of legal subordination to the employer has the right to be covered by the union contract. However, the relevant employer organization sought the reversal of this decision, which was not resolved at the time this paper was written. In sum, in the period covered by this study, free lance self employed workers were the only way around strict EPL although there have been recent attempts to remove this loophole.

There are several other factors contributing to Greece's high self employment rate. First, Greece has relatively high unemployment rates (around ten percent) and there is substantial evidence that self employment is positively correlated to unemployment, al-

though the direction of causality is contested (Rissman, 2003; Blanchflower, 2000; Audretsch et al., 2006). In addition, tourism, an important economic sector for Mediterranean countries, might present more opportunities for self employment. For example, Italy, recognizing tourism as a source of self employment opportunities, passed Act 236 in 1993, offering individuals financial aid and technical assistance to start their own tourism firms (OECD, 2000). The combination of EPL favoring self employment, relatively high unemployment, and a strong tourism sector all contribute to Greece's high incidence of self employment. The quantitative analysis will capture two of these three elements, missing tourism's possible contribution.

The third outlier is the Netherlands, which has an extremely high part-time employment rate, almost ten percentage points more than the next highest country, Australia. A full 66% of working women in the Netherlands work part-time compared with 30% in most EU countries; and the median employed woman works only 16 to 23 hours per week (Doorne-Huiskes, 2004). The high part-time employment rates seem to result from a combination of values, prosperity, and insufficient child care. In the Netherlands *both* men and women with children are more likely to reduce their working hours than other Europeans though married, less educated women with young children are the most likely to do so (Wel and Knijen, 2006). Further, part-time work is encouraged by legislation improving its standing relative to full-time work. In 1993 laws extended minimum wages and paid holidays to part-time workers working more than one-third normal hours, and in 1996 the provision was expanded to force full equality between all part-time and full-time work with prorated pay and benefits. Finally, in 2000, legislation allowed all workers to request the right to move between full and part-time work, requiring firms to accommodate these requests and to justify rejections. The government initially introduced legislation supporting part-time work when the country was experiencing high growth and needed to attract additional workers into the labor market (Plantenga, 1996). Unions supported the legislation to prevent part-time workers from becoming a cheap substitute (Rasmussen et al.,

2004). Further, child care is scarce and was not addressed by the government until the mid 2000's (Euwals, 2007), leaving part-time work as the primary option for working mothers. Surveys find that employed Dutch women actually prefer part-time employment, and more educated women prefer part-time work for *both* themselves and their partners (Wel and Knijen, 2006). Consequently, the Netherlands has one of the lowest *involuntary* part-time employment rates in Europe (Doorne-Huiskes, 2004). In sum, Dutch women prefer part-time work and the government encourages that preference through guaranteeing equal rights for part-time workers and by not putting a strong emphasis on child care needs. The quantitative analysis will find the relationship between the proportion of women in the marketplace and the high level of part-time work, the relationship between legislation and part-time work, and will suggest a weak negative relationship between economic constraints and part-time work, but will ignore both the importance of child care and cultural preferences.

For fixed term employment in Spain, self employment in Greece, and part-time employment in the Netherlands, it is clear that economic, legal, and cultural motivations are all at play. In Spain, fixed term work is primarily a legacy of legislative changes originally designed to combat high unemployment in the 1980's. In Greece, self employment is the result of the combination of high unemployment, strict EPL for both regular and atypical employment, and a strong tourism sector. Finally, in the Netherlands, part-time employment is the result of workers' preferences to balance family and work, economic prosperity, legal protections for part-time workers, and a limited supply of child care.

2.4 Research design

This study uses a series of predictors for atypical employment designed to capture the three hypotheses: "free-market seeking," "constrained individual," and "entrepreneurial spirit." The study also controls for the proportion of women in the labor force, a well-proven factor

in part-time employment rates, and possibly a factor in other atypical employment rates. Data are drawn from a variety of sources (see the appendix) but primarily rely on OECD, ILO, and Eurostat statistics.

The first two variables related to the "free market seeking" hypothesis measure the proportion of the workforce belonging to a union (union density) (Checchi and Lucifora, 2003; OECD, 1990-2008) and the total number of strikes and lockouts per 100,000 population (ILO, 2004; UN, 2007). Many studies also use union coverage (how many workers are affected by collective agreements) because in some countries, like France, many more workers are covered by contracts than belong to unions. The second union measure, the number of strikes and lockouts, is not as well standardized as union membership.[3] Union density ranges from 8 to 88% of the workforce in the OECD countries and has declined over time. Strikes and lockouts range from 0 to 25 per 100,000 population, with no discernable time trend. Most countries have few incidents (Austria, Canada, Czech Republic, Estonia, Hungary, Japan, Netherlands, Romania, Sweden, Switzerland, UK, US) or infrequent activity (Belgium, Cyprus, Finland, France, Greece, Ireland, Italy, New Zealand, Norway, Portugal, and Spain). Only Denmark, Iceland, and Poland had high strike rates during the 1995-2006 period while Denmark had more activity during the 1998 general strike, and again in 2002 when the public sector contracts were renegotiated. Strong unions could have various effects on atypical employment. On one hand, unions impose constraints that firms seek to avoid through atypical employment (thus increasing atypical employment) but on the other hand, union negotiations often include clauses explicitly limiting atypical employment. This point is revisited in the policy section, where some recent trends in collective bargaining and atypical employment are summarized.

[3] A description can be found at `http://laborsta.ilo.org/applv8/data/c9e.html`. The ILO data comes from a variety of sources including employers, conciliation services, and newspapers. The method for counting strikes and lockouts is inconsistent across countries with some countries counting incidents by disputes and others by affected employers. Some countries also include definitions of minimum countable incidents. For example, Denmark does not count incidents lasting less than 10 days and has no minimum number of workers per incident while the US does not count events involving less than 1,000 workers and lasting less than one full shift (before 1982 the minimum was 6 employees). Portugal, with a middling level of strikes and lockouts, has no minimum rules for counting a strike or lockout.

The next set of variables related to the free-market seeking hypothesis are two EPL indices published by OECD (1990-2008). The first index ranges from 0 to 6 and codes rules for dismissal notice and procedures, severance pay, and probationary hiring periods. A second index also ranging from 0 to 6 measures EPL for fixed term workers including: when fixed term work is allowed, the maximum number of contract renewals, and the maximum cumulative duration of renewed contracts. A third variable (which, of course, can only be used concurrently with 1 of the 2 EPL indices) takes the difference between these two indices and should capture the relative advantage of using atypical workers. According to these three variables, regular EPL is most liberal in the US, UK, and Switzerland, while it is strictest in Portugal, Sweden, and the Netherlands. Regular EPL is relatively constant over time, with the few countries that altered their laws generally liberalizing (Spain, Portugal, Finland, and Austria). EPL for fixed term workers varies more and is stricter in Belgium, Greece, Italy, Spain, and Portugal while more liberal in the US, UK, Canada, and Switzerland. Belgium, Italy, Sweden, Portugal, Spain, Germany, Norway, Netherlands and Denmark all liberalized their policies between 1990 and 2006. By 2006 most countries had relatively liberal policies with the exception of Italy, Spain, Portugal, and France. EPL can also vary by occupation; Austria, Belgium, Denmark, Greece, France, and Italy all have stricter protections for white collar workers while Germany and Spain recently equalized such disparities.[4]

In the past several years two EU directives on atypical employment have passed and a third was proposed. In 1997, Directive 97/81/EC outlawed discrimination against part-time workers, mandated pro-rated pay, required the elimination of laws limiting part-time work, and encouraged firms to hear requests to move from full to part-time work (or vice versa). In 1999, Directive 99/70/EC outlawed discrimination against fixed term workers, required employers to inform fixed term workers about permanent opportunities, and mandated that national governments pass legislation doing one or more of the following: 1) specifying the

[4]For other indices and discussion please see Deakin et al. (2007); Botero et al. (2004); WorldBank (2007).

circumstances under which fixed term contracts are permitted, 2) specifying the maximum total duration of renewed fixed term contracts and 3) limiting the number of permitted contract renewals. Finally, in 2002, Directive 0072 was proposed to prohibit discrimination against temp workers; to ensure temp workers have access to all workplace facilities, to require temp firms to pay workers for time between assignments; to ensure that temp workers receive overtime, breaks, and paid holidays; to require agencies to inform temp workers about permanent openings; to prohibit temp agencies from charging workers fees; and to encourage unions to negotiate on behalf of temp workers along with permanent employees. This directive never passed. The EU directives affect several countries in this analysis, although some argue that they (or at least the part time directive) are too weak and not specific enough to have an impact anyhow (Jefferey, 1998). Different implementations of the directives are considered in the policy section of the paper.

The final variable related to the free market seeking hypothesis is non-compensation costs, the proportion of the firm's cost of hiring an employee beyond wages (BLS, 2007b; ILO, 2002).[5] In practice, the variable captures different aspects of labor policy in different countries. The variable was originally included to capture the United State's employer-based health insurance system, a costly component of regular employees' compensation and consequently a widely cited reason for US firms to use atypical contracts. Despite high health insurance costs, the US does not have extremely high non-compensation costs compared to other OECD countries since firms' payroll taxes[6] are relatively low. "Non-compensation costs" is actually a grouping of many types legally required and optional benefits such as life insurance, retirement, disability, income guarantees, sick leave, accident insurance, and family allowances. The various benefits under this title only share the fact that they are non wage costs paid by the employer. Between low non-compensation

[5]Two sources were used for this variable, one using all workers and the other using only production workers (values were almost identical for those countries covered by both sources).

[6]The term "payroll tax" generally refers to both taxes withheld from employees' paychecks for programs such as social security and taxes (such as unemployment insurance) paid by employers that are directly linked to employing a worker. Of course, here we refer to those payroll taxes paid by the employer, not the worker.

costs and high health care costs, American non-compensation costs are a middling 20 percent of wages. Countries with the lowest non-compensation costs are New Zealand and Denmark, while Belgium, France, Italy, and Sweden all have high non-compensation costs. Most countries have stable non-compensation costs over time with the exception of Poland which dramatically reduced them in the mid-1990's. Non-compensation costs measure both incentives for individuals to stay in regular employment when benefits are tied to the worker-employer relationship (e.g. health insurance in the US) or incentives to take atypical jobs when benefits are not contingent on the worker-employer relationship (social security). Thus, the hypothesized overall effect of non-compensation costs is unclear.

There are three variables related to the second hypothesis, constrained individual choices. The first, unemployment rates, measures whether difficult labor market conditions might force workers into atypical jobs.[7] Unemployment rates vary widely within and between countries. Countries with relatively constant unemployment rates over time include Switzerland, the United States, Luxembourg, Norway and Austria, while Finland, Spain, Sweden, and Ireland all had high unemployment in the 1990's followed by a later recovery. The Eastern European countries showed a steady increase in unemployment rates over the entire period.

The second variable related to "constrained individual choices" is a measure of real wages which was constructed using mean manufacturing wages (BLS, 2007b) and adjusting them using PPP exchange rates (WorldBank, 1990-2005) and the CPI-U inflation index (BLS, 2007a) to convert them to real manufacturing wages in 2006 dollars. Real wages are stable across time for all countries with slow steady growth. The only exception to this is Norway, which shows some fluctuations in the late 1990's.[8] Theoretically, as workers' real wages increase, they should be able to withstand longer periods of unemployment and

[7] OECD data, standardized using ILO guidelines

[8] According to Johansen et al. (2007) Norway's manufacturing wages fluctuated based on the interaction between political party competition and the centralized wage bargaining institutions. The hypothesis is supported by the fact that the odd fluctuations in Norway's manufacturing wages correspond to a labor coalition's control of parliament from 1990 to 1997, 2000 to 2001, and in 2005.

be less pressed to accept atypical employment arrangements. On the other hand, as the Netherlands narrative demonstrated, individuals might be more eager to take flexible jobs in a more secure economic environment.

The final variable related to "individual constraints" is unemployment insurance replacement rates, averaging replacement rates across several unemployment scenarios.[9] Denmark and the Netherlands have the highest unemployment insurance replacement rates while the US and the UK have the lowest. Unemployment replacement rates changed in several countries during this period: Italy increased benefits as did Switzerland and Ireland to a lesser extent, while Denmark first increased and then decreased them. This variable should measure whether workers are pressured into taking atypical employment or if they have the luxury of taking their time to look for a permanent job.

The third hypothesis, "entrepreneurial spirit," is the most difficult to measure. Traditionally, researchers use self employment, entry into self employment, and the TEA index (a measure combining self employment stocks and flows) (Gartner and Shane, 1995; Iversen et al., 2005; Chandler and Lyon, 2001). According to the TEA, the US was entrepreneurial in the late 1990's but less so in the 2000's while Romania and Estonia show consistent increases, and Sweden, Finland, and Belgium have consistently low entrepreneurship. Self employment is the most common measure of entrepreneurship, but is also a poor one, as it can indicate of a weak, rather than an innovative, economy. A third measure, the patent application rate, was also tested in this analysis. Unfortunately, the patent application rate is dominated by corporate, not individual, filings and it includes foreign innovators seeking protection in the domestic market. In fact, in the United States about 46% of patent applications are by US corporations while only 13% are individuals and the remainder are government and foreign applications (USPTO, 2007). Patent ap-

[9]The measure was calculated by the OECD and is defined as the average of the gross unemployment benefit replacement rates for a worker with a full record of employment at two earnings levels (67% and 100% of average production worker earnings), in three family situations (single, married with dependent spouse, married with spouse in work) and for three unemployment spell durations (first year; second and third year; fourth and fifth year).

plication rates tend to be low and constant within countries with the exception of Cyprus, Ireland, Estonia, Luxembourg, and Slovenia, which all grew over the period.

The last independent variables are controls. The first is the proportion of the labor force that is women. All countries had progressively more women in the labor force from 1995 to 2006. Denmark, Iceland, and Sweden all approached 50% of the labor force by the end of the period, while Spain and Italy only reached 40%. The gini coefficient was also tested as a control, as was a dummy for EU membership.

Data are inconsistently available across years and countries. Self employment and part-time employment come from the OECD while fixed term employment comes from Eurostat, and is thus only available for Europe. Table 2.1 shows the availability of the three dependent variables by country and year. Descriptive statistics for all variables are in table A.1 in the appendix.

Correlations between independent variables are predominantly unrelated to one another as illustrated in table 2.2. All correlations take into account the panel data structure and are calculated as $((\beta_{x|y} * \beta_{y|x})^{.5})$ from two bivariate two-level regressions for each pair of variables. This is a simple back-door method since β is $\frac{cor_{xy}}{cor_x}$, so the product of the two beta's is simply $\frac{cor_{xy}^2}{cor_x * cor_y}$. Taking the square root gives the correlation coefficient, having adjusted for within country-correlation in the regression.

The different types of atypical employment have weak correlations, likely because they have distinct causes; fixed term employment is correlated with part-time and self employment with a Pearson correlation .15 and .084 respectively, while part-time and self employment have a .24 correlation. As such, each of the three types of atypical employment are treated as separate dependent variables.

In pooled time-series data, observations are correlated across years and countries can also be correlated spatially, violating ordinary least squares (OLS) assumptions. To illustrate this correlation structure, the residuals from three OLS regressions predicting the

	90	91	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06
Australia	o*	o*	o*	o*	o*	o*	o*	o*	o	o*	o*	o*	o*	o*	o*	o*	o*
Austria	*	*	*	*	*	o*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*
Belgium	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Canada	o*	o*	o*	o*	o*	o*	o*	o	o*	o	o*	o*	o*	o*	o*	o*	o*
Czech				o*	o*	o*	o*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*
Denmark	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Finland	o*	o*	o*	o*	o*	xo*	xo*	xo*	xo*	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*
France	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Germany	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Greece	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Hungary			*	*	*	o*	o*	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*
Iceland	o*	o*	o*	o*	o*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	x*	x*	x*	x*
Ireland	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Italy	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Japan	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*
Lithuania				*	*	*	*	*	x*	x*	x*	x*	x*	x*	x*	x*	x*
Luxembourg	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Netherlands	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
N Zealand	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*
Norway	o*	o*	o*	o*	o*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*
Poland	*	*	*	*	*	*	*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*
Portugal	xo*	xo	xo	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo*	xo	xo*	xo*
Romania			*	*	*	*	*	x*	x*	x*	x*	x*	x*	x*	x*	x*	x*
Slovakia					o*	o*	o*	o*	xo*	xo*	xo*	xo	xo*	xo	xo*	xo*	xo*
Slovenia				*	*	*	x*	x*	x*	x*	x*	x*	x*	x*	x*	x*	x*
Spain	xo*	xo*	xo*	xo	xo	xo	xo	xo	xo*	xo*	xo	xo	xo*	xo*	xo	xo*	xo*
Sweden	o*	o*	o*	o*	o*	xo	xo	xo	xo	xo	xo*	xo*	xo	xo	xo*	xo*	xo*
Switzerland	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*
UK	xo*	xo*	xo*	xo	xo	xo	xo	xo	xo	xo	xo	xo	xo	xo	xo	xo	xo*
US	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*	o*

x fixed term work
o part-time work
* self employment

Table 2.1: Dependent variable availability

	UD	SR	REPL	TEPL	DEPL	NCC	U	MW	UR	I	WW
union density	1										
strike/lockout rate	.073	1									
regular EPL	.036	.029	1								
short EPL	.14	.22	.081	1							
regular-short EPL	.14	.24	.24	.95	1						
non-compensation	.078	.080	.22	.091	.16	1					
unemployment	.19	.089	.044	.038	.016	.031	1				
manufacturing wage	.46	.21	.0042	.22	.20	.24	.21	1			
unemploy rep rate	.11	.0020	.091	.29	.25	.22	.051	.17	1		
innovation	.14	.037	.041	0	.0054	.20	0	.27	.073	1	
women working	.32	0	.27	.41	.31	.0026	.29	.34	.30	.29	1

Table 2.2: Correlations of independent variables

proportion of the workforce in each type of atypical employment were correlated across pairs of years within countries for each regression. This tested not just for correlation across adjacent years, but across all temporal lags. The scatter plot in figure 2.3 shows the correlation between residuals for a pair of years on the x axis (e.g. the correlation between self employment residuals for 1998 and for 1999) and the difference in years on the y axis

(1998-1999 = 1). Part-time work has a much stronger time correlation, and is correlated through all year lags.

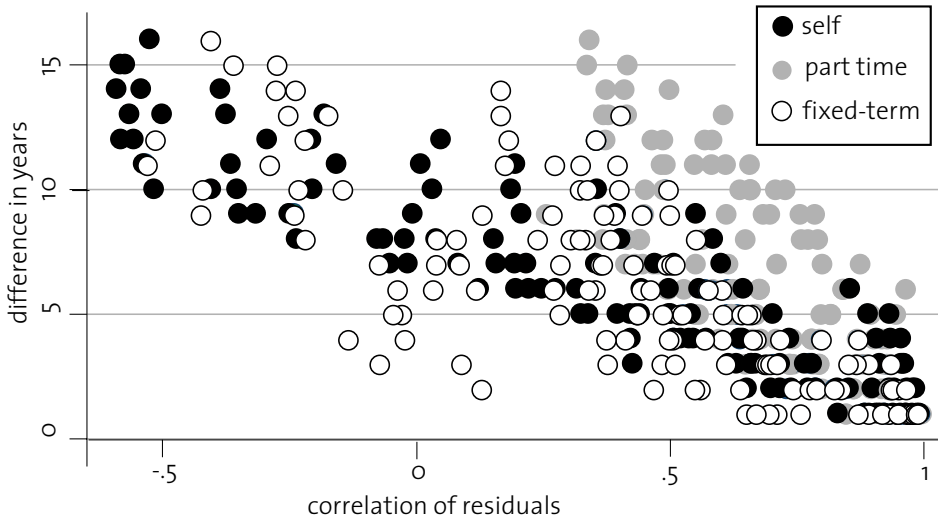

Figure 2.3: Correlation between residuals predicting atypical employment

There are several models that adjust for pooled time series correlation structures. First, there is a fixed effects model which transforms each variable into a comparison between the country-year and the country-specific mean (which is the equivalent of using country dummy variables) or in other words uses exclusively within-country variance. In equation 2.1, Y_{ij} is the level of atypical employment in country i in year j, $\overline{Y}_i$ is the average level of atypical employment in country i over all periods j, X_{ij} is the set of independent variables for each country i in year j, and $\overline{X}_i$ is the mean of the independent variable for each country. The model intercept is β and γ is the vector of parameters weighting the independent variables. While this model deals with the correlation across time within countries, it ignores the variance between countries when estimating the parameters. This is a significant loss of information, particularly for those variables that are relatively static within countries, such

as unemployment insurance replacement rates.

$$Y_{ij} - \overline{Y}_i = \beta + \gamma(X_{ij} - \overline{X}_i) + \varepsilon_{ij}, \tag{2.1}$$

In comparison, model 2.2 uses a random error component (ε_{ij}), an error component specific to the country (ε_i) and a country-specific intercept ($\beta_{\bar{i}}$), while the effects of all the independent variables X are assumed to be the same for all countries. In this model, ε_i and β_i are random parameters that are not estimated along with the fixed parameters, but their variance is estimated along with ε_{ij}'s variance. The model reduces the total number of parameters from the fixed effects model and uses the variation between countries, as well as within, to estimate parameters. Just as the country-specific error is assumed to be drawn from a normal distribution (random effects), the country-specific intercepts are also assumed to follow a normal distribution.

I also tested models with random coefficients for each of the independent variables in this model. This model assumes that the independent variables' coefficients vary by country (again drawn from a normal distribution).[10] For the temporary and self employment models, the simple random intercepts model in equation 2.2 was the best fit.

$$Y_{ij} = \beta_0 + \beta_{\bar{i}} + \gamma X + \varepsilon_{ij} + \varepsilon_i, \tag{2.2}$$

While atypical employment levels are correlated by country, it is also possible that they are correlated by time. For example, there could be a European recession, or perhaps organizational fads spread simultaneously. If this is the case, years are not only nested within countries, but countries are also nested within years, and a crossed random effects model

[10] Both testing random coefficients models, and then for additional confirmation, running individual models by country, the effects of the independent variables were found to be similar for all countries, and thus the intercept model was sufficient.

is necessary.[11] This model is illustrated in equation 2.3, where μ_i and ν_j are the random intercepts for years and countries. The random intercept for a given year is shared by all countries and the random intercept for an individual country is shared by all time observations within that country. As in the other models, X_{ij} indicates the independent variables, while Y_{ij} indicates the outcome variable, one of the three types of atypical employment.

$$Y_{ij} = \beta_0 + \gamma X_{ij} + \mu_i + \nu_j + \varepsilon_{ij}, \tag{2.3}$$

2.5 Results

Results from the fixed effects regressions show that the level of fixed term employment increases with union density, higher unemployment benefits, higher wages, and more women in the workforce. In terms of the hypotheses, this suggests some support for the "free market seeking" hypothesis, if firms avoid union-imposed constraints through fixed term contracts. There are interesting results for "individual constraints," as higher real wages and unemployment benefits are related to *more* fixed term contracts—perhaps because individuals are less fearful of facing periods of unemployment between contracts and therefore more willing to take fixed term jobs. The entrepreneurial variables have weak findings, which is unsurprising given the measurement problems. In table 2.3 σ_u and σ_e show the standard deviations of the residuals for the mean values for each country, and for the observations within each country while ρ indicates the fraction of the variance due to the country specific effect.[12]

Results from the fixed effects and random effects regressions show that part-time work

[11] Crossed random effects models were tested and found to be the best model for predicting fixed term employment. The crossed effects model was compared to the nested model using a likelihood ratio test. Fixed term employment is the only atypical employment that required the crossed effects model. It could be that fixed term employment rates result from international organizational trends as it is a relatively new form of atypical employment.

[12] Based on a Hausmann test I do not test a simple random effects model for fixed term contracts since the random effects are correlated with predictors.

	fixed term	pt employment		self employment	
	fe	fe	re	fe	re
entrepreneurship					
patent app rates	6.13 (.856)	-6.97 (.650)	-13.09 (.393)	21.9 (.049)	27.3 (.016)
self employment	.264 (.306)	.221 (.055)	.121 (.218)	-	-
employer constraints					
union density	.232 (.007)	.020 (.527)	-.029 (.261)	.021 (.368)	.006 (.772)
strike rates	-.124 (.107)	-.102 (.019)	-.109 (.012)	.036 (.253)	.029 (.374)
regular EPL	-1.30 (.461)	.138 (.868)	-.664 (.336)	1.97 (.001)	1.87 (.000)
reg-short EPL	.799 (.247)	-.521 (.105)	.045 (.873)	.390 (.072)	.323 (.122)
non-comp costs	-.140 (.500)	.111 (.291)	-.058 (.522)	-.219 (.003)	-.140 (.045)
worker constraints					
unemployment	.012 (.916)	-.037 (.518)	-.024 (.660)	.296 (.000)	.276 (.000)
unemp replacement	.161 (.007)	.028 (.307)	.007 (.801)	-.038 (.052)	-.034 (.085)
median income	.651 (.008)	.212 (.084)	.255 (.028)	-.237 (.007)	-.352 (.000)
controls					
women	.588 (.090)	1.15 (.084)	.878 (.000)	-.410 (.000)	-.541 (.000)
constant	-44.0 (.068)	-45.2 (.000)	24.3 (.008)	37.3 (.000)	45.0 (.000)
σ_u	11.61	7.43	4.86	7.14	4.61
σ_e	1.355	.797	.797	.584	.584
ρ	.987	.989	.974	.993	.984

() indicates the P value for the coefficient
σ_u indicates the sd of the estimated residual for mean country predictions
σ_e indicates the sd of the estimated residual for the within country predictions
ρ indicates the fraction of the variance due to countries

Table 2.3: Fixed and random effects coefficients

increases when there are fewer strikes and lockouts, when real wages are higher, and when there are more women in the workforce. A higher strike rate probably discourages part-time employment because hours are normally included in union negotiations. The result for wages confirms the fixed term employment results; countries with higher mean wages have more part-time work. Finally, there are tentative effects for self employment being related to higher part-time employment, perhaps lending some credence to the entrepreneurship hypothesis. As expected, the proportion of women in the work force plays a significant role in predicting part-time work. There is little support for the three main hypotheses in this regression since strikes seem to discourage part-time work and a high income encourages it. Rather, the statistics seem to reinforce the Netherlands' story: changes in part-time

employment are not driven by tight economic conditions or firms' desires to circumvent union and government regulation, but rather, by a prosperous environment with union representation, where women are free to choose part-time work.

Finally, the fixed and random effects regressions for self employment show that patent rates, stricter EPL, lower non-compensation costs, higher unemployment, lower unemployment benefits, lower wages, and fewer women in the workforce are all related to more self employment. This provides clear support for two hypotheses and mixed findings for the third. For "constrained individual choices," in a context of economic insecurity with low benefits and low wages, more individuals are self employed. With respect to the "free-market seeking hypothesis," in a context of strict EPL there is more self employment (perhaps dependent self employed) though non-compensation costs (conceived of as a constraint on employers) actually decrease self employment. This could be because higher non-compensation costs fund a more secure safety net for workers, measuring something akin to the unemployment benefits variable. In part, they also measure the amount of benefits a worker receives from an employer, which should encourage workers to prefer regular employment. While the patent application rate effect might lend additional support to the entrepreneurial hypothesis, we consider the results tentative given the measure's aforementioned faults and the weakly significant results.[13]

While random and fixed effects are two of the most common methods to deal with pooled time series data, there could also be correlations between countries by year or predictors could have different slopes for different countries, two possibilities ignored up to this point. Table 2.4 shows in the first column a crossed effects regression with all variables for each type of atypical employment. The second column illustrates the "best" model; these are not necessarily the "best" in a strict hierarchy of statistical tests. Because data

[13] Several models were run that are not presented here. These include several crossed effects models, interactions between EPL and unemployment, running the model with logit transforms of the atypical employment rates (because they vary between 0 and 1), and running effects by country groupings (Mediterranean vs. Anglo Saxon for example). Multiple diagnostics such as plots of quantiles of varname against quantiles of normal distributions, tests of the normality assumptions, and plots of predictions and residuals were run. The results are available upon request.

availability varied by variables, the most limited (like strikes and lockouts) were removed. Then several models were run with restricted samples including all remaining variables. These models were compared using a likelihood ratio test and once the significant variables were found, the model was rerun with the largest possible sample, omitting non-significant variables as necessary, to increase sample size by including observations for which the omitted variable was not available. Models tested include both crossed effects and nested models with various combinations of controls including an interaction term between EPL and unemployment rates, designed to capture the effect of concurrent firm demand for atypical workers along with individual willingness to accept atypical employment. (This is not displayed because it was not significant.) Crossed effects were found unnecessary for the part-time and self employment models as the two models' coefficients were almost identical and the change in log likelihood was negligible. In contrast, the regressions for fixed term employment improved dramatically using crossed effects.[14]

For self employment and fixed term employment, the crossed effects and best models' findings mirror the results from the fixed and random effects models except that the gap between regular and temporary EPL seemed to slightly decrease self employment in the crossed model. The regressions for part-time employment do not contradict the fixed effects and random effects regressions, but removing some insignificant variables that limited the sample size brought formerly insignificant predictors into the significant range.

The "entrepreneurial spirit" hypothesis fares surprisingly well in these analyses. Patent rates are positively and significantly associated with self employment, as expected, but negatively associated with part-time work. Self employment is positively related to both fixed term and part-time work, as expected. Patent application rates might be associated with lower levels of part-time work because patent applications inadvertently measure the

[14]The random effects for the self employment equation were not normally distributed. Rerunning the model using a transformed dependent variable, $ln(\frac{proportionself employed}{proportionnotself employed})$, still yields non normal random effects, as tested by the skewness kurtosis test, Shapiro-Wilk test, and plots of the random effect against the normal distribution. Thus the last column of the table, the best self employment model, uses Stata's glamm commands using country level effects and robust standard errors.

	fixed term		pt employment		self employment	
	full	best	full	best	full	best
countries/observations	14/123	15/234	20/181	21/330	10/183	24/369
Fixed Part						
entrepreneurship						
patent app rates	-2.24	-	-11.99	-59.37***	25.05*	59.17**
self employment	.38*	-	.15	.32***	-	-
employer constraints						
union density	.12*	.20***	-.02	-.082***	.01	-
strike rates	-.12	-	-.11**	-	.03	-
regular EPL	-1.94	-1.0	-.52	-2.12***	1.91***	2.22***
reg-short EPL	.91	.51 *	-.084	.15	.36	-.69**
non-comp costs	-.04	-.43***	-.017	-.13*	-.18**	-.05
worker constraints						
unemployment	-.02	.13**	-.025	-.05	.28***	.19***
unemp replacement	.15**	.042	.01	.038	-.036	-
median income	.44*	-	.24*	.48***	-.30***	-
controls						
women	.3943	-.1851	.9486***	.8750***	-.4781***	-1.2968***
EU member	3.70	-	-.76	-	.43	-
Random Part						
country(sd cons)	8.07	9.02	5.93	5.53	6.83	13.65
year	.00035	1.52	.00025	-	.044	-
residual variability sd	1.31	1.25	.776	1.09	.58	2.87
Log Likelihood	-248.21	-458.02	-273.45	-559.68	-274.22	-764.22

*** P= .001, ** P =.01, * P =.05

Table 2.4: Crossed effects and best regression coefficients

strength of certain economic sectors that hire more full-time workers. Also economies with high levels of self employment are found to have higher levels of part-time or fixed term employment. Presumably, this measures the cultural-entrepreneurship hypothesis.

There was mixed evidence for the "free-market seeking" hypothesis. First, union strength is related to more fixed term employment but to less part-time employment. This could result because working hours are included in union contracts (traditionally the case) but fixed term employment is not. This has changed in recent years; in the early 2000's union contracts began to limit fixed term employment, mandate fixed term workers' benefit levels, and even include clauses automatically converting fixed term workers to permanent posi-

tions (Campbell, 2005). As such, firms used to be able to escape union pressures through fixed term work but have never been able to do this through part-time employment. EPL, a more clear cut measure of the "free-market seeking" hypothesis, has the anticipated effect of increasing self employment and a wide gap between EPL for regular and fixed term workers is associated with more fixed term employment and less self-employment, as shown in the regular versus short term EPL gap row of table 2.4. Surprisingly, stricter EPL is associated with *less* part-time employment which makes sense when part-time workers are covered by regular EPL, as in the case of the Netherlands or following 1997 EU directive guaranteeing part-time workers the same rights as full-time workers.[15] Overall, it seems that firms in countries with strict EPL and unions use both self and fixed term employment to avoid constraints, while part-time workers are prevented from playing the same role by union contract provisions and recently added legal protections.

Higher non-compensation costs are consistently and strongly linked with *less* atypical employment. Originally, this variable was included to measure constraints on firms, but it also measures the general strength of the safety net including those employer-provided benefits that might tie workers to regular jobs. (Non-wage compensation costs can include insurance, pension, health insurance, occupation injury insurance, unemployment insurance, and family allowances.) In sum, the variable measures two opposing effects simultaneously: one binding workers to regular employment (e.g. health insurance in the US) and one offering a safety net that might make atypical work more feasible (e.g. unemployment insurance).

The most robust findings were for the "constrained individual choices" hypothesis. High unemployment rates are related to higher levels of fixed term and self employment while generous unemployment benefits also encourage fixed term employment, presumably because workers can bridge between assignments with public benefits. I hypothesized

[15]The negative relationship between EPL and part-time work is consistent over time, not increasing after the 1997 directive, when we test sub-periods. In addition, EPL's relationship to part-time work is not driven by the part-time outlier, the Netherlands, as the results hold excluding the Netherlands.

that an economy with high wages would have *less* of all forms of atypical employment, but in fact, wages are related to *more* fixed term and part-time employment. This is the relationship anticipated by the Netherlands story, where workers (particularly women) in a prosperous economy, willingly choose part-time work. In contrast, results show that in a more robust economy workers are *less* likely to become self employed. This confirms the literature we referred to at the beginning, showing that self employment is often employment of last resort, and that prosperous economies consequently have less self employment. Regressions over shorter time periods (not shown here) suggest that the relationship between a weak labor market and self employment strengthened in the post-2000 period.

Finally, I confirmed the literature, finding that the proportion of workers in part-time employment increases with more women in the workforce and that the proportion of women in the workforce is negatively correlated with self employment rates.

Figure 2.4 shows the time trends for predicted and actual levels of atypical work in a few countries. Overall the models do a relatively good job of predicting trends. For the most part, smaller fluctuations are captured by the model, although it misses the recent decline and then partial rebound of self employment in Greece as well as the US's level of part-time work.

Figure 2.5 breaks down the predictions for two countries for each type of atypical employment in 2006, focusing on the three outliers examined in the qualitative narrative. In the figure each coefficient from the "best" model column in table 2.4 are used in combination with the original data for each country in 2006 (i.e. the percent of women in the labor market etc). This represents a decomposition of some of prediction points in figure 2.4. The horizontal line indicates the sum of all components, leading to the overall prediction. Thus the model predicted for France that patent rates would contribute .-36, self employment 2.85, unions -.68, EPL -5.3, the difference in temp vs. regular EPL -.165, non-compensation costs -4.05 and so on, summing to a total prediction of 12.17% part-time employment. The number at the bottom of each bar indicates the actual atypi-

cal employment level; in this case, 13.6% of French employment in 2006 was part-time. While the model overall does a good job of predicting atypical employment, the random country effects are a big part of that prediction. While the variables of interest are statistically significant, they contribute significantly less than the predicted country random effects. From the narrative we might have expected the Netherlands' part-time prediction to be more driven by the women in the labor market and the high income, but, in fact, the women and income variables play a very similar role in predicting France's part-time employment rate. For fixed term employment, EPL plays a slightly larger role in predicting Spain's fixed term employment rate than the UK's, but fails to capture the dramatic roll we would expect. For self employment there is no visible difference between the two cases displayed beyond the country effects and the proportion of women in the labor force. This graphic draws our attention to how we need to look beyond the model's statistical significance. While the model has statistically significant effects in the expected directions and accurately predicts the countries' trends, it largely does so through countries' random effects, and very little through the explanatory variables of interest. While the magnitude of the fixed part of the regressions' effects is not large, it was still statistically significant. In this sense, the quantitative analysis did find significant results, but at the same time, our predictions suggest that the model did not capture the same information as our historical narratives of Spain, the Netherlands, and Greece.

2.6 Atypical employment policy

The OECD's EPL index used in the analysis focuses on dismissal and job protection, but there are many ways beyond job security that atypical workers are disadvantaged. For example, they are often not part of unions, have weaker worker safety protections, no overtime pay, and sometimes no minimum wage. This situation is being addressed by governments in a piecemeal fashion, first determining where these workers are vulnerable and then im-

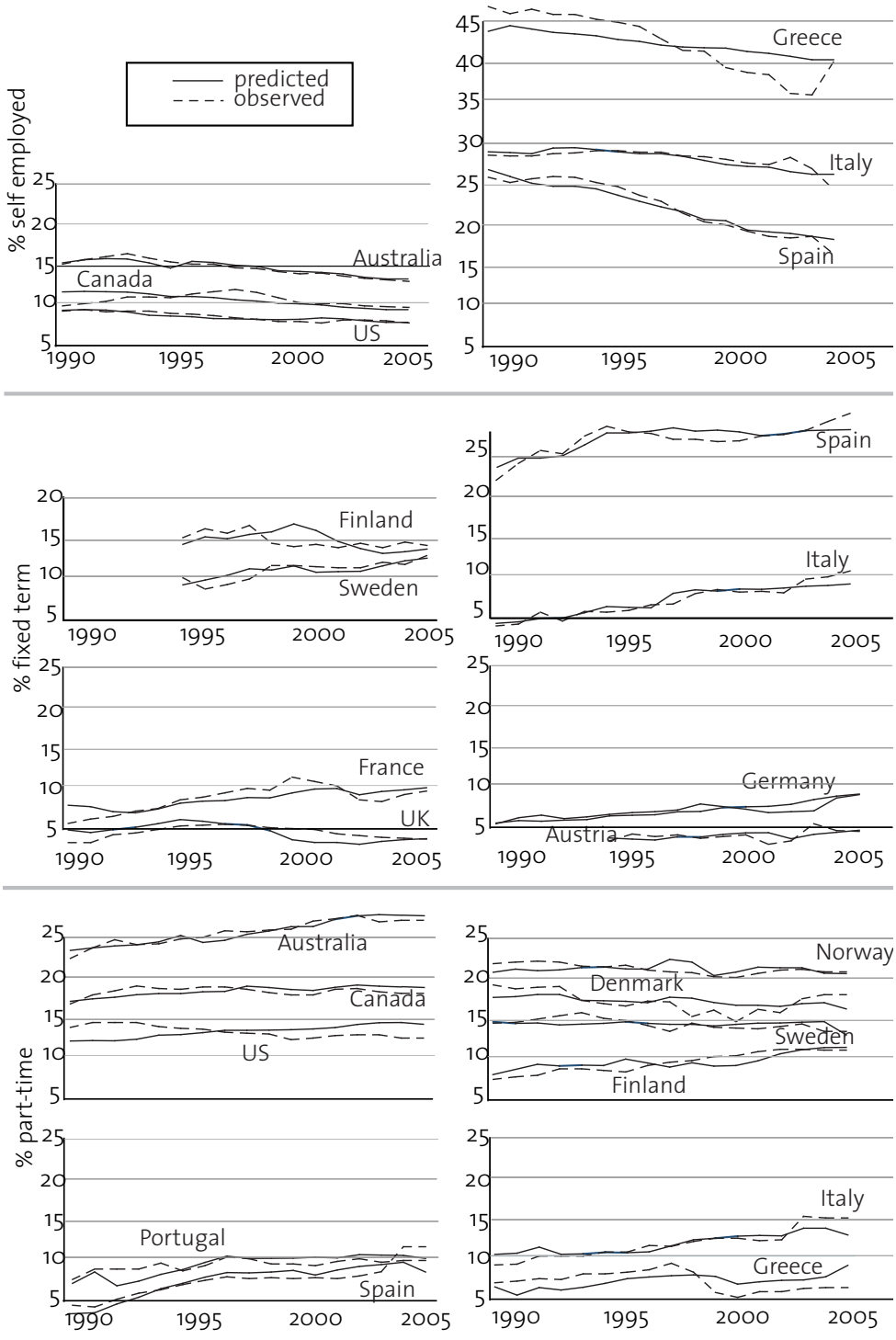

Figure 2.4: Predicted versus observed atypical employment

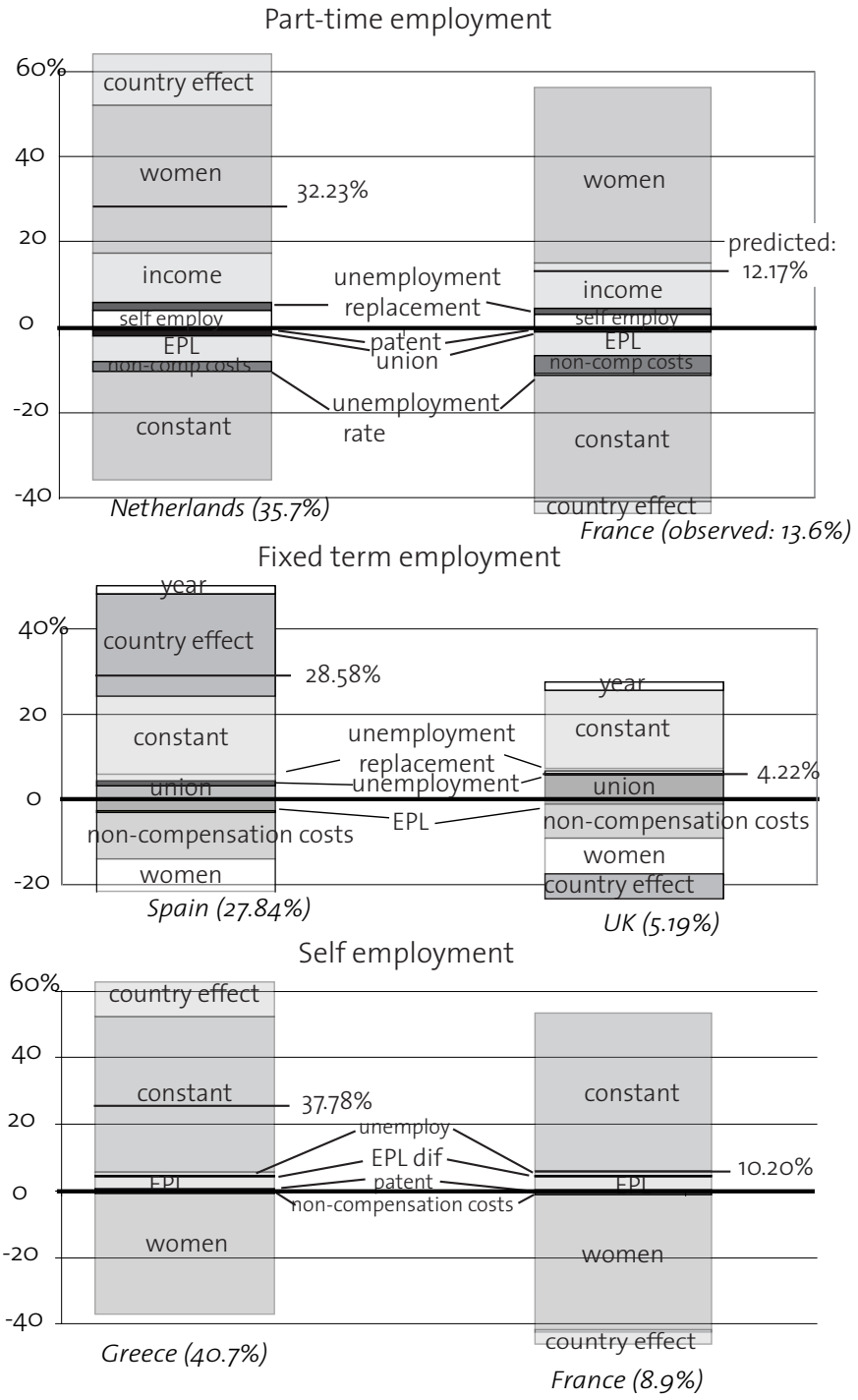

Figure 2.5: Decomposing predicted atypical employment for 6 cases in 2006

proving their status through legislation, the courts, or unions. This section starts with a comparison of diverse legislative, judicial, and union approaches to regulating atypical employment and concludes with a comparison of the different approaches.

In the United States several protections such as the US Fair Standards Labor Act (guaranteeing minimum wage and basic workers' rights) or OSHA (Occupational Safety and Health) apply to all "employees." The Fair Standards Labor Act covers temps, fixed term, and part-time workers, but can exclude independent contractors. The exclusion of independent contractors is addressed through the courts which determine whether a worker should have the status of "employee" (covered in detail in the next section). Atypical workers who are "employees" still do not qualify for some other protections and benefits. For example, the Family and Medical Leave Act only applies to employers over some threshold in size and requires workers to have worked for the employer 1250 hours in the last 12 months; thus many atypical workers do not qualify. Similarly, unemployment insurance has varying eligibility requirements (by state) in terms of hours worked under an employer and minimum earnings to be covered, so atypical workers are, again, less likely to be covered. Furthermore, only laid off workers are eligible for unemployment insurance and in many states workers finishing a fixed term contract or in between temp assignments are not considered to be "laid off." In sum, atypical workers miss out on the greater portion of the social safety net that was designed with a traditional employee-employer relationship in mind (Stone, 2006). Which workers are left out of which social scheme varies across countries, depending on whether the benefit is issued through the employer, state, or unions, the worker's relationship to those parties (status as employee or as union member), and on eligibility requirements. There are currently three broad approaches to improving work conditions for atypical workers: legislative, judicial, and unions.

The first legislative approach to improving atypical workers' rights is often called the "flexicurity" model. This approach de-links workers' benefits and the safety net from the employee-employer relationship and replaces it with benefits and a safety net coming di-

rectly from the government. While analysts discuss the "Danish flexicurity model" as a unified approach of weak limits on firms and extensive worker protections, it is actually not one clear or consistent policy and Denmark has had several changes in their policies in the past few years (Zhou, 2007). Today Denmark has a low payroll tax burden on businesses, meaning that only 10.3% (2005) of firms' compensation costs were not wages compared to 22.7% in Finland or 31.2% in France (BLS, 2007b).[16] While there is no "at will" employment, as in the US, dismissal notices are significantly shorter than in much of Europe, reaching a maximum of 6 months notice. Workers also receive generous unemployment benefits with replacement rates on average 50% of prior salary, as measured by the OECD.[17] This is the highest unemployment insurance replacement rate among the OECD countries (with the exception of the Netherlands) with 53%; the next highest countries, Belgium and Portugal, have much lower rates around 42 and 41%. The generous Danish program has actually become more restricted in recent years, reducing unemployment insurance maximum benefit periods from 5 to 4 years, and requiring unemployment insurance recipients to accept job offers or to enter retraining schemes. Denmark also spends significantly more per person on retraining. The downside of the Danish system is that it is expensive. In 2004 before a recent tax cut, average income tax rates could reach 59.1%, among the highest in Europe (Denmark, 2004). The positive side of a high and progressive income tax and strong safety net is that Denmark is very equal, with a gini coefficient of only .24 in 2005.

This "flexicurity" model is currently a popular one among analysts because the Danish labor market is doing extremely well with an unemployment rate under 5% and some of the highest salaries in Europe at $26 per hour (2006 PPP); only workers in Belgium, Austria, Finland, Germany, the Netherlands, and Norway earn equivalent or better wages. However,

[16]These numbers might not be exact- the ILO reports 6.3% in 1999, the same year that the BLS reports 7.5%. Regardless, according to all sources, the tax burden is low.

[17]This is the average of the gross unemployment benefit replacement rates for a worker with a full record of employment at two earnings levels (67% and 100% of average production worker earnings), three family situations (single, married with dependent spouse, married with spouse in work) and three unemployment spell durations (first year; second and third year; fourth and fifth year.)

the causal relationship between flexicurity and Danish success is uncertain; there are many alternative explanations for the high wages, such as 74% union membership (OECD, 2003) and 82% union coverage (both in 2000) (ILO, 2004). Further, the flexicurity system does not entirely remove incentives for atypical employment. In 2005 Denmark still had 18% of its workforce in part-time jobs, 9% in fixed term, 9% self employed, and 6.5% in temp work (OECD, 1990-2008; Eurostat, 2007).

The second legislative approach is to patch the leaks in the current social safety net that are breached by atypical employment contracts. The European Union as a whole has taken this approach, in particular with the two aforementioned directives regarding part-time and fixed term work. Directive 1999/70/EC on fixed term employment did not set forth specific requirements for countries, but developed a framework that countries could use to limit fixed term work. The directive requires countries to specify who is covered by a law limiting fixed term contracts (i.e. individuals in apprenticeships might be excluded), to specify whether employers must justify fixed term contracts (i.e. to replace a specific individual on leave), to specify the maximum number of contract renewals possible and the total maximum duration of those contracts, and possibly the total period (including the time in the fixed term contract) that must pass before a worker could be rehired in the same fixed term position. All EU states responded to the directive (including several applicant countries) with diverse implementations. Table 2.5 lists implementation by country. Several countries excluded public sector workers or trainees from the limitations on fixed term contracts while others, like Italy, have seemingly arbitrary exclusions like workers in catering. Many countries also exempted workers from the law as long as the contract is justified. This strips the law of its power insofar as firms can always find plausible justifications to infinitely renew contracts. Similarly, Denmark applied the law only to workers not under collective agreements. With over 82% of their workforce covered by collective agreements, this clause makes the law inconsequential. Most of the implementations are relatively weak, such as Sweden's limitation to a maximum of three years of contract

renewals within five years. However, other countries such as Austria took the directive as an opportunity to implement a strict limit of three months on renewed fixed term contracts. Note that the Netherlands entry (which might be confusing in the table) says that any series of contracts enduring more than 36 months or renewed more than three times is reclassified as permanent unless breaks of at least 3 months in length occurred between contracts. Overall, one might say that the countries followed their legislative inclinations before the directive; the highly regulated countries limited atypical employment and free market countries minimized the law's impact.

Directive 97/81/EC on part-time work had more specific requirements. The directive required pro rata compensation for part-time workers, a non-discrimination law, and required that firms facilitate switches to and from part-time work. Few European countries varied implementation. Some countries like Austria, Belgium, and Greece excluded public workers while Denmark excluded public sector seasonal workers; Finland excluded workers in tourism, and the UK excluded judicial and armed forces workers. The implementation of the anti-discrimination clause can vary from a general affirmation of equality to Austria's requirement that the employer has the burden of proof when a worker complains about discrimination. Some of the laws add special provisions such as Austria's promotion of part-time work for employees taking care of elderly parents or Belgium's decision to integrate it into parental leave and retirement policy. Denmark denied the original law and refused to guarantee workers a right to part-time work because it is already included in collective agreement negotiations. In France, part-time policy is also a part of family policy, where requests for part-time work should be justified by family reasons. Finland requires any new tasks in the organization to be offered to part-time workers before new workers are hired. Table 2.6 shows for EU countries the adjustment to the baseline directive and for non-EU countries, their relevant policies for part-time work.

The second means to insure atypical workers' rights is through unions and collective bargaining. Atypical workers are difficult to organize because employers often do not have

	scope	justification necessary?	allowed renewals	max contract duration	max cumulative duration
Austria	excl. public	yes	1	-	3 mo
Belgium	all	if so, no limit	4	3/6 mo	2/3 yrs
Denmark	excl. unionized	no	0	-	-
Finland	excl. public	yes	court determined "abuse"		
France	all	yes	1	9/18/24 mo	50% contract length break
Germany	excl. public	if so, no limit	3	2 yrs	2 yrs
Greece	all	if so, no limit	3	2 yrs	2 yrs
Ireland	excl. training	if so, no limit	after 3 yrs, 1	4 yrs	4 yrs
Italy	many industry exceptions	by industry if so, no limit	1	3 yrs	3 yrs
Luxembourg	excl. public, edu, & religious sectors	yes	2	2 yrs	2 yrs
Netherlands	excl. public	no	unlimited/ 3	3 mo bet contracts	36 mo/ unlimited
Portugal	excl. public	yes	2	6 yrs	33% contract
	& other sectors	yes	2	6 yrs	length break
Spain	all	yes	no limits if collective agreement		
				6 mo	12 mo
Sweden	excl. managers & family workers	yes	-	3 yrs	5yrs
UK	excl. training	no	-	4 yrs	4 yrs
Czech	excl. disabled, minors, training	no	-	2 yrs	2 yrs
Hungary	all	yes	-	5 yrs	5 yrs
Iceland	excl. managers	no	-	2 yrs	2 yrs
Lithuania	all	yes unless unionized	-	5 yrs	5 yrs
Poland	all	if so, no limit	2	-	1 mo break
Slovakia	excl. small firms	for renewals	-	3 yrs	3 yrs
Slovenia	all	yes	-	3 yrs	3 yrs

Details excluded (i.e. Denmark's weaker restrictions for universities)
source:Commission of the European Communities CEC (2006); EIRO (2005); ILO (2008)

Table 2.5: Fixed term employment legislation

	scope	provisions (beyond EU standard for EU)
Austria	excl. public, managers, homeworkers	allows older workers to reduce hours; employer has burden of proof in PT discrimination case
Belgium	all	bargaining rights, paid training leave, workers have the right to switch PT/FT
Denmark	excl. seasonal public	union responsible for bargaining
Finland	excl. casual workers	must offer PT more work when available
France	all/ appeals allowed	right to leave & vocational training
Germany	all	right to switch PT/FT
Greece	excl. public	must inform unions as PT:FT ratio changes
Ireland	excl. casual	none
Italy	all	right to training & must inform unions as PT:FT ratio changes
Luxembourg	all	PT for parents guaranteed PT promoted as partial retirement
Netherlands	excl. defense workers	EU directive only
Portugal	public and private	EU directive only
Spain	excl. skilled & managers	access to PT work
Sweden	all	right to PT work for parents
UK	excl. judicial, military	EU directive only
Australia	$<$ 35 hrs/week	pro-rated wage & sick leave, 4 wks holiday
Canada	$<$ 40 hrs/week	no longer required to offer PT hrs if available not entitled to paid holiday or vacation
Czech*	$<$ 40 hrs/week	mothers' and caretakers' right to switch to PT
Hungary	all	EU directive only
Iceland	all	right to take leave in summer months
Japan	all	minimum daily wages & proportional leave
Slovakia	$<$ 40 hrs/week	same protections as FT
Switzerland	$<$ 40 hrs/week	same protections as FT
US	employer-defined	OSHA and Fair Standards Labor Act apply, no anti-discrimination, pro rated benefits, or right to PT work

*ILOEX contradicts EU source stating that Czech mothers have no right to PT work, but division 3 section 241 of the Czech legal code at `http://www.mpsv.cz/files/clanky/3221/labour_code.pdf` says Czech women with children under 15 or pregnant have the right to PT work.

Table 2.6: Part-time employment legislation

the legal obligation to negotiate with them, because they often change positions and employers, and because their legal employer could be a firm they have never gone to or that they might not even be aware is their employer. In America, atypical workers' rights with respect to unions have vacillated. A 2004 ruling on the National Labor Relations Act in 2004 found that temp workers cannot organize alongside direct hires although they can organize to negotiate with the temp firm. At the same time, on the ground, temps were making progress through organizing and the judicial system. In response to an IRS suit, Microsoft was forced to reclassify their independent contractors. While the ruling's intent was that the workers be reclassified as employees, Microsoft reacted by reclassifying many of them as temps. In response, the workers successfully organized the Washington Alliance of Technology Workers. While Microsoft is not legally obliged to negotiate with the union, the temps have nevertheless made some headway with respect to wages and access to facilities, although not with respect to stock options. Microsoft is a special case where the workers are well educated and remained at the (indirect) employer for an extended period, making collective action easier. Overall, the courts support atypical workers' rights to organize but do not usually require the firm to negotiate with them (Jaarsveld, 2006). Consequently, there are many efforts by established unions like the AFL-CIO to organize atypical workers as well as smaller unions like the LA Service Employees International Union which advocates for home workers who are independent contractors, the Chicago Coalition for the Homeless which advocates for temps and day laborers, and the Freelancers Union which offers free-lancers insurance and advocacy.

Efforts to unionize atypical workers have made more progress in other countries where unions have more power. In 1998, in Italy, the three main Italian trade union confederations, Cgil, Cisl and Uil, set up specific workers' organizations to represent atypical workers: the New Labor Identities (Nuove Identit di Lavoro), the Association of Atypical and Temporary Agency workers (Associazione Lavoratori Atipici e Interinali, Alai-Cisl), and the Committee for Employment (Comitato per L'occupazione). These organizations

have campaigned for the definition of precise juridical regulations and for a clear definition of the field of work and rights of workers employed on atypical contracts. In particular, they ask for rules regulating individual contracts and for the identification of professional and sectoral profiles, tools to stabilize precarious situations in the labor market, training, certification, and access to credit, insurance, and supplementary pension. In recent years, these trade unions bargained at the national, company and local levels. In addition, recently unions in the UK representing journalists, actors, and construction workers have started to organize independent contractors (Böheim and Muehlberger, 2006). Union organization is, in some sense, partly judicial action, as courts must grant them the right to organize before the next steps can be taken.

The other way courts are engaged in protecting atypical workers' rights is that they determine whether workers are "employees." The debate about whether independent contractors might also be employees has been raging for over forty years since the term "dependent contractors" was developed (Langille and Davidov, 2000) and has continued to this day including the ILO's 2003 conference on "The Scope of the Employment Relationship." The classification of a worker as self employed or an employee controls workers' access to an array of rights such as liability for on-the-job accidents and retirement benefits.

In Canada "employees" are protected by the Employment Standards Act (guaranteeing paid vacations, equal pay for equal work, parental leave, and a maximum hours of work), the Labor Relations Acts (right to collective bargaining), and employees have the right to dismissal notice under common law. Independent contractors have none of these. The courts use multiple criteria to determine whether a worker is an employee including: the worker's "control" over his work, economic independence (risk and chance of profit), the duration of the employment relationship, reliance on the employer, the exclusivity of the relationship, the right to use substitutes, ownership of tools, the freedom to reject job opportunities, fee variation, integration into the organization, the degree of specialization or skill, and whether more than 80% of the worker's income comes from a single firm.

The courts have often reclassified workers (for example, redesignating salesmen working on commission as employees) although owner-operated trucking has had mixed success (Langille and Davidov, 2000).

In Belgium, reclassification cases are often pursued by the social security administration (which loses revenue from misclassification) and have been supported based on the criterion of: exclusivity of the employment relationship, limitations on competitive activities, terms of notice that are similar to those in an employment relationship, and obligations for particular individuals to personally perform the services, and earning profit from others' work. Reclassification takes place for both high and low-skill jobs although in Belgium high earners are often better off classified as non-employees because their social security payments to the government are lower than they would be as employees (Engels, 2000).

In Germany, independent contractors (compared to employees) get no sick pay (vs. 6 weeks), no paid holidays (vs. 4-6 weeks), no unjust dismissal protection, and no social security contributions. Until 1999 Germany used the following criteria to define "employees": whether the contractor's tasks are identical to employees', control over the time and place of work, number of clients, and bearing entrepreneurial risk. Meeting one criterion was not sufficient; for example courts considered those with only one client but able to set their own hours to be independent contractors. In 1999 a more rigid legislative approach was tested, classifying workers as employees if two of the following applied: labor is performed within one individual or family, the contract is regular and for one customer, tasks are normally performed by employees, and the worker has no direct contact with the market. This legislation was rapidly revoked, returning to a more flexible court-enforced approach with a new category of workers: "worker-like persons," who have the right to sick and vacation leave and collective bargaining, but not social security. The courts are often inconsistent in their judgments with one court deeming pharmaceutical representatives "employees" because the number of customers the worker visits is controlled through reporting, while another court found the same workers to be a independent contractors

because they could chose their customers (Däubler, 2000).

In Japan the benefits of being an employee rather than a contractor include worker's compensation, severance pay, and protection from unfair dismissal. As in Germany, the courts have developed a new "mixed contract" in which a worker is protected from unfair dismissal and can receive workman's comp. This category has been applied to designers working at a firm who were paid per design rather than per hour. The courts use a test that rests on the idea of worker subordination and uses the following criteria: whether the worker can refuse work or bargain, the strength and degree of supervisory direction, whether the worker can substitute another person's services for his own, whether the worker owns his own equipment, whether the worker earns more than employees doing similar work, the exclusivity of the worker-firm relationship, how similar the hiring process is to that for employees, disciplinary rules, and the application of provisions regarding allowances and fringe benefits. Japanese courts are inconsistent in their application of these rules, finding that owner-operator truck drivers were independent contractors, carpenters working on-site during standard working hours were reclassified as employees of a construction firm, scientists working on-site and supervised were also reclassified as employees, as were teachers, doctors, engineers, and computer scientists (Ramakawa, 2000).

In the Netherlands independent contractors are covered under some of the regular employee protections like rules about safety and protections, sick and vacation pay, protection for the equal treatment of women, and probationary work periods, as well as social security. However, like in the other countries where independent contractors go to the court to be reclassified, Dutch workers must pass a "dependency test". Criteria include: whether the employer controls work hours and location; whether the worker can substitute another worker to complete the task; whether the employer is paying wages; and whether the worker is required to perform the labor during a certain period. While these criteria are similar to those listed in other countries, in the Netherlands the burden of proof is on the employer. Independent contractors who were awarded employee rights include: club dancers, an Imam

(versus his mosque), a manufacturing home worker, a shopkeeper working in a laundromat, and a schoolboy working for a bulb grower. Employer arguments were diverse from the inability for a secular institution to oversee religious work (the Imam) to the child who was supposedly not an employee because he could come and go as he pleased. These cases are primarily brought to court by the National Institute for Social Insurance, which loses revenue when workers are classified as independent contractors (Peijpe, 2000)

In Sweden the distinction is in part controlled by the courts, the tax agency, and unions. The label of "employee" determines whether the worker is protected by workman's compensation, and whether he or she has vacation, employment security, and benefits. Criterion used by the courts include: a personal duty to perform the work, predetermined work tasks, the length of the relationship, whether the laborer can work for other parties, whether the mode of performing work including the time and place of work are controlled, who provides work equipment, whether the laborer is reimbursed for expenses (like travel), who bears economic risk, and the relative economic and social condition of the worker and employees. Again, the case history is very mixed. In two separate cases hairdressers renting out seats in salons sought to be reclassified as employees. The case of the recently trained apprentice was reclassified as an "employee," while that of a more experienced hairdresser was rejected because the worker brought their own clients. Many of the cases that have been brought to the courts and reclassified seem almost trivial such as a child taking care of her parents' dog, parent volunteers at a nursery (won), and foster parents seeking employee status (lost). Unlike in the other countries, it seems that multiple agents turn to the courts, not merely executive agencies seeking lost revenue (Källström, 2000).

Finally, in the United States the self employed worker pays and receives social security, but is not covered by the Fair Labor Standards Act and does not have health insurance, vacation time, or sick time. The litmus test in the courts for determining whether a worker is an "employee" is that of "control" over the worker. The definition of "control" has not been clearly sorted out, with successful claims for reclassification for shochtim (kosher

slaughterers) who worked alongside regular workers in a slaughterhouse (the firm claimed they could not supervise worker working under Jewish religious laws they did not understand) and Microsoft programmers. Unsuccessful claims include: cab drivers (they control their workplace conditions), grocery store baggers (they have control over whether they come to work), Mexican mine workers paid daily rates and housed in the mining facilities, individuals installing floors for a flooring company, nude dancers (they just rent floor space), landscapers, cleaners, waiters for a catering service, and oyster shuckers for a fishing company. While the courts and legislature have been generally supportive of business, the IRS aggressively sought to reclassify workers since it loses revenue from misclassification. However, in response, the legislature passed laws minimizing firm penalties from IRS reclassification (Linder, 2000).

There are strong similarities across the countries reviewed here. In all cases the courts have the right to reclassify workers as employees, entitling them to greater workplace protections. Only in the United States does access to health care hinge on the contract type, but on the other hand, the self employed have access to social security unlike in many other countries. In all cases, the courts use similar theoretical criteria to assess whether a worker is an "employee." In most countries reclassification cases are pursued by executive agencies (social security or the revenue service) rather than the workers themselves. Finally, in all countries the rules and subsequent decisions are extremely flexible, creating an environment of uncertainty. There are few incentives for workers to pursue legal action which is expensive, has uncertain outcomes, and could lead to job loss. The aforementioned Microsoft case is an example of the odd mix of incentives and recourses available to all parties in this debate. The IRS had the economic incentive to pursue legal action; Microsoft complied, but then had an incentive to use a second form of atypical employment (temp work), and finally with no success on the legislative or judicial fronts, the workers sought a solution through collective action. It is uncertain how this struggle will end, but presuming workers win and get the legal status equivalent to that of regular workers, Microsoft may

as well hire the workers directly and cut out the cost of a middle man.

2.7 Conclusion

In the quantitative analysis we found some support for the entrepreneurship hypothesis; countries with high levels of self employment have high levels of both part-time and fixed term employment, controlling for national constraints on firms' employment activities and on economic conditions. We can imagine then, that in a society that values entrepreneurship, workers use other forms of atypical employment as a means to meet their entrepreneurial goals. The results from the patent variable were sometimes significant in the expected directions, though overall, it was uncertain that the variable captured entrepreneurship as it is also related to the distribution of firm types in the country and is affected by exogenous factors such as international firms' interest in the country's market.

There were particularly interesting results for the "individual constraints" hypothesis. For all factors, as a country becomes more prosperous and has more social protections, there is less self employment. On the other hand, as the society becomes more prosperous and the social insurance expands, the levels of fixed term and part-time employment rise, perhaps because workers are more willing to reduce their work effort to take care of children or are willing to risk periods of unemployment between fixed term contracts. On the other hand, social benefits provided by employers through non-compensation costs seem to discourage atypical employment, encouraging workers to hold permanent regular jobs.

Finally, constraints on the firm, such as union strength and strong EPL, increase self employment and fixed term employment but discourage part-time work. Most likely, the early inclusion of part-time work in EPL, and its traditional inclusion in union contracts, excludes the opportunity for firms to circumvent constraints through part-time contracts.

Atypical employees have lower wages and fewer benefits on average, although there are small subgroups that occasionally earn more (see papers 2 and 3 (chapters 3 and 4),

and (Rica, 2004; Mertens and McGinnity, 2003; Booth et al., 2002; Brown and Sessions, 2003, 2005; Davia and Hernanz, 2002; Graaf-Zijl, 2005; Hagen, 2002; Picchio, 2006). As discussed in the "free-market seeking hypothesis," public policy is a likely incentive for atypical employment. However, it is also the primary avenue towards reducing inequality. As such, worker protections need to be considered in two lights—one as a solution to inequality and one as a cause of atypical employment contracts.

With respect to policy, we have seen three approaches to atypical employment. The first method is legislative and can take the form of flexicurity (de-linking worker benefits from the employer) or by forcing employers to pay benefits for atypical workers (the part-time example) or by forcing atypical employees into traditional employee status (i.e. limiting the renewals on fixed term employment). The second approach was the union approach. The union approach requires that first the courts grant atypical workers the right to organize and second that firms negotiate with them. Once that right is granted, atypical workers must organize, despite often not knowing their coworkers (in the case of temp agencies) and despite rapid turnover. The third approach, through the courts, is primarily used to first ensure the right to collective bargaining, and second, to reclassify independent contractors. Having already examined the empirical evidence, here, the three major approaches are compared from a theoretical viewpoint.

The main question behind the legislative approach is whether to detach protections from the employment relationship (flexicurity) or to patch the existing system. There are strong arguments on both sides of the debate. One major critique of the flexicurity approach is that it assumes social insurance can replace employment protection. This might be a false trade-off; workers can and should have both. Second, the flexicurity approach takes a somewhat defeatist stance towards the breaching of employment security by firms even though these breaches are not an insurmountable problem. Many public systems are regularly breached and patched, such as the tax system. There have been some successes in the "patching the dam" approach such as international union advocacy (European Trade Union Confed-

eration) and the two EU directives on fixed term and part-time work. In some sense, the breaches in workers rights through atypical work are only the result of earlier misguided legislation that loosened regulations on some forms of contracts in hopes of increasing employment. Theoretically, a reversal of that legislation could remove the two tier system, although in Spain, so far, they have been unable to reverse course. In addition, the flexicurity approach is extremely expensive and therefore often politically infeasible. While it is theoretically irrelevant whether it is funded by employer or wage taxes, it can be difficult to pass the appropriate revenue-raising measures on either side.

On the other hand, the flexicurity approach is attractive, in that firms will always find new ways to circumvent worker protections. Insofar as the firm's obligations to their workers begin and end with the paycheck (with the government taxing either the business or individual side of that paycheck) it might be easier to provide worker protections directly from the government. In comparison, EPL necessarily has complicated rules about contract types and regulations for different size businesses and will require expensive enforcement and will inevitably be evaded. Only independent contractors could not be easily covered under a flexicurity scheme because of the ambiguity between a check for services and for pay. Further, many in favor of flexicurity cite the negative side effects of strict EPL. While it is true that past research has found that strict EPL has no effect on unemployment, it *has* been shown to slow down the labor market (increasing average job tenure and the length of unemployment spells), and to increase self employment (though not part-time or fixed term work) (Cazes and Nesporova, 2003). Those arguing in favor of patching the dam argue that this is evidence that strict EPL has no negative effect and we may as well expand it to capture atypical workers. This is, in my opinion, a naive interpretation of these results. Slow turnover can lead to the deterioration of skills among the unemployed. Slow turnover also creates a matching problem (Schioppa, 1993) with worse worker-job matches when the market is stagnant. Further, the EU enabled workers to cross borders for job opportunities, providing incentive for young workers who can't break into a slow-moving labor

market to seek opportunities abroad, inspiring for example, a young Italian brain drain (Becker et al., 2004). Guaranteeing that firms are not paying less non-compensation costs for atypical workers and creating equal rules for dismissal, a flexicurity approach should remove distortions of employer's hiring and firing decisions. Finally, the flexicurity system should guarantee that in a dynamic work environment, workers do not bear the brunt of their employer's flexibility.

Specific policy proposals taking a flexicurity approach include expanding unemployment insurance, funding worker retraining programs, flex-insurance, and social drawing rights. Flex-insurance is a scheme under which employers contribute to a social insurance program in proportion to the flexibility of their contracts. The idea is an extension of US unemployment insurance contributions, where employers with histories of layoffs contribute more. Social drawing rights are a scheme where workers contribute to a national program throughout their lives, accumulating credits (Stone, 2007). These credits can then be used for retraining, care giving, and insuring against career transitions. Of course, all of these flex proposals can be combined with policies from the "patching the dam" approach. While there is a strong theoretical difference between the approaches, practically they need not be mutually exclusive.

The two non legislative approaches, unions and court enforcement, are a bit too piecemeal to be effective. Unions are making significant progress incorporating atypical workers, but it is an uphill battle. In a context where unions negotiate at a lower level (unions bargain at the firm, industry, and overall economy level in different countries where firm-level bargaining is the "lowest" level) the fast turnover of atypical employees will negatively impact their ability to negotiate. In addition, the union approach requires a second approach, court action, to underpin it. With respect to the court-reclassification approach, workers do not have the incentive to take legal action on their own behalf. Thus, court enforcement only works if another party is granted the right to take action on part of the workers, thus protecting workers from retaliatory job loss. In addition, in most countries,

the courts seem to not uniformly enforce a single standard. Perhaps over time one will develop, but currently court enforcement yields inconsistent protections for workers. Finally, if legal action is taken infrequently, and the cost of a firms' workers being reclassified by the courts is low (as is guaranteed by the aforementioned US ruling), then from the firm's perspective the expected costs of reclassification are less than the savings from misclassifying workers. Thus, even when workers are regularly reclassified by the courts, it could still be worth it for firms to misclassify workers, leaving the courts with no deterrence effect.

This paper began with the question of how government policies influence atypical employment levels. Most of the proposed avenues discussed here will reduce firms' incentives to use atypical workers. The flexicurity approach will remove any incentives related to the difference in non-wage compensation or in dismissal protections. On the other hand, flexicurity gives up some protections that many see as unnegotiable. In contrast, while the dam-patching approach might remove some of the cost-saving incentives to use atypical workers, it also creates some unusual incentives. Automatic conversions of fixed term contracts could encourage firms to replace those workers before the limit on their fixed term status. This puts workers in a weak position in an even weaker one. These types of rules could also encourage firms to hire fewer women since they would be forced to accept women's requests to switch back and forth between part and full time as child care needs arise. To date, it is uncertain whether unions can organize and protect atypical workers, although the outlook is better for atypical workers that stay in the same arrangement for an extended period, as at Microsoft. Finally, the courts' reclassification of workers should not influence the incentives to hire atypical workers as long as expected penalties for reclassification are not more costly than the savings from using atypical workers (which seems to be the case today); court reclassification is seldom pursued by workers; and court reclassification is enforced inconsistently.

Given that atypical workers persist in having lower compensation, benefits, and less job security, it seems imperative to design an effective social safety net for atypical workers that

does not create unintended incentives for employers and does not distort the marketplace. Many countries have already begun to address these issues through various approaches. Hopefully, we can find an approach to insure all workers rights that is coherent, clear, and effective, while minimizing the distortion of the labor market by creating unintended incentives for all actors.

Chapter 3

European Labor Institutions and the Fixed Term Wage Gap

3.1 Introduction

"Fixed term" jobs are jobs that have a fixed end date, compared to permanent work, which is presumed to last indefinitely. Some countries, such as the US, have "at-will" employment, meaning there is no substantial difference between fixed term and permanent work beyond the expectation that the employment relationship will endure. The meaning of fixed term work is largely defined by the difference between how a country's legislation treats fixed term workers compared to regular employees. From the employer's perspective, relevant legislation includes: restrictions on firing regular workers that do not apply to fixed term workers and differences in non-compensation costs (firms' non-wage costs directly related to employing a worker) for employing fixed term versus regular employees.

In the past several years, the literature on fixed term employment has greatly expanded. This is a response to the perception, first that fixed term employment is growing rapidly and second, that fixed term workers have lower compensation. The combination of these two factors is hypothesized to lead to a new "underclass" of workers, to explain recent declines in real wages for low skill workers, or to have put downward pressure on wages and job security for all workers. Despite the pervasiveness of these views, the first observation, that

atypical employment is rising, does not have strong empirical support. Figure 3.1 shows that fixed term employment has been relatively stable over the last decade in most European countries. The common misperception of these trends is largely a consequence of how the data are examined. Some studies present growth rates for firms' use of fixed term workers. While these growth rates are often high because the starting point was so low, in absolute terms, the numbers are not so dramatic (Boeri and Garibaldi, 2007; Grip et al., 1997). Also, my own work in chapter 4 suggests that counting the number of firms that use fixed term workers shows larger increases than counting the percent of workers. Those studies that do look at absolute levels (Keller and Seifert, 2005) are often surprised how low the levels of fixed term employment actually are, given the relatively lighter regulation. Spain is the only country where fixed term work truly exploded. From the late 1980's to early 90's, fixed term employment jumped from below 10% of employment to almost 30%, though levels have been stable since 1994. (Dolado et al., 2002). Spain's unusually high level of fixed term employment is related to a distinctive and early legislative history of loosening fixed term employment regulations in an effort to reduce unemployment in the post-Franco era. (A more lengthy discussion of Spain's labor market regulation can be found in chapter 2.3.)

In contrast, there is significant support for the second assertion that fixed term jobs are bad jobs. It has been shown that fixed term jobs have lower pay, less on the job training, and that workers are less satisfied (Booth et al., 2002; Blanchard and Landier, 2001; Cahuc and Postel-Vinay, 2002; Appelbaum, 1992; Callaghan and Hartmann, 1991; Kalleberg et al., 2000a). In addition, Kahn (2007); Hagen (2002); Mertens and McGinnity (2003) show that the negative aspects of fixed term work are not evenly distributed across the population, but that more disadvantaged workers (women, immigrants, very young or old workers, or the less educated) are more likely to be in fixed term jobs. Picchio (2006) expands on this, showing that the effects of a fixed term contract vary by tenure or experience, with the most disadvantaged being most negatively affected by their contract status.

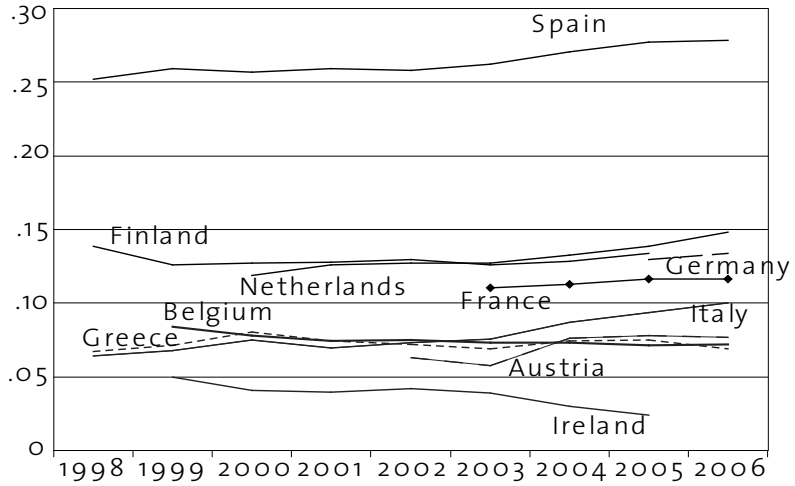

Figure 3.1: Proportion of workers in fixed term contracts

Theoretically, fixed term workers should have have higher wages as compensation for their lack of job security. In practice this is never the case, as there are many other reasons fixed term jobs should have lower wages such as: less skilled workers are selected into these positions; lower paying occupations and industries use more fixed term contracts; firms select workers of uncertain skill in fixed term positions and move them into permanent positions later; or because fixed term workers have less firm-specific knowledge and training and are consequently less productive (Booth et al., 2002; Brown and Sessions, 2003, 2005; Davia and Hernanz, 2002; Graaf-Zijl, 2005; Hagen, 2002; Mertens and McGinnity, 2003; Picchio, 2006; Rica, 2004). While there are many theoretical reasons for the compensation gap and while many of these theories have empirical support, even after accounting for them, the wage gap persists. This leaves some to classify the unexplained difference as outright discrimination (Brown and Sessions, 2003, 2005), analogous to the interpretation of unexplained disparities in gender or racial wage gaps.

Most studies assess relative compensation for fixed term workers using data for individual countries. Figure 3.1 shows each of these studies along with the method they used, the raw wage gap between fixed term and regular workers, and the gap controlling for se-

lection. For example, Brown and Sessions show that while fixed term workers in Britain earn 16% less than permanent workers, controlling for selection they earn only 13% less. These estimates were not the primary goal of these papers. Several of papers use these estimates as starting point, going on to estimate differences in career-long wage profiles for fixed term workers, or to estimate how the wage gap varies by education group. All studies listed in table 3.1 find significant wage gaps for fixed term workers. Only Booth et al. (2002) and Brown and Sessions (2005) found instances of positive wage gaps, where fixed term workers earn more. Estimates are inconsistent, ranging from 6 to 38% wage gaps after accounting for various controls such as workers' education, occupation, firm size, experience, tenure, gender, and marital status. Estimates vary widely because each studies a different country, in a different time period, and uses a different population (i.e. age 16-60 versus 25-65). The studies' estimated gaps are thus incomparable, as the differences across estimates stem both from differences in the countries being analyzed as well as sample characteristics.

A cross-national perspective has largely been ignored because there is limited cross national data available with variables indicating fixed term employment. A first step towards fixing this gap was taken by Kahn (2007), who found interesting differences in countries' *levels* of fixed term employment. Only Brown and Sessions (2005) did a cross-national comparison of the fixed term wage gap. They used data from the International Social Survey Programme's 1997 Work Orientations Survey in ordinary least squares regressions by country to generate coefficients for the fixed term contract's effect on wages by country. The coefficients ranged from negative wage gaps (fixed term workers earning more) in Japan and Norway, the same wages in the US, Italy, Switzerland and Denmark, and positive wage gaps from 12 to 25% in Germany, France, Canada, New Zealand, Sweden, and Portugal with the worst gaps in Germany. This study builds on this Brown and Sessions paper, estimating the cross country gaps consistently, using methods that should better account for selection, and weighting the calculation of the wage gap so that it reflects those

	country	year	method	raw gap	controlling selection
Brown & Sessions (2003)	Britain	1997	Heckman selection	16%	13%
Booth et al. (2002)	Britain	1991-97	OLS	-13 to 6%	6 to 17%
Hagen (2002)	W Germany	1999	Heckman selection	–	23%
Hagen (2002)	W Germany	1999	matching	–	6 to 10%
Mertens & McGinnity (2003)	W Germany	1995-00	OLS	32%	7 to 26%
Davia & Hernanz (2002)	Spain	1995	selection	39%	23%
Rica (2004)	Spain	1995	selection & FE	34 to 42%	$\approx$ 10%
Graaf-Zijl (2005)	Netherlands	1997-98	matching & FE	22%	5.3%
Picchio (2006)	Italy	2000-02	RE, FE & IV	12%	12 to 13%
Brown & Sessions (2005)	Multiple	1997	OLS	16%	17 to 38%

Table 3.1: Literature on the fixed term wage gap

that are likely to be in fixed term employment. In addition, while the Brown and Sessions paper devoted a short paragraph to the analysis of cross-country comparisons, this paper conducts a more thorough analysis and interprets the results in the context of cross-national institutional differences. From this point forward, this paper proposes some possible theoretical explanations for cross-national differences, tests several approaches to consistently estimating cross-national estimates of fixed term workers' relative wages in ten countries, and concludes by outlining a few relevant policy approaches to fixed term employment.

3.1.1 National context, theory, and policy

There are many differences between countries that can influence the gap between fixed term and permanent workers' wages including: unemployment rates, real wages, labor market participation rates, the types of industries that dominate the market, the size distribution of firms, and the age structure of the population. There are also institutional differences across countries, including the social security system, tax code, labor relations, and labor laws. Among the institutional factors, one of the most studied is employment protection legislation (EPL). Research has found EPL to have a wide array of impacts beyond their intended role of fostering job security, such as increasing or reducing productivity (Kenworthy, 2008; Autor, 2007), slowing turnover (Cazes and Nesporova, 2003; Messina and

Vallanti, 2007), decreasing employment (Scarpetta, 1996; Nickel, 1997; OECD, 1999), increasing temporary employment (Kahn, 2007), and increasing incentives to invest in human capital (MacLeod and Nakavachara, 2007). There are several mechanisms through which EPL might also influence fixed term wage gaps.

The first mechanism through which EPL influences wage gaps is selection. If a firm is unable to dismiss workers, it might keep new-hires, workers of uncertain ability, or lower quality workers in fixed term contracts as an extended probation period, leaving the less experienced and least skilled over-represented in fixed term jobs.[1] Selection is probably the primary mechanism through which EPL influences fixed term wage gaps. But even after controlling for selection EPL, might still foster fixed term wage gaps.

A change in the fixed term wage gap based on EPL can happen a the result of any interaction between an unobserved variable and the EPL climate. For example, there can be an interaction between firms size and EPL. It has been shown empirically that big firms pay higher wages, and that big firms use more fixed term workers (Goux and Mourin, 1999). Big firms might react more strongly to the EPL change (since given the big-firm wage bonus) since they have more to lose if they can't fire a worker in a downturn. Thus, when the a stricter EPL regime is imposed on the two economies, the effect will be different in the two countries, with the magnitude of the difference depending on the distribution of workers between big and small firms, the gap between big firm and small firm employment, and the size of the difference in big and small firms' reactions to the new laws. EPL can interact with many unobserved country-level conditions in this way. While the interactions will influence the observed wage gaps in the various countries, they need not actually change the actual expected wages of a worker. For example, the big firms could switch to fixed term workers, paying the exact same wages, leading to a perceived smaller wage gap.

In the selection story the overall wage distribution shifts existing low paying jobs into the fixed term wage category, leaving no effect on the overall wage distribution. In the inter-

[1] This was confirmed empirically by Kahn (2007).

action story the fixed term wage gap can change, again with no change in wages. But there are other scenarios where strict EPL could actually depress fixed term workers' wages. Just as with unions (Lindbeck and Snower, 2001), the combination of strict EPL and fixed term contracts could create an insider-outsider market. Under strict EPL the employer is vested in long term productive relationships with permanent workers because they cannot fire them. The firm might consequently pay permanent workers well to maintain job satisfaction and offer raises as incentives. In comparison, unproductive fixed term workers could be easily fired, and productive fixed term workers could be motivated by the possibility of permanent employment. In contrast to the interaction effects described in the prior paragraph, this mechanism could truly depress wages for fixed term workers.

There are also several causes for the wage gap that have nothing to do with country differences. If these mechanisms dominate the wage-gap dynamics, countries should have uniform wage gaps. For example, fixed term contracts should lead to under-investment in firm-specific training causing fixed term workers to be less productive, and the wage gap consequently reflecting a real difference in worker productivity. Similarly, fixed term workers might be less devoted to their employers and therefore less productive (Guadalupe, 2003). Another hypothesis for the wage gap that does not interact with EPL is the labor competition hypothesis. If fixed term workers are substitutes for permanent workers, but with lower wages and lower firing costs, they should put downward pressure on permanent workers' wages. If fixed term workers are perfect substitutes, permanent workers should receive *lower* wages in compensation for their job stability. It is possible that fixed term workers are initially an imperfect substitute, but as they become a larger part of the labor force they become a more perfect substitute. As the two classes of workers become more similar, wages should converge and thus the wage gap could vary with the proportion of the labor force in fixed term employment. In this case we might expect Spain, where such a large proportion of the labor force is fixed term, to have the smallest wage gap.

If wage gaps do vary by EPL, we should see significant differences across countries'

wage gaps since EPL varies quite widely. Table 3.2 shows some key aspects of countries' worker dismissal protections as of 2000 (the year of the data used in this paper). Note that in some countries, white collar and blue collar EPL differs, though these differences are slowly being eliminated. Overall, Germany and Austria had some of the strictest protections while Ireland has weaker protections, in line with the Anglo Saxon model. This is confirmed by the OECD's EPL index which codes these regulations into a single index (highlighted in table 3.3). Dismissal protection usually begins after some probation period and then increases with tenure up to some limit. Probation periods range from 0 months in Denmark for blue collar workers to 6 months in Germany and 2 years in the UK. This is important to note since fixed term work is often cast as an extended probation period without severance costs, even though probation periods and phased in dismissal protections already exist for regular workers. Of course, there are other EPL considerations besides severance, like the bureaucratic costs of firing a permanent worker; in those countries with strict dismissal protection bureaucratic costs of dismissal can also be very high. For example in Austria, workers can protest being fired through their unions based on personal hardship, forcing employers to incur high costs to pursue the dismissal.

Besides the absolute level of EPL, the difference between EPL for regular employees and for fixed term workers should be a driving factor behind between variation in countries' wage gaps. EPL for regular employment centers around limitations on severance, which do not normally apply to fixed term workers. Rather than requiring dismissal notice, EPL for fixed term employment usually limits the total cumulative duration of fixed term contracts or the number of contract renewals. In 2000, cumulative contracts were limited to 15 months in Italy, 18 in France, 24 in Germany, 30 in Belgium, and 36 in Spain while Austria, Belgium, Finland, France, Germany, Greece, Italy, Netherlands, and Spain all limited the allowed number of consecutive contract renewals. The OECD used these regulations to create an EPL index for regular workers, and another for fixed term workers. The two indices are illustrated in table 3.3. One would would naturally consider the difference be-

	blue collar	white collar	compensation	collective
	statutory notice periods (days notice / time worked)		when unjustified	redundancy layoff payment
Austria	14 days	6 wks / 2 yrs 2 mo / 2-5 yrs 3 mo / 5-15 yrs 4 mo / 15-25 yrs 5 mo / >25 yrs	NA	2-12 months wages
Belgium	7 days / 6 mo 35 days / <20 yrs 112 days / >20 yrs	3 mo / 5yrs	up to 6 mo	depends on unemployment insurance
Finland	14 days / yr, max 6 mo	same	3-24 mo	maximum 24 months wages
France	1 mo / 6-24 mo 2 mos / > 2 yrs	same	> 6 mo	yes, unspecified
Germany	4 wks / 2 yrs 1 mo / 2-5 yrs 2 mo / 5-10 yrs 4 mo / 10-20 yrs 7 mo / >20 yrs	same	<12 mo	yes, unspecified
Greece	5-60 days	1-6 mo	yes	b-collar: 5-105 d w-collar: 1-24 mo
Ireland	1 to 8 wks	same	max 104 wks	1/2 wk's pay per yr service & 1 wk/ yr after age 41
Italy	by collective agreement	same	15 mo	1 yr's wages/13.5 1.5% pr yr
Netherlands	max 6 wks	same	depends on service & age	no
Spain	30 days	same	45 days per yr of service up to 42 mo	20 days/yr service, to 12 mos

source: ILO Termination of Employment Digest & OECD Employment Outlook

Table 3.2: EPL for permanent workers

tween the two indices as a measure of the difference between EPL for regular and fixed term employment. However, in some sense, the regular EPL index measures the difference between fixed term and regular workers because it measures the protections that regular workers have that fixed term do not, while the fixed term EPL index measures a separate topic–time limitations on fixed term contracts. One would expect that those countries with strict regular EPL would not worry about firms avoiding worker protections through atypical employment, and thus countries with low regular EPL indices should have low fixed term indices. However, this only seems to be the case for Ireland, as shown in the table.

EPL can only influence fixed term wage gaps insofar as it is enforced or there are not other means of circumventing it such as through the informal sector, self employment, or part-time employment. This is particularly of concern in the Mediterranean countries where EPL is strict but there can be poor compliance. For example, in Greece 35% of the workforce is self employed, many of whom are not actually entrepreneurs, but rather the underemployed or contractors not subject to EPL. In addition, there is evidence that small Greek firms often circumvent or defy labor protections and unions contracts (Mihail, 2003; Kufidu and Mihail, 1999). This is a serious problem given that the average Greek firm has only 2 employees (compared to the EU's average 6 (Mihail, 2003)). Similarly, in Italy firm size is about one half the average firm size in Europe, and EPL for small firms (under 15 employees) is significantly weaker (Boeri and Garibaldi, 2007; Schivardi and Torrini, 2003). Thus, in Greece and Italy, we might expect strict EPL to not impact fixed term workers' relative wages given the abundant legal and illegal ways to circumvent regulation.

Fixed term workers' relative wages should also vary due to other national differences like unemployment rates and union strength. Union contracts can encourage a larger wage gap for several reasons. First, union contracts can explicitly limit the quantity and duration of fixed term employment. This can limit fixed term work to the least qualified, creating larger wage gaps. Even controlling for selection, unions could still increase the wage gap by preventing fixed term workers from becoming a widely available substitute that would eventually depress permanent workers wages to parity. Finally, as fixed term workers are often not advocated for by unions, there could be an insider-outsider wage system with fixed term workers on the outside. The overall state of the labor market matters too. When unemployment rates are high, there should be a higher wage gap. Workers entering a weak labor market could be forced to start with lower wages. A weak labor market should encourage a large wage gap assuming that when the labor market takes a downturn workers in permanent positions are not forced to take a wage cut and assuming the firm hires new workers through fixed term jobs both to save money on compensation and also because

they to not want workers with the same contracts having very different wages. When the labor market improves, fixed workers should be able to negotiate better, permanent jobs.

Table 3.3 illustrates the four factors (in 2000) hypothesized to predict country differences in the wage gaps. The first column is the OECD Employment Outlook's EPL index, a 0-6 point scale measuring how strict a country's EPL is based on firing notification procedures, time delay before the firing process can start, length of notice before dismissal, severance pay, strictness of defining an unfair dismissal, length of probationary periods, length of compensation following dismissal, and the possibility of reinstatement following the dismissal. The next column shows the OECD EPL index for fixed term workers which is based on whether firms must justify fixed term contract, limits on successive contracts, and limits on the maximum cumulative duration of fixed term contracts. The third and fourth columns indicate the proportion of workers who belong to unions (density) and the proportion of workers who are covered by unions in bargaining negotiations. The fifth column shows the unemployment rate. The last column shows the hypothesized wage gap given that strict EPL, a wide gap between EPL and fixed term EPL, strong unions, and high unemployment, should all anticipate larger wage gaps. Most countries' EPL, union strength, and unemployment rates predict fixed term wage gaps in opposing directions, additively predicting neither large nor small wage gaps. In contrast, Germany and Austria have values for all three variables that consistently anticipate a large wage gap while Ireland has values that consistently predict a small wage gap. These lead to the primary hypotheses: Austria and Germany should have the largest wage gaps while Ireland should have the least. One might also speculate that there are smaller wage gaps in the Mediterranean countries given the abundant ways to circumvent apparently strict EPL.

	EPL index	FT EPL index	union density	union coverage	unemploy rate	predicted gap
Austria	2.7	1.8	37	95	6.3	high
Belgium	1.7	2.0	56	90	7.0	medium
Finland	2.2	3.3	76	90	9.8	medium
France	2.4	4.0	10	90	9.6	medium
Germany	2.7	1.8	25	68	7.8	high
Greece	2.3	4.0	27	-	11.2	medium
Ireland	1.6	0	38	-	4.3	low
Italy	1.8	4.3	35	80	10.7	medium
Netherlands	3.1	.8	23	80	2.9	medium
Spain	2.6	3.0	15	80	15.7	high

source: OECD Employment Outlook, Source OECD

Table 3.3: Country conditions & predictions

3.2 Data & methods

3.2.1 Data

This study uses individual level data from the Luxembourg Income Study (LIS) in 2000 covering 44,508 workers in Austria, Belgium, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, and Spain. The countries were chosen because the LIS data included an indicator for fixed term employment for these countries. However, there are also theoretical reasons to include these countries. First, any study on fixed term employment should include Spain, the country with the most fixed term workers. Second, the study should have at least one representative Anglo Saxon country to represent looser labor market regulation, in this case Ireland. In contrast, Germany, Austria, the Netherlands, and France represent stricter EPL settings, while Italy and Greece represent the Mediterranean countries where regulations can be strict but enforcement may be weak. The study could be improved by including Denmark (the classic "flexicurity" country) and the United States (the biggest free labor market country). The sample was limited to those workers who were not disabled, between ages 25 to 65, who reported employment as their primary activity, who were not in the military and were not self employed. The minimum age of 25 is older

than that used in most studies, but was chosen because the average age at graduation from postsecondary studies varies dramatically across countries, reaching as high as 25 or 26 for tertiary education in Germany (OECD, 2006). Self employed workers were excluded from the study primarily due to Greece. In Greece there are more self employed than in other European countries and these workers earn significantly less. Including them along with permanent workers in the baseline group makes fixed term workers look relatively better off. Presumably, these workers also suffer a form of disadvantaged employment and therefore are not the proper comparison group. Including the self employed in the baseline group did not effect the results for any of the countries except Greece.

Descriptive statistics by country are displayed in the appendix. In all countries, the largest group of workers live with both a partner and children. Living alone is more common in Germany and the Netherlands while living with parents is more common in the Mediterranean countries. With respect to education, Belgium and France have the most educated workers while Italy has the least, although it is worth noting that it is impossible to make *exact* comparisons, particularly at low levels of education (university and post-graduate are comparable). The Netherlands has the most workers in professional and managerial occupations and work the least hours (along with the French) while Spanish and Greek workers work the most. The Germans and Dutch earn the most while the Mediterranean countries earn the least. There are slightly more male workers in all countries, except in Finland where women slightly outnumber men in regular employment thought they are equal when self employment is included. Finally, the proportion of workers in fixed term contracts varies dramatically across countries. Only 3.61% of the Irish sample (66 workers) were in fixed term jobs while 21% of the Spanish sample (670 workers) were. The Spanish data shows significantly fewer workers in fixed term contracts than the Eurostat data (figure 3.1) because part-time workers and those under 25 (both groups more likely to have fixed term jobs) were excluded.

Table 3.4 shows descriptive statistics by employment status. Fixed term workers are a

	permanent workers	fixed term workers
family status		
w/ partner & kids (%)	45.54	42.06
w/ partner, no kids (%)	30.97	24.31
no partner & kids (%)	3.41	5.31
alone (%)	13.57	15.69
partner & parents (%)	0.35	0.72
w/ parents (%)	5.82	11.90
education		
missing	0.38	0.51
no secondary (%)	17.14	23.53
1st secondary (%)	25.69	23.02
2nd secondary (%)	26.27	20.43
tertiary (%)	10.27	9.19
university (%)	16.75	17.79
post-grad (%)	3.50	5.53
effort		
mean hrs/wk	38.11	35.76
wks/yr	50.25	40.46
occupation		
agricultural (%)	5.38	6.39
unskilled (%)	9.85	18.70
service (%)	36.96	32.68
skilled trade(%)	21.27	18.21
manager (%)	12.02	5.41
professional (%)	14.52	18.61
other		
mean age	42.02	38.09
male (%)	55.57	44.46
gov't employee (%)	29.23	34.19
supervisor (%)	25.80	10.91
native (%)	90.43	88.24
mean experience (yrs)	24.69	20.21

Table 3.4: Descriptive statistics by employment status

relatively disadvantaged group. They are more likely to be single parents, live alone, live with both their partner and their parents, and to be single living with their parents. They are also younger, more likely to be women or immigrants, are less experienced, and work less hours. Fixed term workers are not clearly disadvantaged with respect to their education and occupation. Rather, they are over-represented at both extremes of the education spectrum and are more likely to work in *either* elementary or professional occupations.

There is a significant wage gap between fixed term and permanent workers before accounting for differences in the workers' characteristics. The gap is highest in Spain, where

	men	women
Spain	.51	.49
Greece	.60	.71
Finland	.62	.69
Austria	.63	.48
France	.64	.64
Belgium	.65	.65
Italy	.65	.79
Germany	.65	.68
Netherlands	.66	.62
Ireland	.85	.88

Table 3.5: Relative wages for fixed term workers by country

women in fixed term jobs only earn 49% of their permanent counterparts compared to 88% in Ireland. This analysis estimates fixed term workers' relative wages by country after controlling for selection, using a matching model and two different regression-based models. Initially all analyses were done for men and women separately. However, because the resulting wage gaps did not show significantly different trends for men and women, all analyses from this point on aggregate the data. Similarly, all analyses were originally run separately for gross and net wages since Italy and France only report net wages. Results between the two were similar (gross wage gaps were *slightly* larger because of the redistributional effects of the tax system). This paper presents pooled male and female results and uses gross wages for all countries except Italy and France where net wages were substituted. Original results are available upon request.

3.2.2 Matching model

The first method is a matching model in which the fixed term contract is considered the "treatment" and the outcome is wages.[2] The matching model compares the log hourly earnings of each person in a fixed term contract with a comparable individual (or a composite of individuals) was equally likely, given their characteristics, to work in a fixed term

[2]For a nice introduction to matching that describes the methods used here in more detail, see Harding and Morgan (2006) or Rosenbaum and Rubin (1985).

contract job but did not. Unlike the regression analyses, matching does not yield estimates for the control characteristics' effect on wages or for the interactions between the treatment and characteristics, is less dependent on a model specification and functional form, and is non-parametric. Finally, as does regression, the model assumes that we have not omitted any variables relevant to selection into fixed term employment.

There are three standard statistics of interest in matching models: average treatment effect on the treated (ATT), marginal treatment effect (MTE), and the average treatment effect (ATE). Here we use the ATT, which measures the average effect of being a fixed term worker on wages for those who were selected into fixed term work. In comparison, ATE measures the average effect on all workers, and MTE measures the effect on the marginal worker drawn into fixed term work, or in other words, the treatment effect on the next person that would be treated, if treatment were to be expanded 1 person. The ATT was chosen on theoretical grounds, because in this case, the population of interest is those workers likely to be selected into fixed term work. The effect of being a fixed term worker most certainly varies across the population; both my own and others' research have shown that high skill workers and those less likely to work in fixed term jobs, suffer lower wage penalties when they do have fixed term jobs. For the ATE, the weights for individuals who were unlikely to work fixed term would be equal to those likely to work fixed term and since high skill fixed term workers generally have a lower wage gap, the ATE should be lower than the ATT. The ATT was also chosen for pragmatic reasons, as it only requires us to estimate one counterfactual, the wages of fixed term workers had they been permanent.

Given that most workers are permanent, there is an abundant sample of permanent workers to use as a counterfactual estimate of the fixed-term workers. The ATT is estimated by $E[Y(1)|T=1]-E[Y(0)|T=1]$ where T indicates treatment (1 is fixed term and 0 is permanent). Wages are indicated by Y, where $Y(0)$ is the permanent worker's wage and $Y(1)$ is the fixed-term wage. Thus, $E[Y(0)|T=1]$ is the expected wage for fixed term workers had they been permanent while $E[Y(1)|T=1]$ is their observed wages. Of

course, $E[Y(0)|T=1]$ does not exist and is instead estimated using the wages for individuals who were equally likely to work fixed term, given their characteristics, but were actually permanently employed. Given the observable characteristics, X_i we can use the mean conditional wages for permanent workers as an estimate for fixed term worker's wages had they been permanent, and we set $E[Y(0)|T=1,X] = E[Y(0)|T=0,X]$. Thus, if T_i is 1 for the treated, the average treatment effect is the sum of the differences between each treated person's wages and the wages of an untreated comparable person, divided by the total number of treated, $ATT = \frac{1}{\sum_{i-1}^{n} T_i} \sum_{i=1}^{n} T_i E[Y_i(1) - Y_i(0)|X_i]$. The ATT estimate assumes that selection into fixed term employment is based on observable characteristics, that there is common "support" (or that no individual's treatment status can be perfectly predicted ($Pr(T=1|X) < 1$, i.e. there is a comparable untreated person for every treated person), and that individuals are independent observations.[3]

The first step of the matching model is to estimate the "propensity score, or probability of fixed term employment for all individuals, $PR(T_i = 1|X_i)$, given their observable characteristics X_i, using a logit model (see Borooah (2001) for a description of a logit model.) Independent variables predicting fixed term employment include: a dummy for fixed-term employment, experience and years of experience squared, 5 dummies indicating household type, 5 dummies indicating education, 5 dummies indicating occupation, and dummies for gender and public sector employment. Propensity score estimation and matching were done within country.[4] The family predictors were included in the analysis, although they were insignificant for several of the logit models.[5] The predicted probability of being in fixed term employment was then used in kernel matching[6], which matched fixed term workers

[3]We present the exponential of the ATT (which is the difference in ln wages). This is exp((ln)fixed term-ln(permanent)) which is equivalent to relative wages: fixed term /permanent.

[4]While presented results are stratified by country, other models are by country-gender, occupation, education, occupation-country, and education-country. All are available upon request from the author.

[5]See Bryson et al. (2002) and Rubin and Thomas (1996) for discussions of how including insignificant explanatory variables can influence support, estimates' variance, and how the inclusion of a predictor can have different effects depending on its relationship to the outcome variable.

[6]See Caliendo and Kopeinig (2008) for an excellent guide to matching

to a weighted average of those permanent workers with a propensity score within .06 of their own. Weights are assigned based on the difference between a fixed term worker's propensity score and the permanent workers' propensity scores within .06 of his/her score. The weight is $w_{ij} = \frac{K(\frac{p_i - p_j}{h})}{\sum_j K(\frac{p_i - p_j}{h})}$ where p is the propensity score, i and j indicate the treated and the untreated observations, h is the bandwidth (.06), and K is the epanechnikov kernel, $K(u) = \frac{3}{4}(1-u^2)$. This is basically a smoothing method, comparable to a moving average. This works particularly well here given that there are so many more untreated than treated. After treated individuals are matched, the difference between incomes is taken and then averaged across treated individuals. Presented standard errors are bootstrap estimates[7], although I also calculated the standard errors as defined by Anadie and Imbens (2004); Lechner (2001).[8]

3.2.3 Regression analysis

The wage gap estimates, obtained with the matching model, are compared to results from two regression analyses. It is anticipated that the regression analyses will have slightly different results based on the heterogenous effect of the fixed term work treatment across the population. While the ATT finds the effect of fixed term work for those likely to be in fixed term work (or for, on average, lower skill workers who suffer more wage penalties in fixed term work) regression analysis should find lower wage penalties weighting all segments of the sample equally. Ideally, for the regression analysis, we would examine the fixed term wage gap by country with a multilevel model, in which we could estimate the specific country-level characteristics that influence the wage gap. Unfortunately, with only ten countries and one of year data, a multilevel model is impossible. As such, two

[7] i.e. standard errors are calculated through repeated sampling of the data

[8] This standard error formula assumes independent observations, fixed weights, homoskedastic income within the fixed term and the permanent workers, and assumes that income's variance does not depend on the propensity score: $se = \frac{1}{n_{t=1}} * var(Y|t=1) + \frac{\Sigma(w_j^2)}{(n_{t=1})^2 * var(Y|t=0)}$ where $t=1$ and i indicate a treated observation, $t=0$ and j indicates an untreated observation, $n_{t=1}$ is the number of treated (presumably matched), w_j is the total weights applied to each untreated observation, and Y is income.

other regression-based approaches are taken that will allow us to consider whether countries' fixed term wage gaps vary significantly by country. We will then use those results to qualitatively consider what country-level factors promote wage gap.

First, OLS regressions by country were estimated. This model independently estimates the slope for fixed term employment by country, as illustrated in equation 3.1. In this model, the dependent variable, $ln(W)$, is the natural log of hourly wages. Independent variables include a dummy for fixed-term employment X, experience (E and E^2), 5 dummies indicating household type (F), 5 dummies indicating education (S), 5 dummies indicating occupation (O), and dummies for gender (G) and public sector employment (P). This model allows the effects of all of the independent variables to vary by country and avoids the country-correlated observations we would have pooling countries. However, the model also does not use the information from the variability between countries when estimating the individual parameters. The model also assumes that there are no omitted variables and that all worker characteristics relevant to the fixed term wage gap are included.

$$ln(W_{ij}) = C_i + \beta_i E_{ij} + \gamma_i E_{ij}^2 + \sigma_i \mathbf{F_{ij}} + \lambda_i \mathbf{S_{ij}} + \zeta_i \mathbf{Oij} + \xi_i G_{ij} + \psi_i P_{ij} + \alpha_i X_{ij} + \varepsilon_{ij} \quad (3.1)$$

The second model (illustrated in equation 3.2) pools countries and estimates random intercepts by country and random slopes for fixed term work by country. In this model, countries are still indicated by i and the workers by j, so that individual j in country i has wages W_{ij}. The intercept has two parts, a general part (C) and a country-specific part (K_i). The effect of a fixed term contract on wages is estimated by two parameters, the overall mean part (α), and the country-specific addition (γ_i). This joint effect, $exp(\alpha + \gamma_i)$, indicate the relative wages for fixed term employment based on an overall effect *plus* the country-specific effect. The exponent of $exp(\alpha + \gamma_i)$ indicates the relative wages. In this equation fixed term contracts are still represented by X but all other variables have been shortened into a single vector, Z with the accompanying vector of parameters, β. The

country-specific intercepts are mean that each country has its own mean wage. The random parameters K_i and α_i, are not estimated along with the fixed parameters, but their variance is estimated along with ε_{ij}'s variance. The model uses variation between countries to estimate the parameters, assuming that they are drawn from a bivariate normal distribution with zero mean. We then consider the empirical Bayes prediction of the random intercepts and slopes as the outcomes of interest.

$$ln(W_{ij}) = (C + K_i) + (\alpha + \gamma_i)X_{ij} + \beta \mathbf{Z_{ij}} + \varepsilon_{ij} \tag{3.2}$$

3.3 Results

3.3.1 Matching

The first step of the matching analysis was to estimate 10 logit models predicting the odds that a worker was in fixed term employment in each of the 10 countries. For all logit models all the predictors were significant at the .05 level for all countries with the exception of most of the household status variables (living with partner and children vs. single, etc.) for most countries and some of the educational variables for just Belgium and Greece. The models were all significant improvements in explanatory power with psuedo R-squares ranging from .04 to .1. As expected, those with less experience, living in alternative family situations (with parents, as single parent, or with both spouse and parents), or those working in the public sector, with less education, or the two extremes of the occupational distribution (elementary or skilled trades and professional) were more likely to be in fixed term positions. Managers with a university education living with their spouse and children were the least likely to work in fixed term employment.

The matching diagnostics were good, though not perfect. All observations in the occupational and educational matching fell within support, meaning that everyone was matchable (with the exception of two individuals: one in Belgium and one in Austria). The dis-

tribution of propensity scores between the treated and untreated groups generally showed similarly shaped distributions, although, by definition, the treated had higher propensity scores. Finally, the treated versus the untreated mean characteristics were compared and the balance of the sample pre and post matching was calculated. Generally there was a significant difference between the treated and the untreated on all characteristics before matching. After matching, most significant differences disappeared although even post-match permanent workers still had more education on average, were more likely to work as managers, and had more work experience. The observations were balanced with respect to tertiary and secondary education, skilled jobs, and some family characteristics like living alone.[9] Several patterns emerge across variables and countries. First, Finland and Germany had the most significant differences between fixed term and regular workers post-match. In Finland, there were significant differences between fixed term and regular workers in higher education and the higher ranked occupations. In Germany there were significant differences for a variety of variables including work experience, family condition, post graduate training, and managerial professions. The best balance between fixed term and regular workers was achieved for Spain, likely due to the wide array of workers in fixed term work. The fact that the countries with the largest wage gaps (Germany and Finland) were also those with the worst matches could mean that selection is not well-controlled in this analysis for these countries. However, it also could mean that there is stricter sorting of low-skill workers into fixed term jobs in these countries – which is supported by the logistic regressions used in the propensity score analysis where Germany and Finland were better predicted than those countries with better post-match balances like Greece or Ireland.

Figure 3.2 shows the result of the matching analysis for relative fixed term wages by country. The left hand panel shows the relative wages of fixed term workers before matching and the right hand panel shows it after matching (the exponential of the difference in ln wages, or the percent of permanent workers' wages earned by fixed term workers.) The

[9]Diagnostics for all matches are available upon request.

bars's breadth indicates the 95% confidence interval for the estimate and the bars are ordered from the greatest difference between fixed term and permanent wages to the least. Italy and France are in bold, indicating that net (not gross) income was used. The most remarkable aspect of these findings is how large the relative wage gap is. Fixed term workers earn between 55 to 75% of their permanent counterparts' hourly wages. The confidence intervals are quite wide for some countries like Ireland, where there were few fixed term workers.

The shift between between the left hand and right hand panels is the change due to controlling for selection through matching. For some countries, like Greece, Belgium, and Spain, this is a significant shift while for other countries, like Germany, it makes almost no difference. Fixed term contracts play a larger role as a sorting mechanism in those countries with a greater shift.

There seems to be a significant difference across countries, with the first group of countries (Germany, Netherlands, Italy, Finland, and France) having significantly lower wages for fixed term workers than the other countries (Austria, Spain, Greece, Belgium, and Ireland). One should note that in the separate analyses of men and women, the same trends held but since men and women had similar patterns, the analyses were merged with the intent of reducing the standard errors, particularly for Ireland. As anticipated, Ireland (with less EPL, less union control, and a strong economy) and Greece (with many avenues to circumvent EPL) have higher relative wages while countries with strong EPL like Germany have lower relative wages. Austria has a surprisingly small wage gap given its strict EPL.

3.3.2 Regression analysis

If we recall from the discussion of methods, it is anticipated that the regression analyses will have slightly different results based on the proven heterogenous effect of the fixed term work treatment across the population. The matching should have found the effect of fixed term work for those likely to be in fixed term work (more low-skill workers who gener-

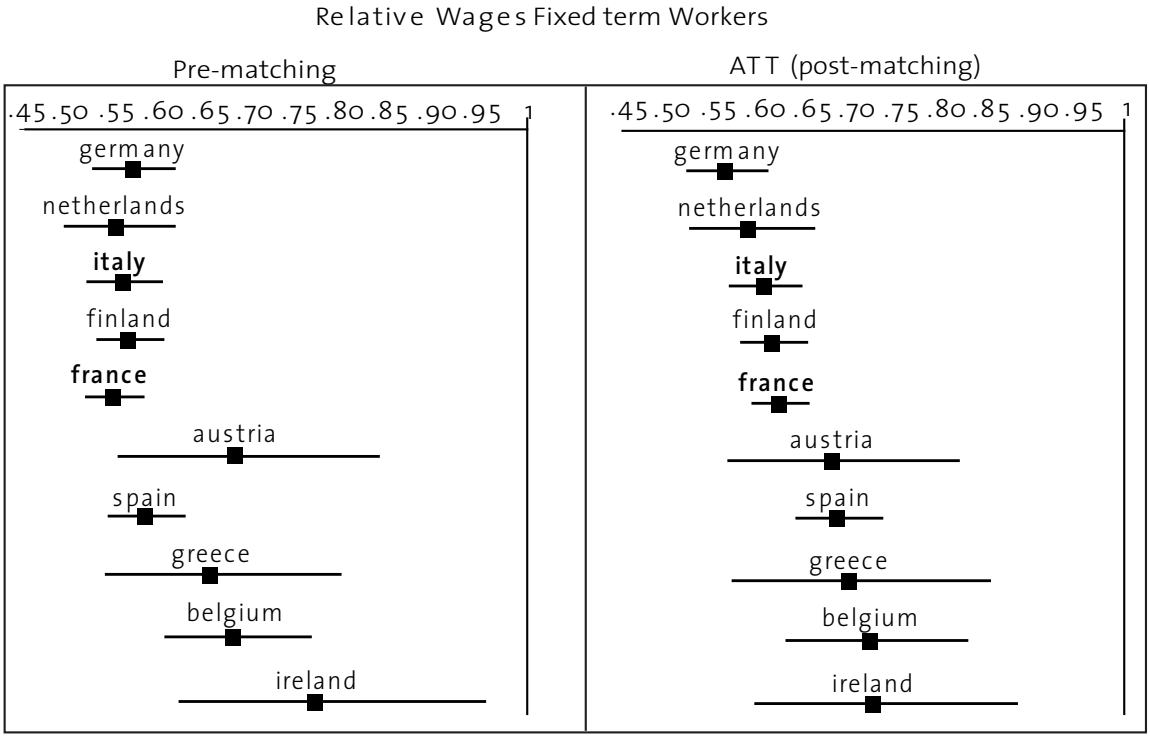

Figure 3.2: ATT by country

ally suffer worse fixed term wage penalties) while the regression analysis should weight all workers equally, finding a smaller wage penalty. Surprisingly this is not the case. Regression results confirmed the matching results, showing that fixed term workers in Germany and the Netherlands are significantly more disadvantage than fixed term workers in Ireland, Greece, and Belgium. The first panel in figure 3.3 shows the exponential of the coefficients for the dummy variable indicating fixed term employment in the country-specific OLS regressions. The bars illustrate the 95% confidence interval. The graphic shows that in Germany fixed term workers earn about 56% of the hourly wage that their fixed term counterparts do, while in Belgium they earn about 73%.

The second panel of figure 3.3 shows results from the random slopes model. The points are the exponential of the fixed term dummy plus the exponential of the country-specific random slope for fixed term employment, again indicating the percent of permanent work-

ers' hourly wages earned by fixed term workers. These results confirm that in Germany fixed term workers have the lowest relative wages while in Ireland, Greece, and Belgium fixed term workers are relatively less disadvantaged. Due to the overall fixed term employment coefficient being much larger than the country effects, most countries' confidence intervals overlap with the only significant contrast being between the two extremes.

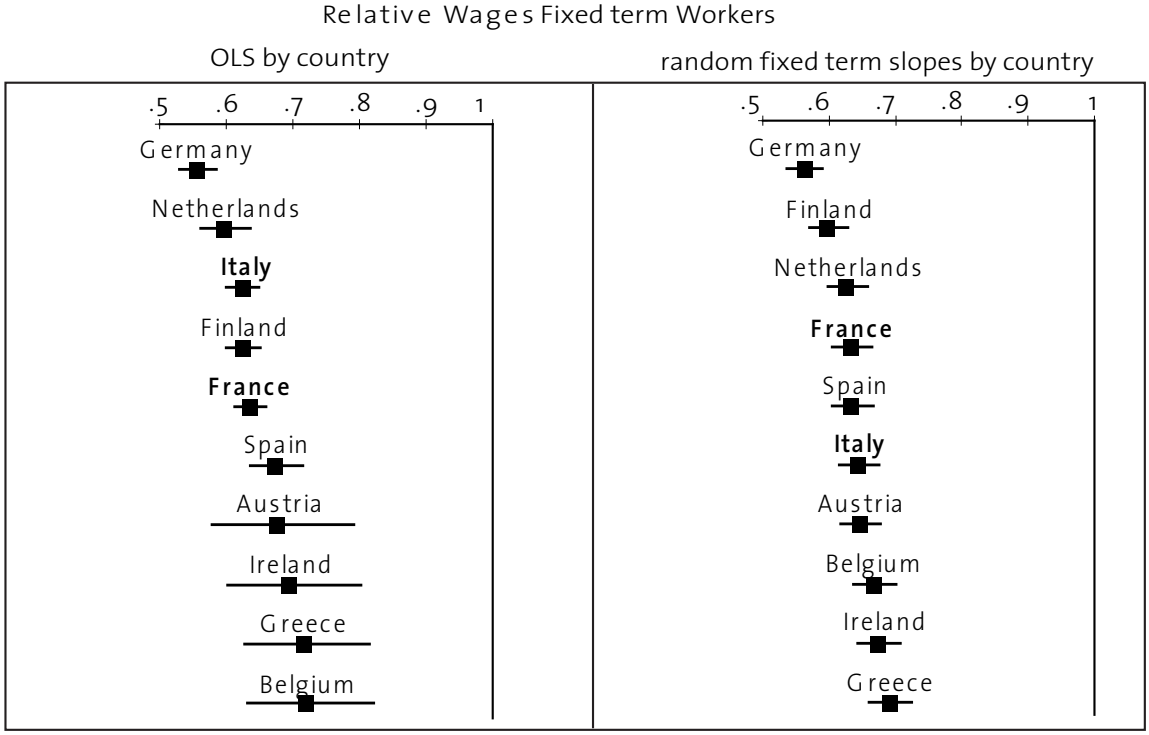

Figure 3.3: Relative wages for fixed term workers predicted by regression analyses

The regression analyses, unlike matching, also yield estimates of the controls' influence on wages for all workers. OLS coefficients (effects controlling for all other variables) indicate that a year of work experience can increase wages anywhere from 2 to 4.6% (the highest returns are in Spain) and that returns to experience slow down more quickly in Greece. Public employment largely increases earnings, except in Finland. Education increases income in all countries, although Belgium has by far the best returns to post-graduate education followed by Finland and the Netherlands. Going up the occupational ladder, salaries grow too, though the Finns seem to get the best returns to managerial posi-

tions followed by the French and the Spanish. The Finns and the Greeks have the highest returns to professional occupations. Men earn more everywhere.

The random slopes regression does not allow the effects of the other independent variables to vary across countries but it did estimate overall regression coefficients. With about 10 years of experience, an additional year of experience yields about a 3% increase in salary, men earn about 28% more than women, and public sector jobs pay about 7% more. Wages also increase with more education such that post graduates earned 43% more than those with less than a secondary education. Occupational returns increase and then decrease slightly for professionals. *All* controls were significant at the .001 level.

Surprisingly, the regression and the matching results have very similar results. The correlation between the relative effects from ATT and from the OLS coefficients for fixed term work equal to .9664 (for only 10 countries, of course). The correlation between the relative wages found using the ATT and that found using the random slopes coefficients (plus the fixed term dummy) was .8204, while the correlation between the two regressions' effects was .8911. Ranking the ten countries by ATT and the OLS coefficients, the top five countries with the worst wage penalties rank in the same order: Germany, Netherlands, Italy, Finland, and then France. The ordering changes a bit in those countries with lower wage penalties, with Austria and Spain alternatingly placed as 6 and 7, and then Greece, Belgium, and Ireland being the countries with the lowest wage gap. The random slopes model shows generally the same pattern although Italy has a smaller wage gap in this analysis, putting it alongside Austria and Spain.

3.3.3 Relative wages and institutional differences

Originally, we conjectured that those countries with strong union coverage, high unemployment rates, and strict EPL would have the largest wage gaps. While we cannot test this statistically with only 10 countries, looking at the scatter plots in figure 3.4 it seems that strict EPL is the only one of the country characteristics that strongly correlates with

wage gaps. It is worth noting, that those countries with weak enforcement of strict EPL like Italy and Greece (and possibly Spain) should probably be shifted a bit left on the EPL axis since in practice there is a freer labor market than we would anticipate based on legislation. Italy and France probably slightly over-estimate relative wages, as net wages, rather than gross, were used. Looking at the plot, it seems like there are very strong results at both extremes of the predictions. Ireland, Belgium, and Greece have weaker EPL and also high relative wages, while strict countries like Germany and the Netherlands have lower relative wages and middling countries like France and Finland are in the middle. Italy is the only country that definitively defies the trend. Even if we were to hypothesize that our measurements might be wrong in Italy (an underestimation of the gap due to using net wages or an overestimation of EPL due to ignoring enforcement), the adjustments would move even further away from the hypothesized trend. The second outlier is that given its strict EPL, we might have anticipated that Austria would have somewhat lower relative wages. Regardless, the EPL index can explain more than a quarter of the variation in the relative wage gaps between countries (compared to a baseline of 10% for a random variable).

The other hypotheses failed. The difference in fixed term and regular EPL is entirely unrelated to the wage gap and is therefore not displayed. This is not an unequivocal rejection of the theory, but could result from the fact that even though the two indices are on the same scale and both measure EPL, the underlying policies are incomparable. Other theorized predictors like union density and the unemployment rate look equally unrelated. Excluding Finland, it seems union density might actually *decrease* the wage gap, though further analysis with more countries or years of data are necessary to confirm this.

One of the most interesting panels in figure 3.4 illustrates the relationship between the ATT and the proportion of workers that are in fixed term jobs. Independent of Spain, those countries with more people in fixed term work have much *worse* wage penalties for fixed term workers. If we recall, we would anticipate that as more people are in fixed term work, and those workers are more perfect low-cost substitutes, wages should equalize. Perhaps

this process does occur, but not linearly. Fixed term workers are still a relatively small minority of the labor force in all countries except Spain. Perhaps the other countries have not reached high enough levels of fixed term work for wages to equalize. Spain, which has a significant level of fixed term work, does not follow the pattern of other countries and rather shows a small wage gap, given the high number of fixed term workers. To truly test this hypothesis we would need to use longitudinal data (following the wage gap as fixed term employment increased) in either an overall labor market or better yet, by sector.

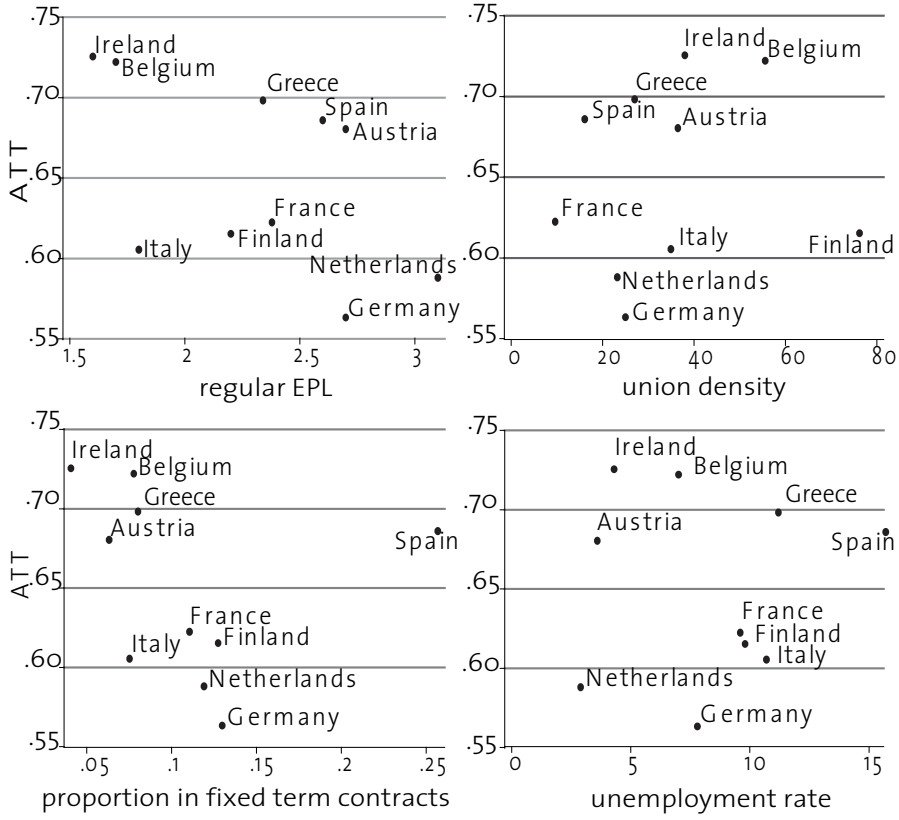

Figure 3.4: ATT by country characteristics

In sum, the regression analysis confirmed that fixed term workers in Germany and the Netherlands do significantly worse compared to their permanent peers than do fixed term workers in Greece, Belgium, and Ireland. When the labor market is freer either through the implementation of the Anglo Saxon model (Ireland), or through EPL avoidance (Greece),

fixed term workers are at less of an economic disadvantage. Other country-level variables, including the difference between permanent and fixed term EPL seem to be unrelated to wage gaps or are at least inconclusive.

While EPL effects are the major finding, we cannot be sure which mechanism is driving the gap. If we recall, there are several hypothesized mechanisms. The first hypothesis was the selection of lower skill workers into fixed term work under strict EPL. The matching model and control variables in the regression were designed to control for this selection. The second possible mechanism was another sort of selection story, in which firm and industry distributions interact with EPL strictness. The third possible mechanism was that strict EPL creates an insider-outsider regime where fixed term workers are paid less. Since the actual mechanism is uncertain, we cannot definitively say that strict EPL *causes* more inequality through fixed term contracts. However, assuming that this study properly accounted for selection, and assuming the third hypothesis plays at least some role, it very well might be that EPL generates more inequality than would exist otherwise.

3.4 Fixed term employment policy

EPL seems to generate a two tier system of permanent workers with job security, union coverage, and higher compensation compared to atypical workers with none of those benefits. This leads us to two questions. First, what is the best approach to equalizing the disparity between these two types of work and second, is there a general solution applicable to all countries? Of course, the idea of different solutions for different countries makes sense; each country has its own unique conditions. That said, the EU has an increasingly integrated labor market. There is already evidence that some countries, like Italy, are losing many of their young educated workers to other countries. Given that fixed term workers are disproportionately young it seems likely that unequal EPL could contribute to this problem. Given the free flow of workers across Europe, a uniform solution, at least for Europe, might

be desirable.

The first oft-proposed solution to the two tier problem is encouraging transitions from fixed term work to permanent work, as in Spain where there are incentive payments for firms converting fixed term contracts to permanent, or through automatic transitions after a period of fixed term work. This is the strategy proposed by the EU in the 1999 EU Directive 99/70/EC. The directive followed the form of pre-existing legislation in some member states. The directive required countries to specify who is covered by the law, whether fixed-term work must be justified by employers, the maximum number of consecutive fixed term contract renewals allowed, the maximum length of each contract, and the maximum cumulative duration of successive contracts. Country implementations vary dramatically (Ireland's Protection of Employees Act 2003, Spain's Reforma Liberal 2006, England's Fixed-Term Employees (Prevention of Less Favourable Treatment) Regulations 2002, Germany's Gesetz über Teilzeitarbeit und befristete Arbeitsverträge, and the Netherlands' 1999 "Flexicurity" law to name a few). Some countries, like Denmark, chose to circumvent the law by excluding any workers represented by unions (the vast majority of Danish workers). Other countries, like Austria, took an extreme approach, limiting fixed term contracts to three months. Still others, like Italy, drafted simply inexplicable laws, excluding specific industries like agriculture, tourism, and catering from the legislation. By far the most common approach was to limit the total cumulative duration of contracts to around 2 to 3 years.

The results of this study suggest that this is the wrong approach since wage gaps seem to be more related to permanent EPL than fixed term EPL. Further, the directive's approach of limiting fixed term work has been criticized as inadvertently encouraging firms to fire those workers approaching their time limits rather than hiring them as permanent workers, consequently increasing worker turnover (Blanchard and Landier, 2001). To counteract these possible perverse incentives, Spain offers the aforementioned contract conversion incentives, though this is also prone to abuse, as it encourages firms to initiate all employment

in fixed term contracts. Perhaps the clearest critique of the EU approach is voiced by Eichhorst and Konle-Seidl (2006) who argue that as long as *any* EPL gap between the two types of workers remains, fixed term jobs will be bad jobs.

Another oft-discussed solution is flexicurity (see paper 1 (chapter 2) for a longer overview). There are three main aspects of flexicurity. First is the removal of EPL protecting workers from dismissal. Second is the implementation of a general social safety net including primarily generous unemployment insurance but any shift of non-salary compensation costs from the firm to the state (health insurance, pensions, and so forth). The third key component of flexicurity is worker retraining and continued learning. This includes programs that offer education while workers are still employed as well as after separation. Theoretically, the flexicurity approach would have no legal separation of workers by contract type, and all workers would be bargained for by the same unions. There are several challenges to the implementation of this approach. First, many fear that the idea of flexicurity can be manipulated to deprive workers of their protections (the first aspect) while never granting them the increased safety net (the second aspect). Another challenge is that the implementation of training (the third aspect) is organizationally difficult and requires responsive workers who are willing to actively participate. Finally, many argue that this approach works better with a homogenous population that supports high taxes and redistribution (as in Denmark).

While the EU approach and the flexicurity approach are the most often discussed methods to counteract the two tier system, there are others. One is simply creating a slow progression of worker protections. Most countries already have this to some extent; workers beginning their careers have less protections than workers ten years into their careers.

Also, simply removing the possibility of a fixed term work contract is a seldom discussed, but clearly feasible, option. Presumably this solution is avoided because there are instances in which firms legitimately prefer fixed term contracts, for example during unanticipated periods of activity. Another approach is to simply remove permanent workers' protections moving to a free Anglo Saxon type labor market. Some countries have already

begun to slowly reduce permanent workers' protections moving toward more uniform contracts (i.e. Germany's Hart Reforms) although many countries are loath to remove workers' protections.

Others propose policies more specific to fixed term employment like "flexsurance," which works like the US's unemployment insurance system in which employers with regular layoffs pay more. In this case, firms using more flexible contracts pay more into a social fund that insures fixed term workers against periods of unemployment. Another proposal is modeled on Austria's 2002 new severance pay. Under this scheme, severance payments are slowly built up as firms regularly pay 1.53% of gross wages per month into a severance payment account. Consequently, at the time of dismissal there's no distortion of firms' choice of whether or not to dismiss a worker. Upon dismissal, the worker can access the fund while looking for a new job. At the end of the worker's career, the fund becomes part of his retirement and he/she can cash it out or take it as an annuity. While this seems innovative, it is not actually a new program. Increasing social security payments by 1.53% and allowing workers to access these funds upon dismissal would accomplish the same goal. A disadvantage of this model is that it gives high skilled workers yet one more advantage. The less skilled are more likely to be laid off and have longer spells before finding their next job, meaning that the program amounts to simply more retirement funding for the well-off. In sum, there are various approaches each with their advantages and disadvantages. The flexicurity approach is, justifiably, a much-discussed option.

3.5 Conclusion

This paper is the first consistent cross-national estimation of the fixed term wage gap. The first finding is that there is a significant disadvantage for fixed term workers in *all* countries. Because the results cannot clearly determine the mechanism of this wage gap and because some selection might not have been accounted for, we cannot definitively prove that these

same workers would be better off in permanent employment. However, the results are suggestive of the idea that unequal contract types might be an important cause of labor market inequality. The second finding of this paper was that the extent of the fixed-term disadvantage differs by institutional regime and that a more liberal EPL regime seems to minimize the wage gap. Thus, it may be wise to develop consistent EPL that does not generate a two-tier system and to find a consistent solution for the whole European labor market.

Future research should follow up on this analysis, adding comparisons with Denmark and the US, as data become available and should examine the impact of the new implementations of the EU directive, as these changes will certainly alter the incentive structure around fixed term work and possibly alter wage gaps. Finally, in many countries like the US, wages are only a small portion of the disadvantages faced by fixed term workers, dwarfed by the differences in non-wage compensation like health insurance. Future work should expand to examine the size of non-wage compensation gaps. Despite the future work that needs to be done, this was an important first look at international variation in fixed term workers' relative wages.

Chapter 4

Employment Intermediaries: A model of firm incentives

4.1 Introduction

Temporary or or contingent work has supposedly spread in recent years (Clinton, 1997; Kalleberg et al., 2000b; Kalleberg, 2000a; Mangum et al., 1985) raising concerns about their effect on social stratification, as these workers have lower pay, fewer benefits, less on-the-job training, and less job satisfaction (Booth et al., 2002; Kalleberg et al., 2000b; Houseman et al., 2003). "Temp work" or outsourced work means that a firm hires a worker through a second firm. The second firm remains the worker's legal employer, although the worker physically works at the firm that purchased his or her services. (Throughout this paper I refer to these workers as "indirect employees.") Empirically, temp work can be difficult to distinguish from direct employment at firms that provide services to other firms (such as accounting) but where the worker still physically comes to work at the direct employer's. In the US economy, the purchase of labor services, temp work and otherwise, is growing rapidly. In the past decade, firms increased their purchases of services more than they increased direct hires, with the consequence that business services grew at a rate of 5.8% every year from 1988 to 1997, twice the rate of the rest of the economy

(Clinton, 1997).[1] The fastest increasing sub-sector within the business services category is the temporary help industry, which grew 11% annually from 1979 to 1995, five times more quickly than all other non-farm employment (Autor, 2000).

This paper presents an agent based model (ABM) of job search and job-worker matching in a labor market with intermediaries.[2] The general term "intermediaries" is used here to refer to both "temp agencies" and "contractors" because the functional difference between contracting labor through a firm like a temp agency and simply outsourcing is difficult to distinguish; the difference largely has to do with the implicit length of the contract, the worker's skill, and the level of integration into the primary firm. The model describes how firms might adjust their use of intermediaries in response to (dis)incentives such as intermediaries' better ability to match workers with jobs and tests whether these hypothesized incentives could be one explanation for the patterns of intermediary use in today's US labor market. It further examines how these incentives function differently in different occupational labor markets. This model does not look at wage gaps as an outcome of organizational decisions (only as a determinant) and (like most simulations in the social sciences) does not provide proof that the tested scenarios are necessarily the definitive explanation, but rather, that they are one feasible explanation. Finally, it includes several important assumptions about indirect hiring that will become clear in the explanation of the model.

The model's results suggest that intermediaries can provide a valuable service to firms by increasing the firm's capacity for searching for new workers. The model also finds that incentives to use intermediaries differ depending on the skill variability of workers in the occupational labor market. In addition, organizational ecology is very important in firms'

[1] "Business Services" is a Bureau of Labor Statistics category including: advertising and public relations services; computer system design and related services; employment services; management, scientific, and technical consulting services; and scientific research and development services.

[2] Those unfamiliar with ABM might see Macy and Willer (2002) for an introduction. ABM is a simulation composed of interacting agents that follow micro rules, generating macro system behavior. In this article, the micro agents are firms, jobs, and workers (the basic elements of a labor market) and macro system properties are employment rates, vacancy rates, and other aggregate labor market indicators.

decision to use intermediaries and finally, in an environment where firms pay a percentage of salaries as a fee to intermediaries, lower-skilled jobs are sorted into indirect employment.

4.2 Background

One of the primary concerns about atypical employment is that these workers have lower compensation (Booth et al., 2002; Segal and Sullivan, 1997; OECD, 1999). Figure 4.1 shows the annual wage-gap between direct and indirect workers for four occupations in the US using the March Current Population Survey (CPS).[3] The figure shows that while indirectly employed janitors and clericals are consistently paid lower wages, indirect programmers and accountants earned lower wages only until the mid 1990's, after which they fluctuate around the same level as regular workers. Plotting total income rather than wage income (not depicted here), indirect programmers and accountants consistently earn *more* income than their direct-hire counterparts. This could be because these workers have secondary income sources such as independent contracting. Indirect hires in all occupations are consistently less likely to receive health insurance from their employers as illustrated in figure 4.1, where the line indicates the difference between the proportion of regular and indirect workers with employer-provided health insurance. Note that some of the workers that are counted as "uninsured," actually have insurance through a secondary source such as a spouse's employer-provided health-insurance scheme. Insurance through a spouse is more likely for high-skill workers who are both more likely to be married and more likely to be married to a partner who has health insurance benefits. To date, evidence suggests that differences between direct hire's and indirect hires' individual characteristics (work effort, education, residential location, age, and gender) do not fully explain the compensation gap. In fact, CPS data suggest that indirect clerical workers should have higher compensation

[3]There is a discontinuity in coding in 1992 and 2002. Indirect workers are identified by matching industry and occupational codes for workers so that a secretary working for a clerical services firm is assumed to be an indirect employee. This method captures some temp workers, some workers employed through intermediaries, and some workers simply working at service firms.

than their direct-hire counterparts, given that they have more education and reside in more urban areas, two characteristics normally correlated with higher wages. Because of indirect employment's significantly lower wages, many researchers assume that the firm's primary incentive to use intermediaries for firms is to save money on compensation, particularly in low-skill occupations (Houseman et al., 2003; Kalleberg et al., 2000a).

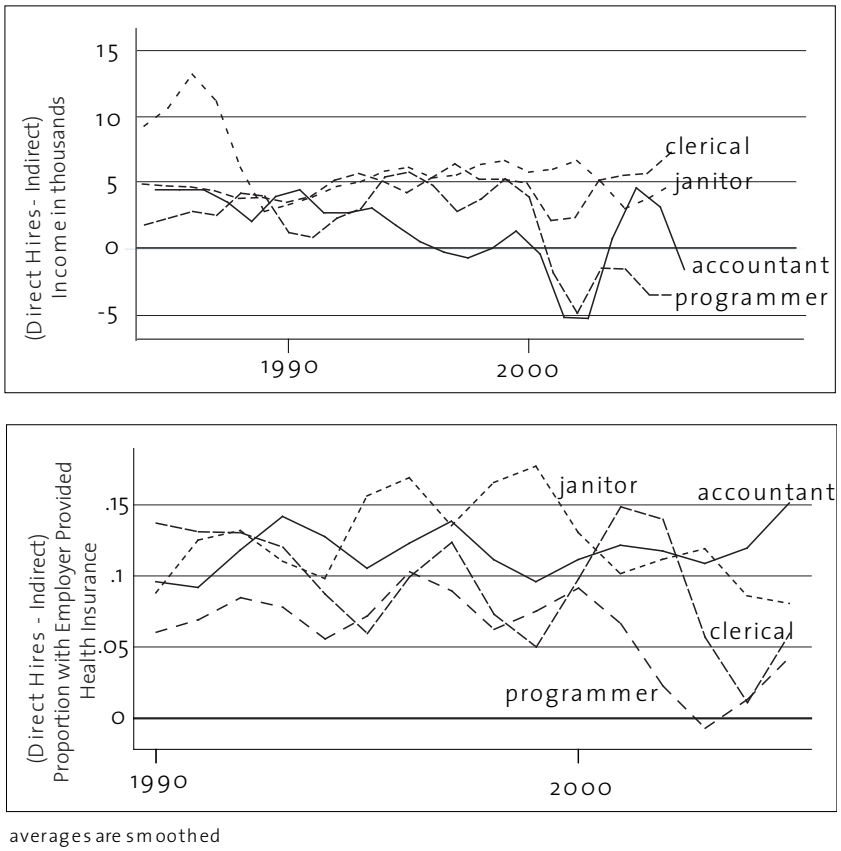

Figure 4.1: Indirect hires' relative compensation (King et al., 1983-2006)

There are many hypothesized incentives for firms to use intermediaries. In contrast to those arguing that intermediaries are about reducing compensation, some researchers argue that firms *underpay* direct-hires in the high-skill labor market, using indirect employees as a temporary substitute while searching for permanent employees willing to accept lower

wages (Houseman et al., 2003).[4] Another potential benefit is that intermediaries could match workers and jobs more efficiently (Katz et al., 1999), decreasing the firm's human resources expenses. Other hypothesized incentives include: maintaining a flexible labor force, testing low-quality or risky workers, hiring specialized workers for short periods, increasing employee-job match quality, and focusing on firms' core competencies (Deavers, 1997; Gramm and Schnell, 2001; Abraham, 1990; Mangum et al., 1985; Mayall and Nelson, 1982; Young and MacNeil, 2000; Davis-Blake and Uzzi, 1993; Osterman, 1999; Pfeffer and Baron, 1988). In addition, in high-skill positions, indirect hires might be more productive working at the intermediary than with their occupational peers (for example a programmer employed at a firm customizing software, rather than the firm using the customized software). On the other hand, there are also many hypothesized disincentives to using intermediaries including the importance of firm-specific skills, intermediaries' fees, large firms' ability to internally smooth labor consumption, and union regulations prohibiting hiring workers through intermediaries.

The most cited incentive in the US context is primarily that intermediaries allow firms to reduce compensation costs, though not primarily through wages but rather by cutting health insurance costs. In the United States, this incentive is embedded in the tax structure: there are federal tax incentives for businesses to provide equal health benefits to all their employees. Firms can qualify for these tax incentives, despite denying part of their workforce health insurance, if they purchase *services* instead of labor, using only direct-hires (who all have health insurance) in the tax calculation.[5] From the employee side, some re-

[4]While researchers claim that the incentive is saving on compensation, firms themselves disagree. The National Organizations Survey directly asked firms' human resources officers why they hire through intermediaries. HR departments responded that their firm does it primarily because of work fluctuations and because contractors' have specialized skills. Most responded that it does not lower costs.

[5]The US tax code offers businesses tax deductions for health insurance and pension expenditures. However, these tax deductions are only available if these benefits are not provided in a way that favors high skill workers. US Code Title 26, subtitle A, Chapter1, Subchapter D, PartI, Subpart A, Section 401 a(4) states that deductions are granted:

> if the contributions or benefits provided under the plan do not discriminate in favor of highly compensated employees (within the meaning of section414(q)). For the purposes of this paragraph, there shall be excluded from consideration employees described in section 410(b)(3)(A) and (C).

searchers find that high-skill indirect hires receive the same total compensation in the form of fewer benefits but higher wages; presumably workers with spousal benefits might seek contract work to cash out their benefits (which is illegal in regular employment contracts) (Houseman et al., 2003). While from the worker's perspective, it seems clear that firms save money on compensation, there is actually mixed evidence whether, including the cost of intermediaries, firms actually save money (Benson, 1999; Young and MacNeil, 2000; Mayall and Nelson, 1982; Mangum et al., 1985; Deavers, 1997; Davis-Blake and Uzzi, 1993). However, studies do find that those firms with higher wages are more likely to contract out work (Abraham, 1990; Gramm and Schnell, 2001), presumably because indirect employees do not receive those higher wages.

This paper uses a micro-simulation (an agent based model) of the labor market to study the question of which firm incentives could motivate the use of intermediaries. The simulation first matches worker and jobs in a free labor market, looking at the resulting labor market dynamics like unemployment and vacancy rates. The model then introduces intermediaries, tests different incentive theories, and looks at the subsequent overall levels of intermediary use. The mechanism that matches jobs to workers in this artificial labor market is based on the Gale-Shapely marriage matching algorithm (Gale and Shapely, 1962). In this algorithm, men and women rank each other as possible mates. Then, men propose to their highest ranked woman. If they are rejected, they propose to their second choice, and so on. Woman accept proposals if they do not already have a partner or if the new offer is preferable to their current partner. Their prior (jilted) partner must then propose to the next highest ranked woman on his list. Given an equal number of men and women, this algorithm is proven to find a stable solution where everyone is matched and no man and woman would rather be with each other than their current partner (Gale and Shapely, 1962). The solution is optimal for men, leaving them matched to their highest-ranked fea-

The definition of "highly compensated" is regularly updated and was changed twice while this paper was written. It is defined, generally, as employees earning over some threshold or constituting some top percent of the firm's workforce. The consequent penalty is that firm expenditures on pensions, health insurance, and life insurance are taxed 15%.

sible partner. In the simulation, companies are equivalent to men making offers to workers, instead of to women. The model was implemented with firms playing the male role because I assume that workers generally apply to a broad array of jobs while firms make proposals to individuals chosen from large applicant pools.

There are several labor market models using similar simulation methods. The most similar is Stovel and Fountain (2003), which explores Granovetter's "strength of weak ties" theory (Granovetter, 1973), testing whether workers are more likely to be matched to their jobs through their close friends or through their acquaintances ("weak ties"). Stovel and Fountain test how the shape of a social network limits information in the labor market and affects the quality of worker-job matches. Tesfastion (2001) uses an extension of Gale-Shapely in an agent-based model, testing whether the ratio of jobs to workers or of firms to workers is more important in allocating negotiating power. Tassier and Menczer (2001, 2005) used social networks in a job matching model similar to Stovel and Fountain's, first examining how networks evolve through job matching, and second assessing how employment rates vary between social groups as a function of their network structure. Other models use job matching algorithms to examine frictional unemployment rates (Hosios, 1990), many-to-one matching (Echenique and Yenmez, 2005), or matching in wage posting games (Montgomery, 1991; Peters, 1991; Shi, 1998). Other abms matching workers and jobs include (**?**Neugart and Storrie, 2006; Richiardi, 2003) To date, there is no implementation of these methods examining the role of labor market intermediaries.

4.3 Model

The model describes the spread of intermediaries, focusing on four labor market scenarios. The model is laid out on a 2-D grid with four types of objects on the grid: firms, jobs, workers, and contractors. Firms and workers stay in fixed locations for the duration of a simulation, while jobs and contractors appear and disappear. Two sets of experiments vary

a total of seven parameters, with the parameters related to hypotheses about tradeoffs between incentives and disincentives to use intermediaries. The parameters are listed in the appendix and control the following (dis)incentives: 1) intermediaries' better screening capabilities 2) intermediaries' fees 3) compensation differentials and 4) workload variability. The decision to use an intermediary for the next vacancy is based on a utility function that firms regularly update, measuring whether they have had greater utility from past direct or indirect hires. The primary model output is the level of intermediary use under each scenario, though the model also measures the unemployment rate, vacancy rate, firm utility, and job and vacancy duration, most of which are used to qualitatively tune the model using empirical data. The model has several underlying assumptions, which are detailed after a description of the model's algorithm.[6]

The model's algorithm is illustrated in Figure 4.2. The broad overview is: first workers, firms, and jobs are created; then, workers are matched to jobs using a variation of the Gale-Shapely algorithm; next, workers and jobs suffer separations; and finally, contractor arrangements are updated. Then the model starts the process again starting with the matching step.

In each run, there are 1000 workers and 138 firms. Jobs are assigned to firms in a skewed distribution with most jobs at a few firms but no firm having more than 10% of the jobs.[7] Workers are created with skill levels sampled from four empirical distributions of occupational educational attainment (general, minimum wage, programmers, and accountants) and are assigned a skill floor (a random uniform deviation below their skill level, indicating the worst job they would accept). Distributions come from BLS and CPS data, and are depicted in figure 4.3. Jobs are assigned skill levels and floors using the same methods as for workers, but for jobs the skill floor indicates the worst *worker* the job would accept. Workers have a location on the grid, an employment status, an employer and job

[6]The model was programmed using Java with the Repast library. Code is available.

[7]The function assigning jobs to firms (C.1 in the appendix) determines the ratio of the number of jobs to the number of firms. Since the model should start with approximately 1,000 jobs (to match the workers), the number of firms were chosen accordingly. Thus there are exactly 138 firms.

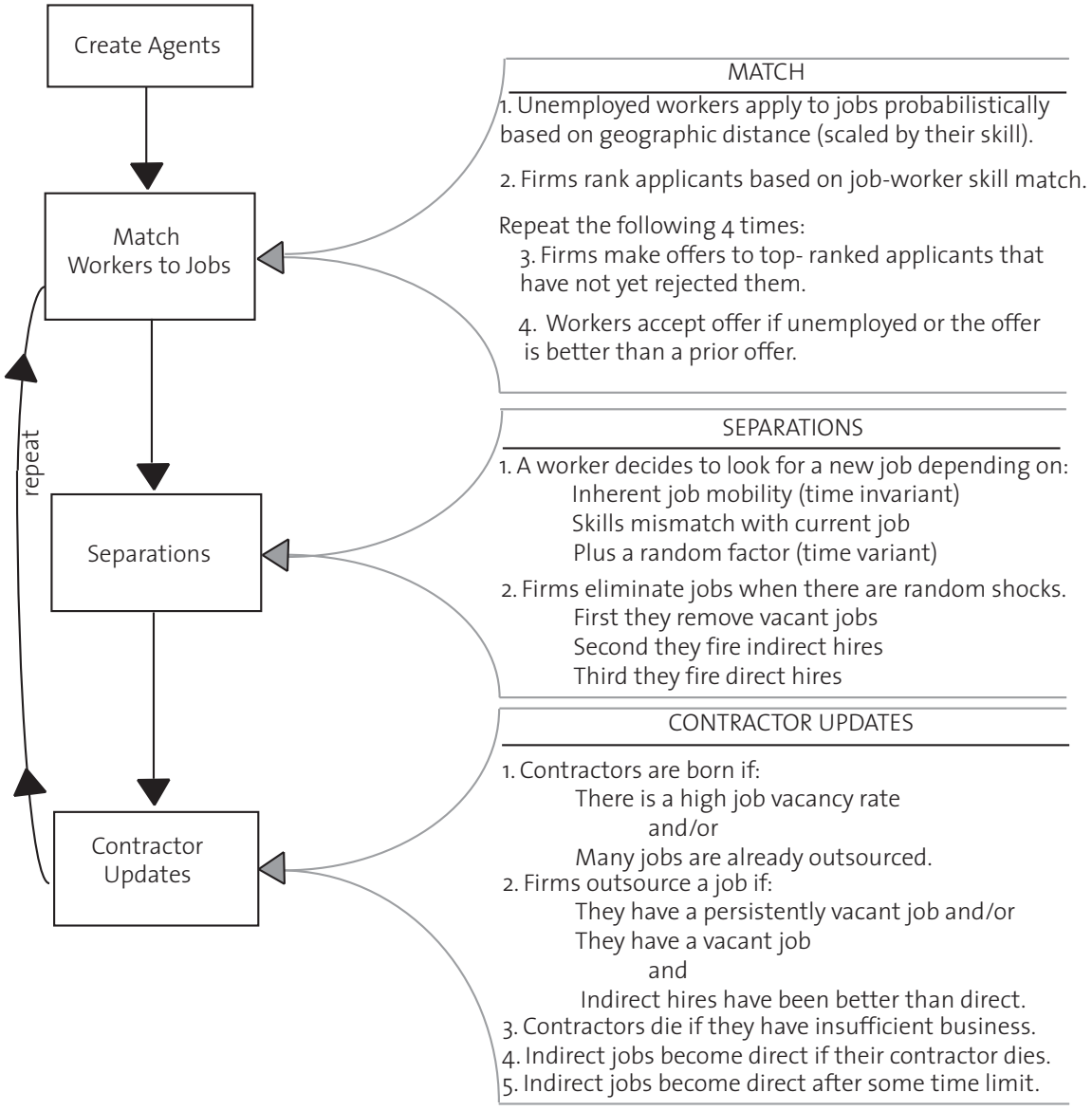

Figure 4.2: Program structure

(when employed), a contractor (if using one), the date they were last employed (if they are unemployed), a random inherent tendency to quit that is time-invariant, and their relative wages when hired through an intermediary. Relative indirect wages is the average percent of a direct hire's wages that indirect hires get, ranging from 90 to 110%. These relative wages are reassigned to a worker each time the worker is hired through a contractor. Firms have locations, jobs (vacant and filled), a contractor (if they are using an intermediary), employees, and a history of their current and past utilities from their direct and indirect jobs. Jobs have skill levels and floors, a firm, an employee (when they are filled), and dates marking the last time they were filled or vacated. Contractors have assigned jobs, workers, vacancies, fee-rates, revenues, and matching rates.[8]

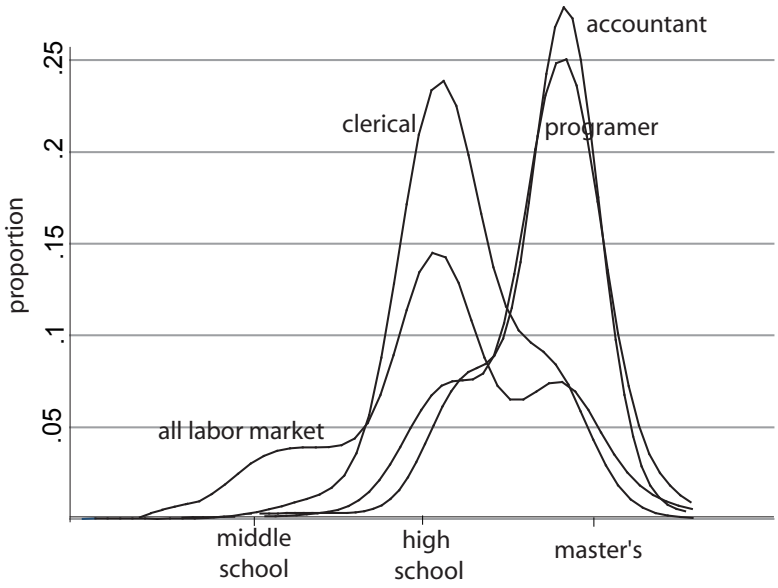

Figure 4.3: Workers' educational distributions

When the model is initialized, all workers are unemployed, all jobs are vacant, and there are no contractors. Workers sort through vacant jobs, calculate the distance to each job, and apply to closer jobs with a higher probability. Workers ignore the match between their skill and the job's skill when sending out applications. This is an unrealistic assumption in those

[8] All equations, variables, and parameters are listed in the appendix.

simulations testing the overall labor market but is a realistic assumption in those testing occupational labor markets. Because the workers apply to so many jobs, this assumption does not make the matching in the overall labor market simulations ineffective; workers still apply for a significant number of jobs they will be considered for. The advantage that contractors have is that workers perceive those jobs hiring through contractors to be closer, and are thus more likely to apply. This distance effect should not be taken at face value, but rather proxies for contractors' better human resources capabilities; they advertise more widely and screen through more applicants. High-skill workers also search in a broader radius than low-skill workers. The formulae are specified in detail in the appendix but the general magnitude of the effect is such that all workers apply to adjacent direct-hire jobs with a 100% probability; a graduate-level worker applies to direct-hire jobs at the furthest possible distance across the grid with a 7.7% probability, while the high-school graduate applies to the most distant job with a .09% probability.

Next, firms rank applicants based on the match between the vacant job's skill and the prospective employee's skill, and then offer the job to their top applicant. Workers accept jobs "tentatively," meaning that they accept with the option of taking another offer during the same matching round, just as women in the Gale-Shapely algorithm can dump a suitor. Firms have four chances to make offers in a single matching round. When a round ends, workers must stay with their last job. The limitation of four offers prevents perfect matching, thus maintaining unemployment and vacancies. If the stock of jobs were not constantly changing, there were no skill floors, the offer process were iterated until matches were stable, and there were equal numbers of workers and jobs, there would be no unemployment or vacancies. While workers are forced to make binding commitments at the end of each round, the "tentative" jobs give them a chance to "think over" an offer or wait for another offer to come in before deciding. This is more realistic than the standard model which forces workers to accept their first offer.

After workers and jobs are matched, there are quits and fires. The quit function (again,

in the appendix) is the sum of three effects. Workers are more likely to quit if they are poorly matched to their jobs, have a high inherent tendency to quit (a time-invariant trait drawn from a normal distribution that could be considered analogous to marital status, age, etc), and if they receive a random shock (a time-variant trait drawn from a random distribution). When a worker "quits," he or she might be matched with the job they just left, since both re-enter the matching pool. As such, "quitting" includes on-the-job search. Direct and indirect hires quit using the same algorithm. Firms fire workers when they suffer random workload shocks (adding or removing jobs) which are proportional to firm size. Shocks are not correlated across firms (economic downturns) but because of the skewed firm size distribution, a negative shock to a big employer strongly influences the overall unemployment rate. When firms fire workers they first remove vacant jobs, then fire indirect hires, and finally fire direct-hires. Firms fire without respect to tenure or match quality.

After matching and separations, the model updates contractor dynamics. Up to two new contractors can be born in a single model step. The first is born if there is a high vacancy rate and the second is born if there is high demand for existing contractors. This represents a continual low level of contractors randomly placed on the grid. The new contractors are allowed to survive for less than 1% of the model duration (presumably on startup capital) before they are forced to meet a revenue threshold. Revenue is calculated as the sum of fee rates times their worker skills divided by the total number of workers they are assigned. If a contractor is earning, on average, less than 10% of the average worker's skill per assigned worker, they are removed from the model. Thus a contractor's health depends on both their ability to match workers and their fee rate. Because contractors are continually born and each has a different number of clients, the number of contractors is not representative of contracting trends but is representative of the service availability because of the way firms

find intermediaries.[9]

Firms decide to use intermediaries the first time when they have a persistently vacant job. This is the trigger that introduces intermediaries into the model. Without this element there is no basis for firms to manage future decisions to use intermediaries. However, using *only* this motivation almost no firms use intermediaries in the simulation. This really just serves to introduce the use of intermediaries into the model, so that firms can calculate a preference between indirect and direct hires. The first time a firm looks for an intermediary they look within a local radius, choosing the one that has the best job-worker match rate. Firms ignore the quality of the contractors' matches and their fees when choosing an intermediary, but do consider it in their utility equation, which is later used to determine whether the firm will use an intermediary for another job. Once a firm has experience with an intermediary, they turn over the next open job to an intermediary when their past utility from indirect hires is greater than their past utility from direct hires. Utility is specified two ways, both outlined in detail in the appendix. In both cases the firm assesses its utility using a weighted history function, weighing its more recent hires more heavily than the older. In the first experiment half of the utility is match quality. Match quality measures the distance between the job's skill and the worker's skill. The optimal match is when the two skill levels are equal. Dissatisfaction is asymmetrical such that the firm would rather have an overqualified worker than an equally under-qualified one. The second half of utility is related to the cost of using an intermediary, or the match fees, which are measured as a percent of the worker's skill. The second experiment uses the same concept where half the utility is related to cost and half is related to match quality. However, in the second experiment costs includes not only fees, but an adjustment for the indirect worker either having a higher or lower salary. Fees are charged on the adjusted salary.

Firms use the same contractors until they either bring their last indirect job in-house

[9]Contractors are designed as simply as possible with exogenous fee rates, no contractor profit maximization, and so forth, as the model is not measuring contract agency dynamics, but rather the growth of the contracting relationship. For a mathematical model with more realistic intermediaries see Neugart and Storrie (2006).

or the contractor goes out of business. When the contractor goes out of business, the firm finds a new contractor the same way they found the first one, and continues to use the utility from the prior contractor in historical utility calculations. If the firm cannot find a contractor, they hire directly. Finally, indirect hires that have been at the same firm for more than four periods automatically become direct hires.

The algorithm description included several underlying assumptions in the model's mechanics, some of which are varied in the following experiments. The first assumption is that firms are more likely to fire indirect hires than direct. This setting is premised on the fact that research finds that atypical workers are more likely to transition to unemployment than traditional hires (Corsini and Guerrazzi, 2007; Amuedo-Dorantes et al., 2006; Garcia-Perez and Munoz-Bullon, 2005) and that most OECD countries have stronger limitations on dismissing permanent employees than indirect or short term employees (OECD, 1999, 2003). According to the national organizations survey, firms also report using indirect workers as an adjustable labor force, firing them when demand declines. The second assumption is that when indirect workers are not fired, after some period they must become permanent employees. Temp workers are usually sent to a new assignment, or if they stay on at the same firm, they are hired permanently. For this reason, temp contracts often have clauses specifying a fee the firm will pay the temp agency if they hire the worker. This model is designed to reflect that temp workers cannot remain in the same job as a temp worker indefinitely. They must switch jobs or become permanent hires. The third assumption is that high-skill workers conduct a broader job search than low-skill workers.This assumption is supported by evidence showing that the longer a worker is unemployed (generally less skilled workers), the less likely he/she is to relocate for a job (Herzog et al., 1993) and is also the premise of the spatial mismatch literature that argues that poor inner city workers do not search for or find employment in the suburban ring (Kain, 2004). Further, low-skill workers are generally found to move less for jobs. Fourth, throughout all experiments the utility calculation assumes that firms value match quality, and like to pay less

for their workers, both in fees and wages. Finally, in terms of match quality, the model assumes that both workers and firms prefer to find a job/worker that is a perfect match, but that a worker would rather be somewhat under-qualified for his job (this assumes some sort of ambition on the part of the worker), while the firm would prefer a slightly over-qualified worker.[10] The assumptions listed here are not tested, as they are based on a combination of the sociological literature and common sense.

4.4 Experiment One: Fees vs match quality

The first experiment examines a trade-off that firms face between intermediaries' ability to better sort through workers and the fees they charge for their services. The experiment finds that non-wage incentives can be sufficient to encourage the use of intermediaries. There are two parameters controlling these dynamics. One parameter controls intermediaries' ability to screen more workers. This parameter makes intermediaries' jobs appear closer to workers, thus more workers apply to these jobs, and the intermediaries are more likely to make better matches. The parameter ranges from 1.0 to .1 (10 settings), where the worker sees a contract job with the same probability as a regular jobs at 1.0 and where the contract job appears twice as close at .5. To avoid confusion, the reader should be reminded here that this distance effect is a proxy for the breadth of the worker's job search and that contractors' ability to "shorten" the distance measures their ability to sort through more applicants. The differences between a broad skill distribution like the "minimum wage" distribution and a narrow one like "accountants" captures the importance of credentialing in an occupational labor market, since it narrows the skill distribution. The second parameter sets contractors' fees, ranging from 5 to 35% of the employee's skill level (7 settings). All combinations

[10]Note that the firms' preference for over rather than under-qualified workers also enters the firm's utility function. Firms' utilities (though not their hiring decisions) are also influenced by wages. Wages are based on the worker's skill (rather than the job's or an average of the two). This means that when the firm hires someone, it just wants a perfect match and prefers the over-skilled to the under-skilled, but when they calculate their future decisions to use intermediaries, they still value match quality the same way, but are also happier with lower skill (cheaper) workers.

of the parameters were run (70 combinations) 20 times each. The consequent transition to using intermediaries under three parameter settings is illustrated in Figure 4.4 and the final proportion of jobs filled through intermediaries at all parameter settings is illustrated in figure 4.5. Considering standard error (not depicted in figure 4.4) the levels are statistically different for the displayed runs after the 100th tick (a model's "time" element). Figure 4.4, shows that the transition to using intermediaries is rather abrupt; as soon as intermediaries are available, firms rapidly adopt. When intermediaries are most appealing (with lower fee rates and greater search radius enhancement) the transition is quicker.

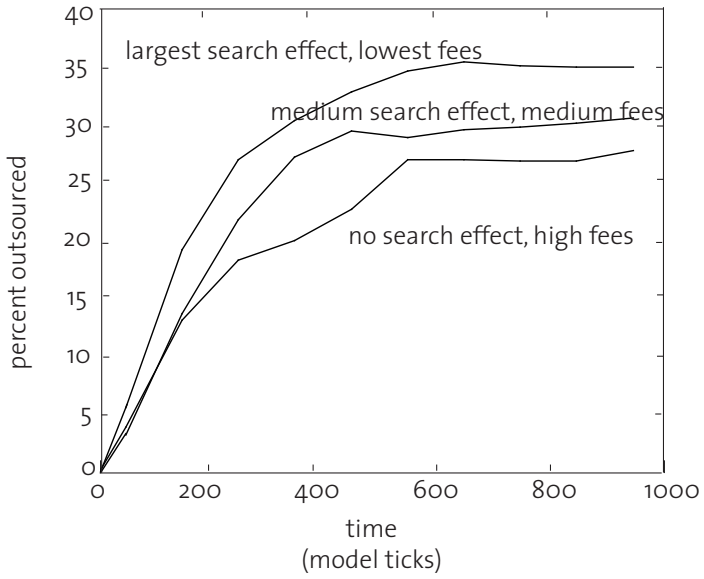

Figure 4.4: Transition to using intermediaries, mean of 20 runs per scenario

The contour plot, figure 4.5, illustrates the final proportion of jobs matched through intermediaries at all parameter levels. Higher fees discourage firms from using intermediaries while their ability to screen more applicants increases it. Even when there is a low search radius effect and high average fees, firms still use intermediaries over 25% of the time in the first experiment. This comes about for two reasons. First, fees are assigned from a distribution. This means that even if on average fees are high, there are some intermediaries that are cheap. Second, firms base their current decisions on their personal utility histories.

Even with no systematic advantage, intermediaries will create better matches 50% of the time. Thus, even when the overall model settings are not advantageous to intermediaries, individual firms can have positive experiences with intermediaries. This is realistic in that firms often make myopic decisions based on their experiences and even though a service might, on average, not be advantageous. Figure 4.5 suggests that the level of workers hired indirectly at the end of the model decreases steadily as intermediaries' fees go up and increases as intermediaries' search ability increases. As parameter settings move towards the most attractive intermediary scenarios (with low fees and good matching) there are two pockets of higher levels of indirect hires and a small pocket of low levels, though overall the relationship is monotonic with the pockets not deviating more than 1.5 percentage points from the surrounding area.

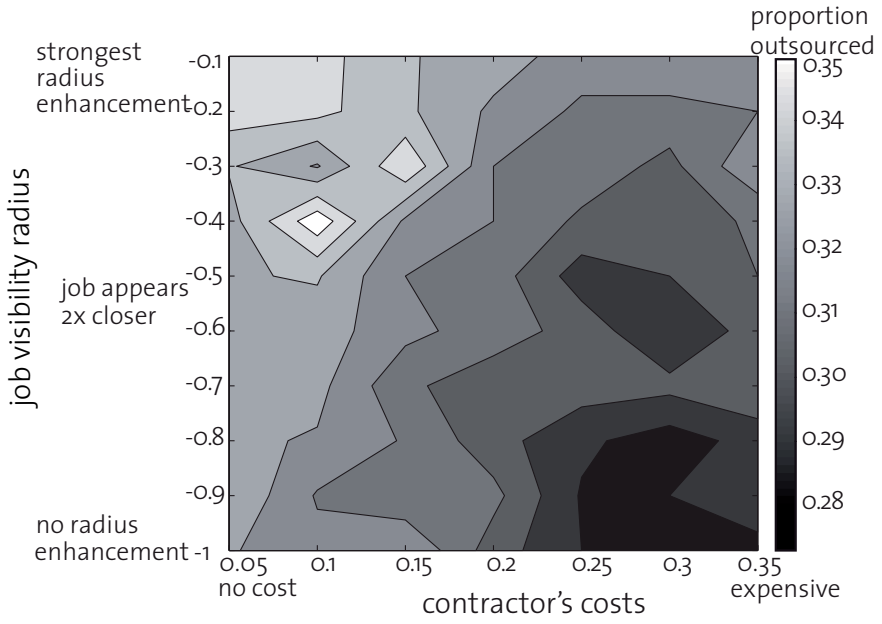

Figure 4.5: End of run proportion of jobs matched through intermediaries

Other model outputs behave as anticipated. Firms' average utility increases when they use intermediaries, increasing 37% with medium search enhancement and medium fees or 100% with the lowest fees and highest search enhancement. Utility gains are not monotonically related to parameter settings, with a contour plot (not illustrated here) showing

some discontinuities in utilities when radius enhancements are low but fees are in the middle range. Using intermediaries is also associated with lower unemployment, primarily because they give hard-to-match jobs and workers a better chance to find a match. An OLS regression predicting the simulations' unemployment rates suggests that the effect is significant though small: when there is a nine point increase in the percent of jobs filled through intermediaries, unemployment declines almost a full percentage point. With respect to skill, contractors are more likely to match slightly less skilled workers, although the model design suggests that intermediaries should match workers at either extreme of the distribution, as both will have trouble finding a good match.

There were several important findings from this experiment. First, when intermediaries have better matching capabilities, they improve firms' utilities and reduce frictional unemployment. Second, match quality is a sufficient incentive, in the face of significant intermediary fees, for firms to use intermediaries.

4.5 Experiment Two: Occupation-specific incentives

While the first experiment focused on firms' decisions to use contractors in the absence of compensation incentives, the second experiment looks at how incentives could vary across occupations. This experiment leaves the contractors' fees constant, and instead allows salaries through contractors to be higher or lower. This form allows the firm to save money through intermediaries.

This model was run with seventy-two permutations of parameter settings, with each setting run 20 times each, for a total of 1,440 simulations. The other parameters such as the grid size and the number of agents are the same as in experiment one. Four cases representing four of the 72 experiments are highlighted in table 4.1. These four are highlighted, as they are hypothesized to approximate the empirical situation in four occupational labor markets.

The first parameter that was varied is the skill distribution of the workers and the jobs in their labor market. This had four settings, all taken from the Bureau of Labor Statistics and CPS data for programmers, accountants, minimum wage workers, and the overall labor market. The second parameter indicates the wage premium (or penalty) for an indirect hire and is based on the empirical wage gap between indirectly and directly hired workers. It has three settings: higher, lower, or the same wages. The third parameter that is varied is the variance of the wage premium which also has three settings. Empirically, the standard deviation of wages is highest for high-skill occupations and lowest for low-skill. This is because the labor market is far-right skewed. College graduates can earn average or *extremely* high wages, while high school graduates are concentrated on the narrower lower portion of the wage spectrum. Because of the skewed shape of the wage distribution, we would expect the standard deviation of log wages to be about the same for the two groups, though in CPS data the low skill workers had a slightly higher standard deviation, possibly due to top-coding the highest wages. The last parameter, work variability, was tested at just two levels: high and low. Less skilled occupations have more variable work hours on an individual level (i.e. low skill workers can work 20 or 50 hours a week), while high-skill jobs have consistent individual hours (a little over 40 hours per week) (BLS, 1988-2004).

parameters				*outcome*	
skill distribution	intermediary premium (3)	variance of premium (3)	firm work shocks (2)	*hypothesized* level of indirect hires	*experimental* percent indirect
minimum wage	negative	low	normal	++++	39.6%
all labor	none	high	normal	+++	41.5%
programmers	positive	medium	normal	++	32.7%
accountants	none	high	low	+	28.0%

Table 4.1: Model predictions and outcomes

Hypotheses and experimental results for 4 of the 72 simulations are highlighted in table 4.1. I hypothesize that workers in the "minimum wage" scenario (with the minimum wage educational distribution, lower indirect compensation, and more firm shocks) are the most likely to be hired through intermediaries because first, the model is set such that firms

save more on the compensation of indirect hires; second, a wide skill distribution among these workers means that there are more opportunities for intermediaries to improve match quality; and third, there might be a high variability in firms' demands for these workers (high firm work shocks). In contrast, firms hiring accountants might use intermediaries less since there is less of a difference between indirect and direct hires' wages; there is a narrower skills-distribution, so firms are capable of finding a good match without an intermediary; and there might be a relatively constant demand for accountants. Generally, firms should realize greater utility gains from intermediaries in professions with wider skill distributions, larger wage gaps, and more firm work shocks. The last column of the table shows the mean proportion of the labor force that was employed through an intermediary at the end of the 20 runs for each of the four combinations of parameter settings. While the reader might disagree with the hypothetical scenarios displayed in table 4.1, *every possible scenario* was run 20 times, and the relationships between variables are assessed for *all* runs using multivariate methods, presented below.

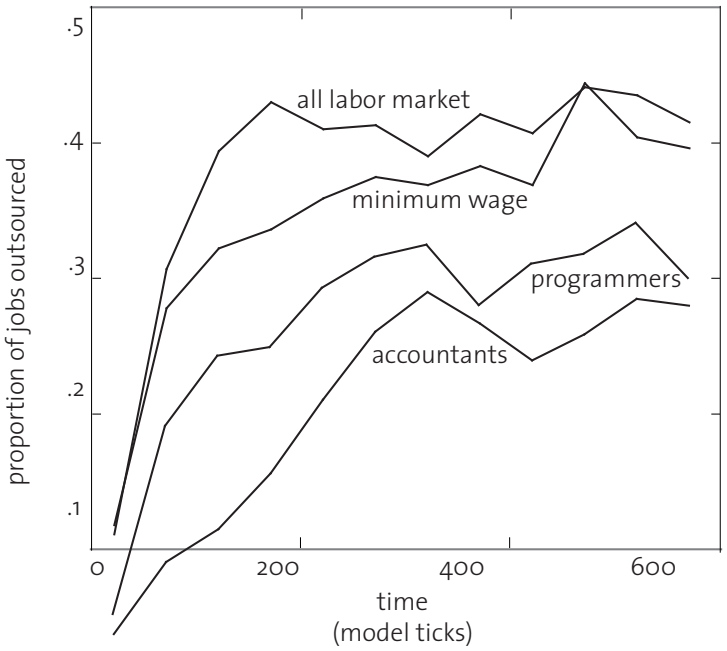

Figure 4.6: The experimental spread of indirect hiring under four scenarios

The simulations' predicted levels of indirect hires for these four scenarios, depicted in figure 4.6, bear out the theoretical expectations. The model finds that firms transition to using intermediaries more quickly for low-skilled occupations. While standard deviations are not depicted in figure 4.6, there is not a significant difference between the overall labor market scenario and the minimum wage scenario except from approximately tick 100 to 200. There is a significant difference between the 2 scenarios using more intermediaries (all labor market and minimum wage) and the 2 least scenarios using fewer intermediaries (programmers and accountants) throughout the entire run. Programmers and accountants have almost significantly different rates of intermediary use throughout the model (significant at .10) with programmers being hired through intermediaries more, probably because accountants are set with a narrower skills distribution and less workload variability. In addition, it is more expensive to use intermediaries for accountants because they have slightly higher skill-levels so intermediary fees are higher. As in the first experiment, firms rapidly adopt intermediaries and then hover around a final stable level of indirect hires, past the 600 time ticks shown here. Across all scenarios, the proportion of workers hired through intermediaries ranges from 20% to 52%.

parameter	β	standardized β	min	max
intercept	-.027	-	1	1
relative comp	.1701***	.1239	.9	1.1
var of above	.1420**	.0517	.05	.15
work shocks	.925***	.2063	.05	.1
ticks	.0004***	.6789	25	575
skill distribution				
min wage	-.0298***	-.1153	0	1
programmer	-.0916***	-.3541	0	1
accountant	-.1147***	-.4432	0	1
all labor	baseline			

OLS regression, observations are model runs
864 observations (72 settings, 12 intervals)
R-square .69

Table 4.2: How do ABM parameters influence indirect hiring?

A simple multivariate analysis predicting the proportion of jobs filled through intermediaries across all runs of the simulation shows that all the parameters except the compensa-

tion variability are significant at the .001 level, while compensation variability is significant at the .01 level. Excluding the predictive power of "time" (ticks) in the model, these parameters of interest explain only 22% of the experimental variance; with "time" they explain 69%. Figure 4.2 shows the OLS regular and standardized coefficients for a multivariate regression predicting the proportion of indirectly employed workers based on the experiment's parameter settings. Of course model time has the most impact; and is followed by the skill distributions, then workload fluctuations, then cost, and finally cost variance. Most of the results make intuitive sense. The more heterogeneous skill distributions promote intermediary use while more narrow distributions like accountant limit it. When workloads fluctuate there is more turnover both giving firms more opportunities to hire through intermediaries, and preventing indirect hires from moving into full-time positions. The only unexpected result is that higher wages for indirect workers ("relative comp" *increase* the proportion of workers hired indirectly. While this seems odd from the perspective of the firm's utility equation, it makes sense when we consider the role of organizational ecology and the fact that intermediaries have to make a profit to offer their services.

Organizational ecology matters because when workers hired through intermediaries earn more money, so do the intermediaries. At lower costs firms might want to use intermediaries, but cannot find them. The intermediaries' profits are driven by an interaction between the worker's skill and the compensation differential, with them earning the most from a high-skill worker with a contract wage premium and the least from a low-skill worker with a contract wage penalty. If we recall, intermediaries are forced out of business if their revenues per assigned job are less than 10% of the average skill level in the model. In a run of minimum wage workers, where many workers have a high school education, the average contractor who matches all of his clients under a wage penalty scenario, can still expect to earn about 17% of the average skill level in that occupation, well above the minimum requirement. But if the intermediary sets a slightly lower fee rate (remember fee rates are set by a random distribution around an average level), or their workers draw slightly

worse penalties, or if they fail to match some jobs, they go out of business. When they go out of business, the firms working with them have to look for another local contractor and if none is available, they hire workers directly. Thus, the firms don't use intermediaries simply because they are cheaper, but also when they are expensive enough to sustain a healthy organizational ecology.

As in the first experiment, the model also produces other experimental labor market statistics including: unemployment rates, vacancy rates, job duration, and vacancy duration. These measures are used to verify that the model is a reasonable approximation of the labor market. The model has unemployment and vacancies fluctuating around 5% regardless of parameter settings or the time in the model run. Since most jobs are at a few firms, this means that the overall unemployment and vacancy rates are autocorrelated, and generally resemble a real-world labor market having business cycles of high unemployment and low unemployment rather than random noise in the unemployment rate.

In terms of skill level, the experiments find that unemployed workers are consistently the least educated, then indirect hires, and then direct hires are the best educated. In the model, one might expect the *mean* skill level of indirect hires would not differ from that of direct hires since firms should use intermediaries at both extremes of the skill distribution since the least-skilled workers should receive fewer job offers and the most educated should be the least likely to receive an offer filling their minimum requirements. Thus, both the most and least educated should be the hardest to match. On average, these characteristics should balance out leaving no difference between direct hires and indirect hires. While the differences in figure 4.7 look small; they are statistically significant, with indirect hires having significantly less skills, and direct hires having more. This is the second unexpected and interesting finding: intermediaries encourage firms to sort workers, keeping the best inside the firm.

A multivariate regression comparing the different parameters' effects on the skills gap between indirect and direct hires suggests that when indirect employees are paid more

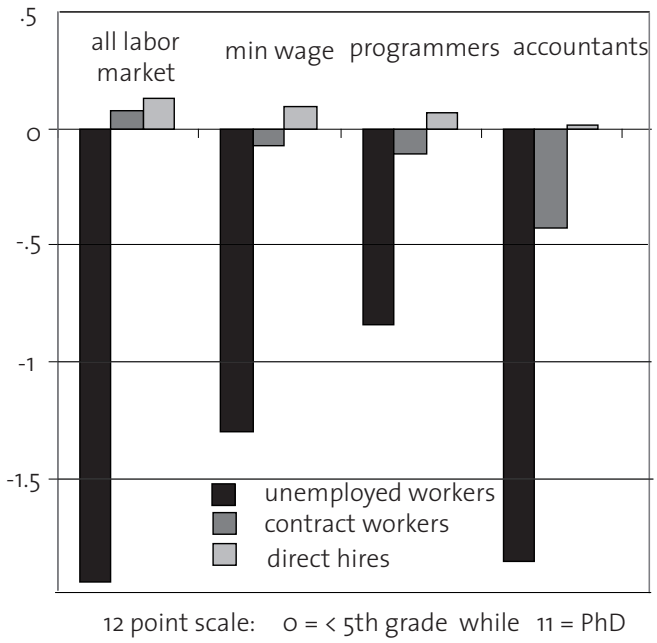

Figure 4.7: Deviations from the average worker's skill

relative to direct hires, the skill gap increases. Second, the regression finds that when firms have more workload shocks there is a lower skill gap (a low rate of shocks increases the skills gap by about 1 degree level or the difference between an MA and a BA). This occurs because the low-skill workers who would stay on and transition into direct hires (thus diminishing the skill difference) are the first to be fired in a volatile market. Third, using the all labor market distribution results in the smallest skill gap, while accountants have the highest skill gap. This is unexpected because, of course, accountants have the narrowest distribution of skills among workers.

Firm utility is one of the most important measures, as it motivates all the model's dynamics. Utility ranges from 0 to 1, with the experiments including contractors increasing utility on average .05 points compared to a baseline model with no contractors. A multivariate regression predicting utility levels based on parameter settings suggests that high contract premiums and more workload shocks increase firms' utilities as does using the programmer or accountant skill distributions (because they are narrower). The premium's

role is attributable to the aforementioned organizational ecology effect. Workload shocks increase utility because they increase turnover, which in turn increases the chance the firm makes a better match and better matches are more likely to survive since the worker is less likely to quit. Thus, turnover is good for a firm in the simulation since the model ignores the importance of on the job training and firm-specific skills. It is surprising that the narrower skill distribution increases utility since utility should improve through intermediaries more in the broader skill distributions. Other model output includes the unemployment rate, vacancy rate, average vacancy duration, and average unemployment duration. All roughly matched the labor market, with unemployment hovering around 5% and most unemployment spells being short, but with a few chronically unemployed.

The model's primary limitation is that as a model, it excludes many incentives and makes many assumptions. This model focuses on the trade-off between the cost of using intermediaries and their better matching ability, omitting dynamics like firm-specific skills and worker substitutability. The model showed that there are strong incentives for firms to use intermediaries even in the absence of wage premiums; that wage premiums and fees can have unexpected effects because of organizational ecology; that firms are more likely to use intermediaries to fill their least-skilled jobs (stratifying workers by contact type); and that even when, in expectation, intermediaries are not advantageous, some firms will persist in using them, misinterpreting natural variability as a systematic advantage.

4.6 Empirical trends and model verification

A significant amount of research has measured the number of outsourced workers (presumably indirect-hires), the types of firms most likely to outsource, and the number and size of firms offering job-matching services. Generally, studies using employer data estimate higher growth rates for atypical employment than those using employee data. I examine three measurements of the trend of using indirect hires in the US economy, and use one to

verify the model.

The Economic Census is a survey of businesses conducted by the US Census Bureau every five years. It collects information on firms' industries and employment. Figure 4.8 shows that between 1997 and 2002, employment at companies providing contract services grew more rapidly than the rest of the economy. (This measure includes direct hires at the service companies, but assuming administrative costs are a constant proportion of staff, this does not effect the data.) It is somewhat ambiguous whether the workers at these firms are actually indirect hires. At a janitorial services firm it is very likely that the workers perform their duties at clients' sites while in accounting firms it is less likely. This data also does not provide information about the total number of workers in an occupation and only has information for two periods. Thus, we can only estimate rate of employment growth for the workers at the service firms, not the growth rate for the proportion of an occupation that is hired through intermediaries. As such, the data is relevant to the model, but not directly comparable.

There are two measurements approaching the question from employee-side data that could be used for verification. The first technique uses the March Current Population Survey (CPS) to construct a time series of the proportion of an occupation that is indirect hires. Indirect workers are identified by matching occupational and industry codes, positively identifying any worker in an occupation working for a firm specializing in providing that same occupational service. Figure 4.8 shows the proportion of workers in a given occupation who are indirect employees using this method.[11] In contrast to the employer side data, this data suggests a much slower growth pattern, and even a slight decline in the clerical sector in recent years. This method is superior because it incorporates the general growth rate of an occupation and can estimate the proportion of the occupation hired indirectly. The method is limited in that it can only identify indirect workers when there are matching codes for occupations and industries (i.e. clerical workers and clerical services)

[11] CPS occupational and industrial codes changed in 1992 and 2002, leaving a slight discontinuity.

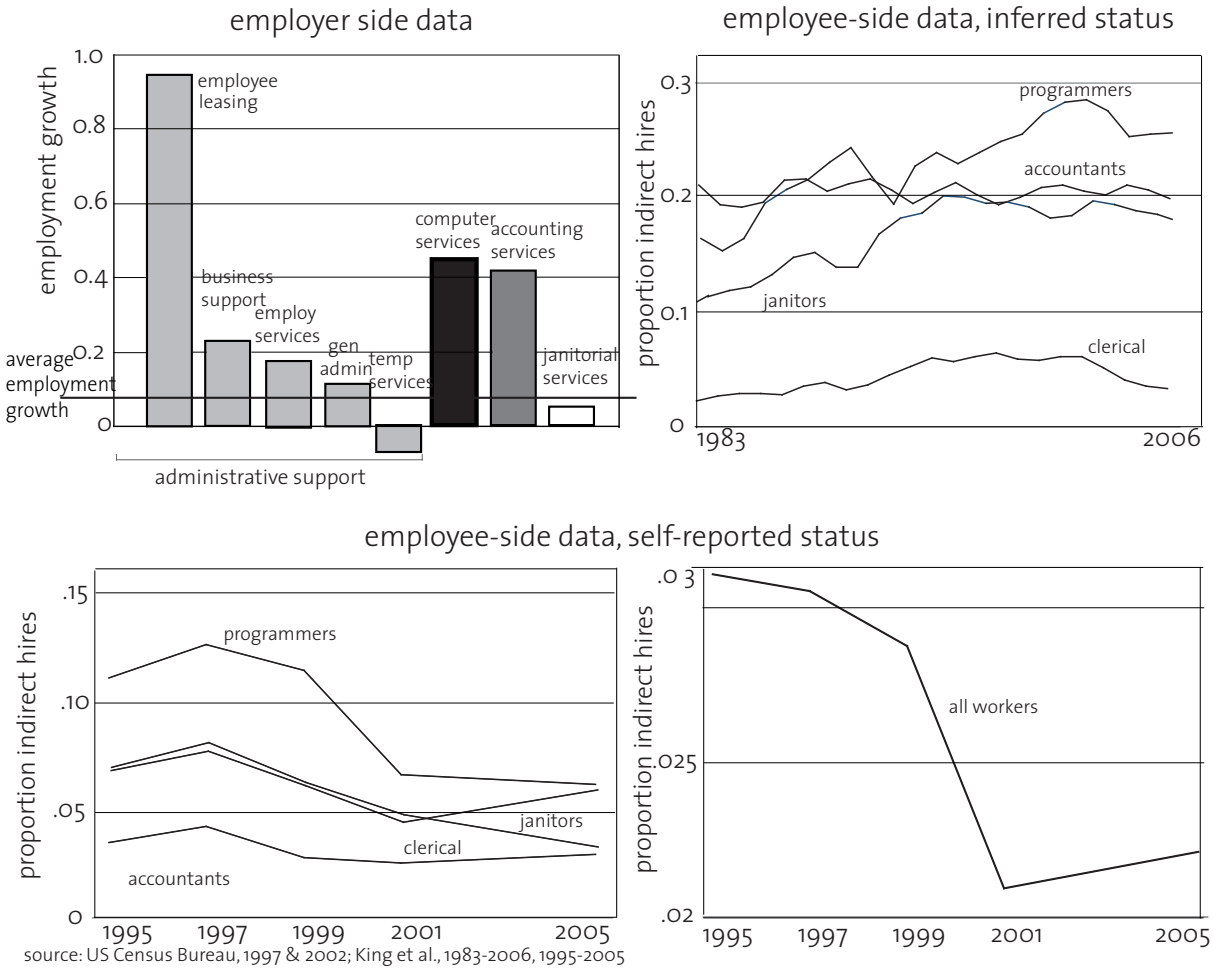

Figure 4.8: Empirical measures of indirect hiring.

and that it misidentifies direct-hires working at an intermediary in the same occupation that they rent out labor in (i.e. an accountant working in an accounting firm). As with the employer-side data, the method is likely to misidentify high-skill workers who actually work at the contractor's site.

The third technique uses the CPS Contingent Worker Supplement (February 1995, 97, 99, 2001, and 2005) which directly asks workers about their employment status. This method counts temporary workers, on-call, casual laborers, day laborers, or any worker reporting that their employer leases out their services. Figure 4.8 shows that for programmers, accountants, janitors, and clericals, this method suggests the opposite trend as the

two prior techniques, suggesting employment through intermediaries has actually declined since 1997.

Why do the estimates vary? Theoretically, the method using the Contingent Work Supplement should be the most accurate since there is no proxy measurement; the survey directly asks the workers about their employment status rather than inferring it. However, individuals often misreport the firm they commute to as their employer rather than the intermediary, who is actually their legal employer (Bjelland et al., 2006). This bias is confirmed by examining a single question from the same survey. Early in the survey, the worker is asked to report his or her employer. Later, the respondent is asked whether they were paid by their employer or a temporary help agency. If they were paid by a temporary help agency, the interviewer then asks them whether their reported employer was the agency or the agency's client. Surprisingly, the majority of respondents report the client as their employer. Figure 4.9 shows that this bias has gotten worse over time. This distorts measurements using both the inferred and direct CPS estimates since the indirect method relies on workers accurately reporting their employer's industry.

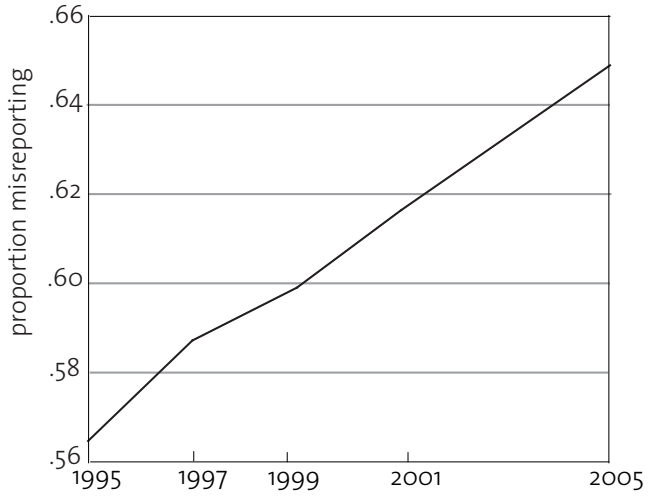

Figure 4.9: Proportion of indirect workers misreporting indirect employers as primary employers (King et al., 1995-2005).

The empirical evidence on intermediary use is of mixed quality (figure 4.8). Neverthe-

less, the data from the CPS was compared to the experimental data. The first 200 ticks (or time periods in the model) were discarded as "burn in."[12] Across all the models the average level of workers hired through intermediaries, as of the end of the model, was 36%. This far exceeds the level found in the empirical data, indicating that incentives are set too high, or important disincentives are omitted. The difference in occupations found in the model is different than the empirical data. In the model the low skilled workers in wider skill distributions seems to be more likely to work in indirect employment while in the empirical data programmers and accountants seem to. I would speculate that this mismatch is largely driven by miscalculation in the empirical data. The empirical data calculation includes, for high skill jobs, a large portion of workers who have regular jobs at service firms (i.e. accountants at accounting firms). In sum, the overestimation in the model is probably due to the omission of more indirect hiring disincentives than incentives while the mis-ordering of occupations is likely due to the empirical measurement problems.

While the predicted levels of indirect hiring do not exactly predict reality, it is first, unlikely to do so in a simple model and second, could easily be remedied by adjusting the magnitude of the effects of tested incentives and disincentives on firms' utilities. It is perhaps more important that the model generally functions like a real labor market. One way to test that is using the unemployment and vacancy rates in the model. In the model, across simulations the mean unemployment rate is 5.3% while vacancies are 5.2%. These are relatively realistic values; unemployment in the US was 5.5% as of May, 2008.

Another way to compare this simulated market to the real market is using the relationship between unemployment and vacancies (Fagiolo et al., 2004). Unemployment rates and vacancy rates are hypothesized to follow the "Beveridge Curve," named after the English

[12]Originally, the diffusion of intermediary use in the model from tick 200 on was fit to the time series from the CPS data from 1983 to 2005, with the assumption that hiring through intermediaries really began in the 1980's. The fits are not presented here primarily because the fitting of the time axis (linking tick 200 to 1983 and the last tick to 2005) was arbitrary. The experimental and empirical data matched for some experiments such as the prediction of programmers' indirect hire rates using those experiments with the programmers' skill distribution, no indirect hire wage premium, a low variance of the wage premium, and few firm shocks. Nevertheless, the same experiment fits the growth in indirect employment for clerical workers and janitors well too.

Economist William Beveridge. The Beveridge curve is a curve relating unemployment rates (x-axis) and vacancy rates (y axis) that has a negative first derivative and a positive second, so that it is concave, with vacancies approaching zero as unemployment increases and unemployment approaching zero as vacancies increase. Empirically we never know the exact shape of the curve, as it slowly migrates in towards the origin and away from the origin as worker-job matching becomes more and less efficient, respectively. Nevertheless, the curve is shown to exist as employment and vacancy rates move in a counterclockwise fashion following the theorized concave curve. During the 1990s and 2000's the Beveridge curve wandered inward towards the origin, in the general range of unemployment around 4 to 8 % and vacancies around 2 to 4% (Valletta, 2005).

Unlike using real data, the ABM can generate full Beveridge curves because multiple experiments are run under each regime. Using data from all experiments, at each combination of parameter settings, at multiple time points (ticks), for those observations beyond the 200th tick, we can generate Beveridge curves from the ABM. In figure 4.10 we see in the lower left hand corner, the plot of the natural log of unemployment versus vacancy from the experiments, suggesting that the natural log of unemployment might be a good fit to predict vacancies, a functional form that naturally fits the Beveridge curve. Next, a model of $v = c + \alpha ln(u) + \beta X$ (where u is unemployment, v is the vacancy rate, X is the vector of experimental conditions, and c is a constant) was fit, finding the significant coefficient of -.0359 for ln(unemployment) with an R-square of .82. (The R-square just using ln(unemployment) is .20) Among the experimental conditions, the skill distribution and the firm shocks also change the prediction. The upper panel of figure 4.10 shows the ABM's combinations of vacancy and unemployment rates and the predicted values. Even though ID's were omitted from this diagram for clarity, it is clear that the experimental and predicted values mirror each other. The bottom right hand plot shows this by plotting residual against the predicted vacancy rates- showing a good fit. The most efficient markets (with a Beveridge curve closest to the origin) in the simulation are those simulations

with the accountants' skill distribution and more firm shocks. This makes sense because it is easier to match workers and jobs in a market. It also suggests that the market is more efficient in a more fluid labor market where firms are often hiring and firing people. The least efficient simulations are those using the all labor market distribution and fewer firm shocks- the polar opposite scenario.

What this exercise shows, is that the simulated labor market is more or less functioning as a normal labor market does. Unemployment and vacancies are not related linearly, but in a concave curve in the shape of the Beveridge curve. In sum, both the absolute levels of unemployment and vacancies, and their relationship, in the simulation, mirror that which we see in the real world, suggesting that the model does a reasonable job of copying the real labor market.

While the estimates of intermediary use in the model do not match the empirical data, more importantly, its other characteristics such as employment and vacancies resemble a real labor market.

4.7 Conclusion

The agent based model had several important findings. The first finding is that independent of compensation differentials, intermediaries' better ability to match workers and jobs is a sufficient incentive for firms to use intermediaries. The second finding is that organizational ecology matters, and consequently higher intermediary fees can increase firms'propensity to use intermediaries by sustaining an organizational ecology of contractors. Another finding is that a percentage fee structure turns indirect employment into a sorting mechanism, where firms hire their less skilled workers through intermediaries. Finally, the model found that in occupations with more heterogeneity among workers, firms are more likely to use intermediaries.

The empirical data suffers many flaws, but did suggest some conclusions and directions

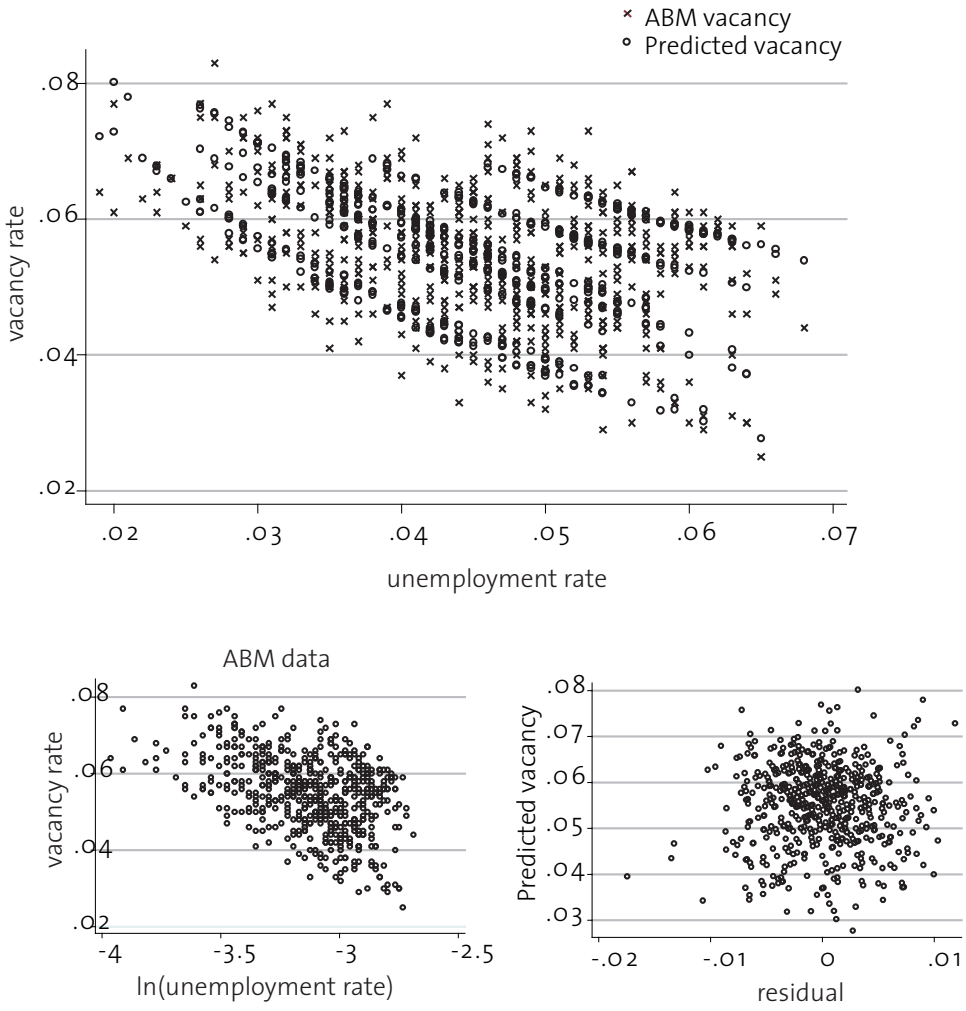

Figure 4.10: ABM Beveridge curves

for further research. First, using intermediaries is not necessarily an exploding trend; this could be a misperception based on using employer-side data. Second, the data suggests that the major transition has most likely already happened and that intermediary use seems to have stabilized in the 1990s. Finally, it is difficult to make empirical conclusions because the definition of an indirect hire is unclear (particularly for high-skilled jobs) and because workers in these arrangements misreport their employers.

This paper was limited in that it did not explore the role of other incentives to hire through intermediaries, like firm-specific skills and tested a context with legal limitations

on indirect employment duration. Finally, the model used compensation gaps calculated from a simple method of taking the difference between indirect workers' wages and direct workers wages rather than controlling for selection on basic worker characteristics like age, race, education, and location, as in the second paper of this dissertation on fixed term wage gaps. As such, the simulation may benefit from a better measure of the compensation gap.

Atypical employment is an important area of research. If there are trends towards using intermediaries with strong repercussions for compensation gaps, and these trends sort workers by ability, we are moving towards a two-tier system of employment with one group of workers enjoying the benefits of direct employment and another group suffering the penalties of indirect employment. Many European countries have moved towards guaranteeing these types of workers equal compensation and union negotiating power in an attempt to combat this labor market segmentation. This same question is equally if not more important in the United States, where employer-provided health benefits are at stake.

Chapter 5

Academic Employment Networks & Departmental Prestige

5.1 Introduction

Sociology departments'academic rankings incorporate both "'objective" measures of department quality (such as citation rates and funding patterns) and "subjective" measures (such as faculty perceptions of graduate school quality). While we might expect that departments could improve their rankings when objective measures are used while they might be unable to improve their positions when reputational measures are used. Yet, regardless of the measure used, academic rankings are relatively constant over time, with the top schools swapping the top positions (Graham and Diamond, 1997). Several rankings for sociology graduate programs from 1925 to 2005 are illustrated in table 5.1. Four institutions have been in the top 10 since 1925 while 8 others have since 1982. These rankings find remarkably consistent results despite their significantly different methodologies. The oldest type of formula is a reputational rank, which was pioneered by Raymond M Hughes. In his 1925 report, Hughes surveyed 20 to 60 faculty members in each field, asking them to rank institutions based on "esteem at present time for graduate work in your subject." The much critiqued US News and World Report rankings build on this formula, basing ranks on a peer assessment surveys (50% response rate) sent to academic department heads and directors

of graduate study in sociology.[1] The National Research Council's (NRC) 1995 rankings are more complicated; they also use reputational measures (also with about a 50% response rate) but augment it with data for about 17 program characteristics such as: size, private vs public university, total research and development (R&D) expenditures , federal R&D, library expenditures, enrollment, total faculty, percent full time faculty, percent faculty with research support, percent full professors, faculty awards, citations per faculty, faculty characteristics, and student characteristics. The NRC found that the reputational measures are consistent with the objective measures. Critics of the NRC rankings argued there was too much emphasis on research-related variables and too little on doctoral training, though the next version of the ratings will incorporate more training-related variables.

The analysis here is done twice, once using the NRC's sociology rankings. Unfortunately this rank does not include foreign institutions. Thus the analysis was done again using the US News and World Report international rankings, which are not specific to sociology.[2] The Newsweek score includes measures of citations, publications, international faculty, international students, faculty:student ratios, and library holdings. While the two rankings were developed using different metrics and only one focused on sociology, the rankings correlate at .625 for those US schools where both ranks were available. The primary difference between the ranks is that the NRC sociology rankings exclude technical/science schools like MIT and Caltech, while these schools are near the top of the general international ranking.

Some researchers suggest the stagnant rankings indicate a closed system where departments find it difficult to move up the rankings and where well-established programs can reinforce their dominance. This organizational situation could be considered analogous to individual-level stratification in a "closed system" where intergenerational transmission of advantage trumps equal opportunity (Lipset et al., 1955). Ideally, stratification should func-

[1] For an excellent critique of the US News rankings see Ehrenberg 2002.

[2] I tested the Shanghai rankings as well, but because there was little difference those results are not presented here.

1925*	1982+	1995**	1995+	2005**
Chicago	Chicago	Chicago	Chicago	Wisconsin
Columbia	Wisconsin	Wisconsin(2/3)	Wisconsin	Berkeley
Wisconsin	Berkeley	Berkeley(2/3)	Berkeley	Michigan (3/4)
Minnesota	Michigan	Michigan(4/5)	Michigan	Chicago (3/4)
Michigan	Harvard	Chapel Hill(4/5)	UCLA	Chapel Hill
Harvard	Chapel Hill	Harvard(6/7)	Chapel Hill	Princeton (6/7)
Missouri	Stanford	UCLA(6/7)	Harvard	Stanford (6/7)
	Columbia	Stanford	Stanford	Harvard (8/9)
	UCLA	Northwestern(9/10)	Northwestern	UCLA (8/9)
	Arizona	Princeton(9/10)	Washington	UPenn

∗ Hughes (1925)
+ National Research Council (1982, 1995)
∗∗ US News and World Report (1995,2005)

Table 5.1: Sociology department ranks

tion as an incentive for individuals to work harder or to acquire more human capital (Davis and Moore, 1945), or for organizations to innovate and improve their product. However, too much stratification might indicate that either individuals can propagate their advantage through their current assets or analogously, an organization can sell more of their product not based on their current effort, but on their brand name or reputational inheritance.

There are two ways sociology departments may maintain their advantages in the rankings. First, it might be that respondents to the reputational survey are rather ill informed, basing their evaluation of doctoral programs not on the programs' actual merit but on what respondents have heard about departments (although the correlation between NRC's objective and subjective measures speaks against this). If this is the case, once a program is highly ranked, it will remain there, as professors perpetuate the reputation without objectively examining it. More likely, once a program is highly ranked, it has the ability to perpetuate its rank by attracting faculty and resources. The simple preference for faculty to move to or between higher ranked schools can cement departments' rank. Departments' consequent central positions in the academic hiring network can further enhance departmental prestige through many mechanisms such as research collaborations or knowing about upcoming trends in the field. While this paper does not explore the specific

mechanisms linking hiring network centrality and prestige, it does confirm the existence of the correlation between centrality and prestige, independent of training and department size–possible spurious causes not previously considered.

There is significant non-network research testing how the institutional prestige of PhD granting institutions influences first job placement. This literature finds that the most prestigious universities hire each other's graduates, over-valuing the institutional prestige of applicants' training institution over other characteristics that might be more predictive of long-term success, such as the time it took to complete the PhD (Bair, 2003; Baldi, 1995; Burris, 2004; Burke, 1988; Hargens and Hagstrom, 1966; McGinnis and Long, 1988; Reskin, 1979; Smelser and Content, 1980).

In contrast, there are only four papers testing whether academic departments' positions in academic hiring networks are linked to academic rank. Burris (2004); Wiggins et al. (2006) and Fowler et al. (2007) create networks linking professors to their current employers and their PhD granting institutions, generating a network of institutions with weighted, directed edges indicating the number of PhDs trained at one department and currently employed in another. These studies analyze computer science, information schools (formerly called schools of library science), sociology, and political science departments and find a significant relationship between network centrality and rank. Their choice of centrality measures vary, though they all use recursive network measures (based on the adjacency matrix's dominant eigenvector) that measure a node's prestige based on the prestige of those nodes it is connected to. Centrality measures used include: eigenvector centrality (Bonacich, 1972), PageRank (Page et al., 1999), and hub and authority centrality scores (Kleinberg, 1998), used by Burris (2004), Wiggins et al. (2006), and Fowler et al. (2007) respectively. Fowler et al. (2007) uses hubs and authorities, making a distinction between prestige from placing students in prestigious departments and hiring professors from prestigious departments. All three studies ignore the link between the department where an academic got their PhD and the department of their first job (the traditional question in

non-network studies) and all ignore placements taking place between the professor's current job and his or her training. The Grannis (2005) approach is slightly different, looking at UCLA's ego network of its faculty trades with other departments. These articles then use centrality scores as a predictor of departmental prestige (Burris, 2004; Fowler et al., 2007) and interpret the relationship as confirming institutional stratification (Burris, 2004) or in the hubs and authorities case showing that placing students in prestigious schools is more relevant to prestige than hiring professors from prestigious schools (Fowler et al., 2007).[3] Ultimately, it is difficult to parse out the relationship since there is a circle of causality—productive researchers increase a school's prestige, but prestigious schools also attract productive researchers.

This paper expands on the current body of research in two ways. First, it considers the impact of prestigious schools training many more PhDs than there are openings in the entire field (henceforth referred to as "overtraining"), and second, it considers that the relationship between between hiring network centrality and academic rank might be spurious, driven by department size which is a reliable predictor of both. In addition, this paper uses a more robust methodological procedure considering the effect of sample bias, bipartite graph reduction, and the choice of network centrality measures.

Currently, most of the literature ignores that centrality and prestige are both strongly influenced by department size (National Research Council, 1982, 1995). Department size *indirectly* influences the rankings insofar as there are more former employees and students from the largest schools and insofar as those individuals rank their previous affiliations higher than those departments they were never affiliated with. Department size also increases centrality *directly* because bigger departments have more edges. Consequently, centrality and prestige should be correlated by virtue of department size even if location in the network is unrelated to prestige. This is well illustrated in one of the four existing

[3]Using the natural log of eigenvector centrality as a predictor of academic rank in regressions,Burris finds coefficients around 1.3 in sociology, history, and political science. Using PageRank, Wiggins finds a correlation between centrality and rank of .81. while Fowler et. al. find correlations as high as .82 between network centrality and prestige rank.

studies. Fowler et al. (2007) shows that ranks can change when we control for department size, particularly for boutique programs with focused research areas. It should be noted that, theoretically, department size might play a valid role in determining prestige since bigger departments have more depth and thus more opportunities for graduate students and researchers to expand their skills. The size distribution of sociology departments included in this study are shown in figure 5.1. The bars indicate actual department size while the line is just a smoothed estimate. The distribution is more concentrated around faculties of about 20, rapidly petering out. Wisconsin has an extreme number of faculty, presumably because the NRC numbers include cross listed faculty.

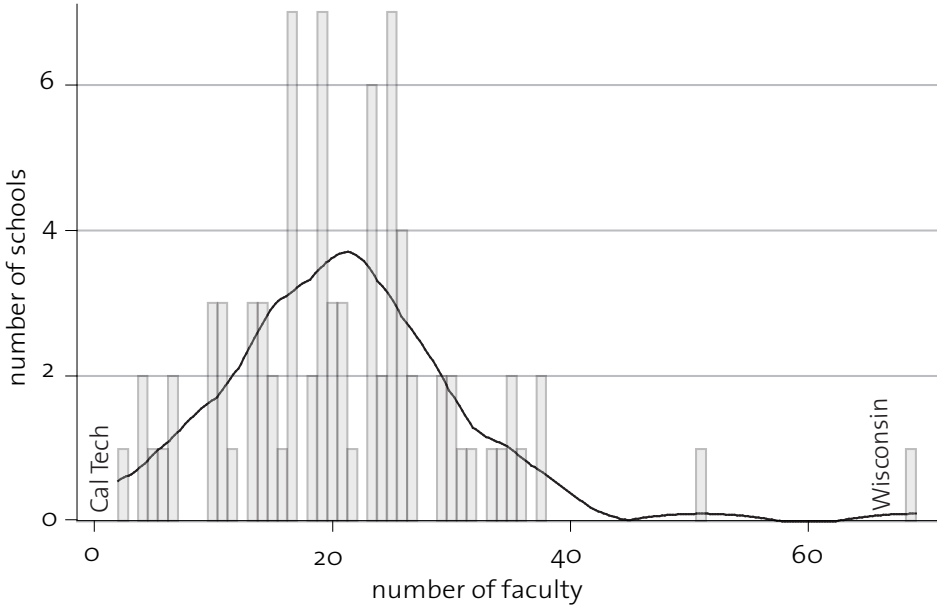

Figure 5.1: Department size

Overtraining can also account for part of the relationship between centrality scores and academic rankings. The current research creates edges between professors and their training departments and their current employers. If the most prestigious and largest departments train a much larger percentage of the job market than they hire, and train more than the less prestigious schools, they will be more central. Figure 5.2 shows two lines illustrating first where professors at the top ten schools were trained and second where pro-

fessors at the average school were trained. The dark grey section shows the proportion of professors currently employed at the top ten schools who were also trained at the top ten schools slowly incrementing from those trained at the top school to those trained at the 10th school. We see at the origin, that over 10 percent of the faculty at top ten schools were trained by the University of Chicago and about 20% were trained at Wisconsin and Chicago combined. Bumps in the graph show that University of Michigan and particularly Harvard trained more of the faculty at top schools. By the end of the chart, we see that over 70% of the professors at top ten schools were also trained in the top 10 schools. The lower line and light grey section of the graph shows where the professors at the average school were trained (including professors from every fifth school in the rankings from 5 to 95). The training school of all faculty at every fifth school was collected as a sample indicative of the training of the average school. The percentage measures the percent of total faculty, not the average percent at each school. This is an important difference because it weights the bigger (and usually better) schools more and is ultimately representative of the mean in the labor market, not the mean school. This line shows the same pattern as that for the top ten schools, with close to half of *all* professors being trained at the top ten schools. The American Sociological Association (ASA) reports there are 598 new PhDs every year and a stock of only 4,227 tenure and tenure track positions in the US. This means that enough students graduate to replace the entire profession every 7 years. At this level of production, all universities can hire from the top schools, while the graduates from the other schools must simply leave the market. This places highly ranked schools at the center of the hiring network. This analysis tests whether the association between hiring network centrality and rank holds independent of this over-training.

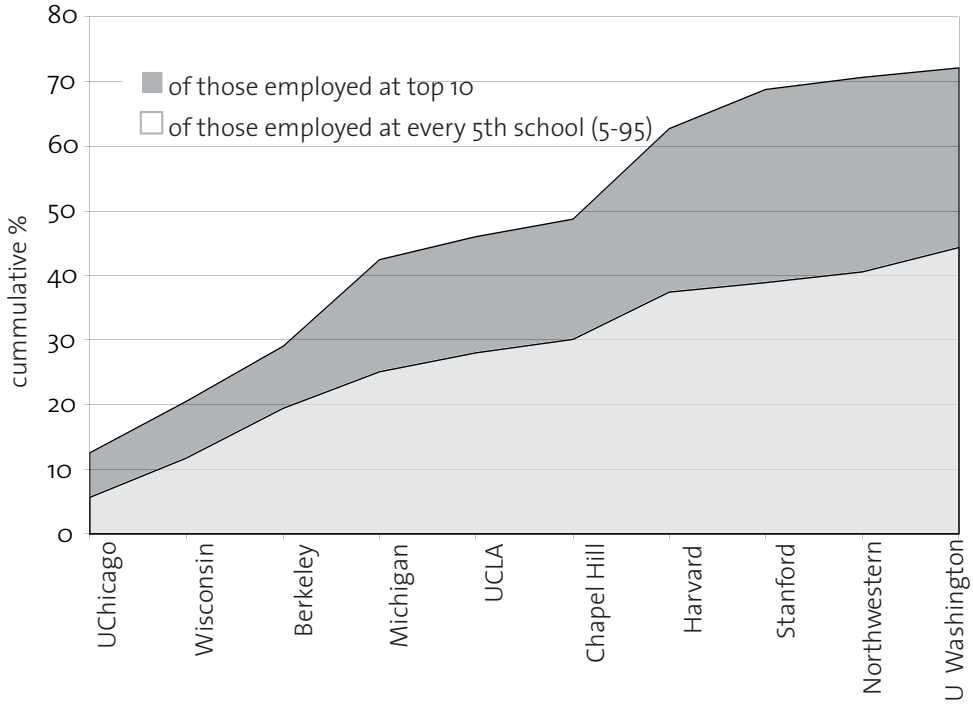

Figure 5.2: Where professors were trained

5.2 Data and methods

Two separate data sets were collected by choosing sociology departments and using current permanent faculty's CV's to code edges between faculty and the departments and organizations they had been affiliated with. The first data set collected faculty from prestigious (according to the NRC rankings) departments (Wisconsin, University of Michigan, Harvard, Berkeley, UCLA, University of Chicago, Brown, Stanford, and University of Arizona). The second sample was collected with the intention to test the effect of having sampled the most prestigious institutions in the first data set. This second group includes Yale, University of Pennsylvania, Northwestern, Princeton, Johns Hopkins, and NYU. The second group still represents exceptional schools; the comparison between these two networks allows us to test whether sampled schools automatically become the most central schools. Edges between individuals and institutions were coded as "PhD training", "tenure-track,"

and "non-tenure track." Non-tenure track jobs include lectureships, post-doctoral positions, non-academic, and visiting appointments. Approximately 7% of the sample did not have their CV's posted on-line. For these cases, edges were coded to the faculty's current institution and their PhD granting institution (which were normally listed). Including these CV's will place the departments that train more students at the center of the network. The edges will drop out for those graphs were training edges were excluded. Thus, including this 7 % should sharpen the difference between the findings using graphs with and without training edges. The two samples included 193 and 241 institutions, 99 and 89 institutions that were ranked by the NRC, and a total of 886 and 882 links for samples one and two, respectively. All network measures used in this analysis were generated using the full graphs including non-academic institutions although the secondary regression analyses use the sub-sample of academic departments with prestige rankings.

Twelve different graphs (differing by sample choice (2), edges included (3), and graph reduction (2)) were used to test whether the relationship between centrality and prestige is robust to graph specification. The graphs either included all three types of edges, excluded non-tenure track edges, or excluded training edges. The first sample was reduced to 99 institutions when non-tenure track edges were excluded and to 178 institutions when student edges were excluded, while the second sample was reduced to 89 and 237 institutions. Each of the 6 graphs was first analyzed as a full bipartite graph with both individuals and institutions and then then analyzed as a reduced graph including only institutions, weighting the edges between institutions by the number of faculty they had in common.

There are 4 main methodological challenges using this data. First, any sampling method biases the graph, enhancing the sampled institutions' centrality. One solution to this problem is to start with seed institutions, and then to sample from the other institutions that enter the analysis, ultimately excluding the original seed institutions from the network analysis. Instead, I include these biased observations, but use two different seeds, concluding that if the results are similar using the two seeds, the conclusions are robust to sample bias.

Second, the data includes both end-of-career and beginning of career professors. This biases the data insofar as older professors with a longer history of institutional connections are more likely to be at more prestigious universities. Other studies have similar problems, for example, coding the edges between a department that trained a professor and their first job the same as their emeritus job (Burris, 2004; Wiggins et al., 2006; Fowler et al., 2007). Third, academia is not an isolated network, which can bias network statistics like transitivity, degree distribution, and clustering (Grannis, 2005) as well as mean degree (Kossinets, 2006). The final difficulty is that the graph is bipartite with two types of nodes (professors and departments) linked by edges (employment relations). Bipartite graphs are also called "affiliation networks." Most centrality measures are designed for one-mode graphs (Borgatti and Everett, 1997) but can easily be adjusted for use with bipartite graphs, or the original centrality measures can be used on the reduced form of the bipartite graph. Centrality measures (defined in the following section) differ based on the approach taken. Figure 5.3 shows two graphs that are different in their bipartite forms but identical in their reduced forms. In the figure, node size indicates degree. Graph 1 could illustrate three professors who have had very mobile careers, while graph 2 could illustrate 19 professors, each of whom is only affiliated with their training institution and their current employer. In the reduced versions of the graphs D and H are the most important nodes, while they are more important in bipartite graph one than in bipartite graph two. Calculating the nodes' centralities, D and H have similar eigenvector centralities in all three graphs. However, D and H have much higher standardized degrees and closeness centralities in graph 1 and the reduced graphs than in the bipartite graph two.[4] Because of these differences, I analyze the graphs both as bipartite and reduced, using the bipartite centrality measures proposed by Borgatti and Everett (1997) and illustrated in Robins and Alexander (2003) (although eigenvector centrality does not need to be adjusted for the bipartite graph

[4]Centrality scores for D in bipartite graph 1 are: .377 (eig), .667 (degree), .889 (closeness); in bipartite graph 2 they are: .469 (eig), .368 (degree), .836 (closeness); in the reduced: .490 (eig), .778 (degree), .818 (close)

(Bonacich, 1972)).

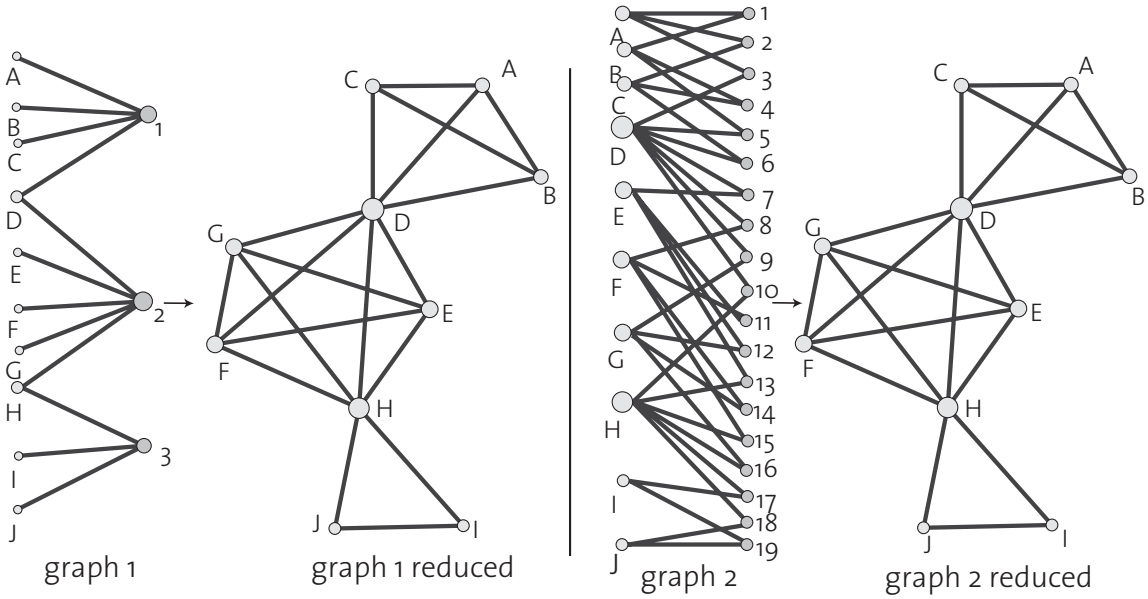

Figure 5.3: Reducing two different bipartite graphs into one reduced graph

Three different centrality measures were calculated: closeness centrality, standardized degree, and eigenvector centrality. Eigenvector centrality was chosen as the recursive measure, closeness centrality chosen as a distance measure (related to how quickly the department can access information from peers about funding, new research trends, recruiting, etc), and degree centrality was chosen as a straw man (it should capture department size and the experience of the department's faculty) and should be the most biased by the sample seed. Most studies focus on using one of the recursive measures of centrality since these measures indicate how important a node is based on the nodes it is connected to. This should be more robust to the sampling limitations. A good example of this would be a prestigious foreign university like Cambridge. While perhaps not many professors in the US system have worked at Cambridge, those that did should be the ones also connected to top US universities. A recursive measure would, as such, give Cambridge a high score, while the standardized degree would not. Surprisingly, as we will see in section 5.3, results are similar using all three measures.

Equation 5.1 illustrates the calculation for standardized degree for node i. Standardized degree measures the percent of all possible connections that an institution has to institutions in the reduced graph (D_i^r) or to professors in the bipartite graph(D_i^b). In both cases, the numerator is the degree of department (the number of edges it has) d_i, and the denominator is the total possible connections in the graph, n_p, the number of professors in the bipartite graph, and $n_d - 1$, the number of departments less the department whose standardized degree is being calculated in the reduced graph. As such, standardized degree measures a combination of department size (faculty and training depending on the graph) and the department's turnover rate.

$$D_i^r = \frac{d_i}{n_d - 1} \qquad\qquad D_i^b = \frac{d_i}{n_p} \tag{5.1}$$

Closeness centrality measures the inverse of the average distance between a given node (i) and all other nodes (j) and is illustrated as C_i^r for the reduced graph and C_i^b for the bipartite graph in equation 5.2. Here, n indicates the number of departments, and and D_{ij} is the distance from node i to node j. The version of the measure used for the bipartite graph (C_i^b) multiplies the average inverse distance by 2 to account for the fact that all connections between institutions are twice as far as in the reduced graph. Thus, closeness centrality measures whether actors can contact one another through short paths (Faust, 1997).

$$C_i^r = \frac{n-1}{\sum_{j=1}^{j=n} D_{ij}} \qquad\qquad C_i^b = 2 * \frac{n-1}{\sum_{j=1}^{j=n} D_{ij}} \tag{5.2}$$

Eigenvector centrality (Bonacich, 1972), is a recursive measure of prestige related to PageRank (Page et al., 1999), hubs and authorities (Kleinberg, 1998), and SALSA (Lempel and Moran, 2000). All four are based on the dominant eigenvector of the graph's adjacency

matrix and all gauge the importance of a node based on the importance of its neighbors. Page Rank adds a damping factor to the adjacency matrix (reducing the edges in the adjacency matrix by some small amount and adding uniform random edges from each node to all other nodes) and then calculates eigenvector centrality. The PageRank adjustment is necessary when a graph has directed edges leading into a node, but no edges leading out. Both SALSA and hubs and authorities use the dominant eigenvectors of the adjacency matrix times its transpose (and vice versa) with SALSA using row and column standardized versions of the adjacency matrix.

For eigenvector centrality, given the adjacency matrix A, where entry A_{ij} is 1 or 0 in the bipartite graph, or the number of connections between institutions in the reduced graph, and where e is the eigenvector pared with A's largest eigenvalue, λ, the i^{th} entry of vector e is the eigenvector centrality for the i^{th} node.

$$Ae = \lambda e \tag{5.3}$$

In other words, the centrality scores are the principle eigenvector of the adjacency matrix. For the bipartite graph, eigenvector centralities for individuals are simply dropped.[5]

It is expected that the degree centrality score will be the most influenced by department size and sampling bias while the recursive measure should be more robust. All the centrality scores are continuous and can be converted into a rank comparable to prestige rank. Analyses were conducted using both the continuous measures and the rank.

There are three variables exogenous to the networks: the domestic and international ranks described in the first section of the paper, and department size. For domestic universities, department size was taken directly from the NRC report when possible, and from departmental web sites when not. Information was drawn from departmental web sites for non-US universities.

Two of the twelve networks are depicted in figure 5.4 using the Kamada-Kawai spring

[5]PageRank and hubs and authorities were also tested, yielding similar results.

layout algorithm (Kamada and Kawai, 1989). This algorithm places "springs" between each pair of connected nodes, where the strength of the string is proportional to the strength of the edge, and places the nodes to minimize the springs' "energy." Thus, nodes are connected in clusters with the nodes they share many connections with. I present just 2 of the 12 graphs for the sake of brevity. The first graph in figure 5.4 is the bipartite graph from sample one (the prestige sample), including all edges (tenure, non-tenure, and student). The size of the nodes indicates their degree and the shade indicates whether they are institutions (grey) or individuals (white). Sampled institutions, of course, have high degrees and are central while European English-speaking institutions are also central but with smaller degrees. The halo of small institutions indicates small departments like UCSF (labeled) or non-profit and public institutions like the Census Bureau. The second graph in figure 5.4 is sample two's (the less prestigious sample) reduced graph excluding non-tenure track edges. The institutions that were part of the first sample remain central, though less dominant, as they were not the sample's seed, while sampled institutions like Yale take a more dominant position. In those analyses excluding non-tenure track edges, foreign institutions either dropped out of the graph or moved to the periphery. Self-edges (indicating that an institution had two relationships with the same individual i.e. training and then employing the same person) become apparent in the second graph because it is sparser. Removing student edges as well, the prestigious central institutions lose a little centrality.

Table 5.2 shows the descriptive statistics for all graphs. Average degree indicates the average number of individuals the organization is associated with in the bipartite graphs and the average number of organizations sharing connections to professors in the reduced graphs, weighted by the number of professors they had in common. Note that the average degree is smaller than the average academic department size because the graphs (particularly those including non-tenure track edges) include peripheral non-academic institutions that only one or two individuals have worked for. Average distance measures the aver-

Figure 5.4: Two sample graphs

age number of jumps to get from one institution to another for the reduced graphs and institution-individual-institution jumps for bipartite graphs. Finally, diameter measures the shortest path between the institutions that are the most distant. Comparing the two samples in table 5.2, the two samples seem similar in at least their descriptive statistics. The reduced graphs have higher average degrees than the bipartite because departments are linked to most other departments. The reduced all edges graph from sample 2 (the less prestigious sample) is more dense than the same graph from sample 1. This is also visible when we plot the two graphs. The version from sample one looks like a few main departments have ties to each other, while in the second sample the same graph looks like there are more small connections throughout the graph. We will see whether this is the case in a more rigorous test later in the paper. In all networks the diameter is small (equal to 4 in all cases) because the networks are star shaped with a few central institutions keeping all organizations closely linked. Removing student edges, average degree decreases because few nodes drop out but many edges do. Those nodes dropping out in the no student edges graph would be schools that only one person in the data is affiliated with–this could be, for example, due to a foreign professor trained abroad but working in the US.

	all nodes	org nodes	avg degree	avg distance	diameter
sample 1					
bipartite all edges	479	193	4.59	1.92	4
reduced all edges	193	193	9.87	2.30	4
bipartite no non-tenure	386	99	6.57	1.73	4
reduced no non-tenure	99	99	6.55	2.35	4
bipartite no student	457	178	3.56	2.08	4
reduced no student	178	178	8.10	2.45	4
sample 2					
bipartite all edges	425	241	3.66	1.98	4
reduced all edges	241	241	21.84	2.28	4
bipartite no non-tenure	273	89	5.79	3.83	4
reduced no non-tenure	89	89	7.44	2.37	4
bipartite no student	421	237	2.95	2.07	4
reduced no student	237	237	12.90	2.35	4

Table 5.2: Descriptive statistics for the 12 graphs

5.3 Analysis

Generally, the analysis finds that ranks generated from centrality measures are strongly correlated to prestige, though the strength of the relationship varies by graph. Closeness centrality changes when the graph is reduced, eigenvector centrality changes when student edges are removed, and mean degree changes both when the graph is reduced and when student or non-tenure track edges are removed.[6]

Using equation 5.4 to calculate the sum square deviations between the predicted and actual ranks for the top universities, I assessed which graph's centrality scores best predicted academic rank. G_s is the graph's sum of squared errors, u is a university, r is u's NRC rank, and e, c, and d are the eigenvector, closeness, and degree centrality ranks, respectively. In this way, the graphs were assessed by their ability to generate centrality measures predictive of prestige for all three types of centrality scores. One could use the original data in the appendix to do the same calculation for each centrality score independently. However, given the high correlation across the three centrality scores illustrated in figure 5.4, it was both reasonable and parsimonious to asses the graphs' predictive quality jointly for the three centrality scores.

$$G_s = \sum_{u=1}^{u=10} [(e-r)^2 + (c-r)^2 + (d-r)^2] \tag{5.4}$$

The first three columns of table 5.3 show ranks generated from the three centrality scores for the best graph and the second shows those from the worst graph. The bipartite graph from sample 1 (the more prestigious sample) excluding non-tenure track edges was the best predictor of academic rank, while the reduced graph from sample 1 including all edges was the worst predictor. It is notable that the best and worst graphs have similar predictions. The primary difference in their predictive values stems from their inconsistent ranking of UNC Chapel Hill. The graph that is reduced using all edges from sample one

[6] All the listed changes are significant at a 95% confidence level.

grossly under-estimates UNC's prestige rank compared to graphs omitting non-tenure track edges (not shown here, see appendix). This would make sense if UNC hired fewer post docs and visiting professors. The last two columns of table 5.3 show the average predicted rank for each school when we average the eigenvector, closeness, and degree centrality ranks across all 6 graphs in each of the two samples (the average of 18 predictions). In the table, the average rank is in bold if the school was part of the sample seed. These two columns highlight the fact that departments have higher rank by all three centrality scores when they are part of the seed. Nevertheless, the top schools remain relatively highly ranked even when left out of the seed. This emphasizes the fact that the sampling bias does influence the analysis somewhat, though the non-sampled schools are still appropriately ranked. While not listed in table 5.3, it is important to note that all the graphs excluding student edges (both sample 1 and 2) were significantly *worse* predictors of rank. In fact, three of the four graphs excluding student edges landing are in the bottom four (of 12) predictions. This lends support to the hypothesis that over-training accounts for a large portion of the centrality-prestige correlation.

		best graph*			worst graph**			across graphs	
	NRC rank	eigen rank	closeness rank	degree rank	eigen rank	closeness rank	degree rank	sample 1	sample 2
UChicago	1	2	2	2	2	3	3	**2**	12
Wisconsin	2	1	1	1	2	1	1	**1**	8
Berkeley	3	5	4	5	4	6	5	**5**	5
UMichigan	4	4	5	4	1	4	2	**3**	13
UCLA	5	6	6	6	5	2	6	**4**	14
Chapel Hill	6	15	15	13	55	21	16	15	15
Harvard	7	3	3	3	57	5	4	**6**	7
Stanford	8	7	7	7	6	7	7	**7**	9
Northwestern	9	11	10	12	10	12	12	11	**4**
U of Washington	10	37	28	29	81	35	45	41	23

$*$ best = sample 1, bipartite, no non-tenure edges
$**$ worst = sample 1, reduced graph, all edges

Table 5.3: Centrality rankings for the best and worst graphs

The three centrality measures are highly correlated with one another, as illustrated in table 5.4. The first column shows the correlation between eigenvector and closeness cen-

trality, the second shows the correlation between eigenvector and degree centrality, and the third column shows the correlation between closeness and degree. The main entries of table 5.4 indicate rank correlation between the centrality score ranks and prestige ranks, while the numbers in parentheses are the correlations between the continuous centrality scores (see equations 5.1, 5.2, and 5.3). While the first 12 rows illustrate the correlations within graphs, the last row is the overall correlation, averaging over all graph specifications. All the correlations using rank generated from centrality measures are better than those directly using the continuous centrality measures. The three centrality measures are inconsistent in the reduced graph from sample one with all edges, the same graph that was the worst predictor of prestige. This is somewhat surprising since visually inspecting that graph, more than any other graph in the analysis, this one is heavily centered around the top schools, and shows strong edges between them. It could be that while this graph is good at predicting the top schools, it fails to predict the others. The key finding in table 5.4 is that the ranks generated by the three centrality measures are similar. Researchers generally use the recursive measures because of the aforementioned reasons, such as being robust to sampling bias. However, all three centrality measures generate the same outcomes, suggesting that even the simplest measures, like standardized degree, are robust.

All the centrality measures are strongly correlated with academic rank as calculated by the NRC (domestic) and US News and World Report (international). For domestic rank, the ranks generated using eigenvector centrality have a .68 rank correlation compared to .72 using closeness and .73 using degree (calculated across all observations where an observation is an academic department in one of the 12 graphs). Correlations are slightly lower (.55, .59, and .59) for foreign international academic rank, because this ranking is not specific to sociology. Correlations between centrality and prestige varied substantially across the individual graphs when they are calculated separately. For example, ranks generated from eigenvector centrality had a correlation with domestic prestige ranging from .39 for sample one's bipartite graph including all edges to .8 for sample one's reduced graph

excluding non-tenure edges. The best predictions tend to come from excluding non-tenure track edges. This makes sense because non-tenure track edges tend to not follow the general hierarchical order in academia. Some individuals work in non-academic jobs at other institutions, drawing power away from the central academic institutions. Early in their careers, many individuals who later end up at lower ranked schools spend some time as post-docs at higher ranked schools. Finally, later in their careers many of those employed at high ranked schools will visit other schools based on characteristics besides prestige, like location, perhaps visiting the European University Institute in Florence over Wisconsin. These non-tenure track relationships create links between low and high ranked schools that do not exist in the regular academic labor market. Of the three types of centrality scores, closeness rank has the most consistent correlations with domestic prestige, ranging from .6 to .8 compared to degree rank, which correlates with domestic prestige from .62 to .77. Thus, we might say that the closeness centrality from graphs excluding non-tenure track relationships are the best predictors of prestige.

graph type			eig-close	eig-degree	close-degree
samp	reduce	edges			
1	yes	all	.614 (.733)	.589 (.888)	.889 (.840)
1	yes	PhD & ten	.915 (.866)	.948 (.989)	.922 (.878)
1	yes	no PhD	.929 (.891)	.935 (.977)	.927 (.870)
1	no	all	.979 (.759)	.865 (.990)	.836 (.792)
1	no	PhD & ten	.983 (.681)	.935 (.981)	.930 (.732)
1	no	no PhD	.949 (.648)	.787 (.873)	.827 (.810)
2	yes	all	.959 (.828)	.931 (.987)	.958 (.863)
2	yes	PhD & ten	.977 (.897)	.955 (.944)	.969 (.951)
2	yes	no PhD	.975 (.854)	.940 (.924)	.955 (.928)
2	no	all	.985 (.811)	.903 (.988)	.905 (.817)
2	no	PhD & ten	.961 (.721)	.936 (.974)	.952 (.764)
2	no	no PhD	.908 (.767)	.841 (.944)	.924 (.696)
overall			.877 (.650)	.913 (.523)	.911 (.795)

entries are rank correlations
(...) are continuous correlations

Table 5.4: Correlations across centrality measures

In terms of biases introduced by using prestigious schools as sample seeds, in sample one (the more prestigious sample) both eigenvector and closeness centrality *under-ranked*

the sampled departments, though closeness centrality did less so. This is the opposite of what was expected; as explained before, it was anticipated that eigenvector centrality (as a recursive measure) would be a more resilient estimate of academic prestige. For sample two, using closeness centrality, seed institutions were ranked on average between 10 and 11 positions *higher* than their NRC ranks and slightly more than 9 positions too high using eigenvector centrality. In fact, the mean rank for the sampled institutions in sample 2 was 3-7 while the NRC mean rank was 13-16 (both 95% confidence intervals). These confidence intervals do not even overlap, suggesting sample bias. The reason that sample one actually under-ranked the sampled institutions is that the sampled institutions were the top ranked ones. As such, it was impossible to over-rank them. However, in the less prestigious second sample there is the anticipated sample bias, with the recursive centrality measure surprisingly *no* more resilient than closeness centrality.

Given the number of students the most prestigious schools train, excluding student edges should have a significant effect on prestigious schools' centralities. Surprisingly, this is not the case for eigenvector and closeness centrality. The mean closeness and eigenvector centralities for the top ten schools (averaged across all those graphs including all edges) are statistically indistinguishable from the averages across those graphs excluding student edges. The top ten schools do, however, have a statistically higher *degree centrality* in those including training edges. The effect of including training edges on degree centrality is inevitable since training more students directly increases departments' degrees. However, the findings from the closeness and eigenvector centrality scores suggest that the top schools are central in a hiring network even ignoring their important role in training.[7]

Another way to show that the relationship between centrality and prestige holds after accounting for over-training is illustrated in figure 5.5. To show this, I re-ranked universities using the average of their closeness centrality ranks from those graphs that either included all edges or excluded student edges. The newly generated rank excluding student edges

[7]The same is true in an analysis using top 20 schools.

is shown on the x-axis while the new rank from all graphs including all edges is on the y-axis. The correlation (for all observed departments) between the two new rankings is .878, showing that ranks with and without training edges are similar. The graphic focuses on the origin of the graph where the more prestigious schools are located. If training future academics is a key component of prestige, we would expect the most prestigious schools to lie below the 45 degree line, with higher ranks when we include training edges. However, this is not the case as most schools lie just along the 45 degree line- only the University of Chicago follows this pattern. The top schools' NRC ranks are also shown in parentheses though there is no particular pattern for these top schools. In conclusion, highly ranked schools are central to academic hiring networks whether or not we consider their training role.

Up to this point, it has been shown that the effects of reducing the bipartite graph, excluding training or non-tenure track edges, sampling prestigious school, and the choice of centrality measures all have an effect on the relationship between centrality and prestige. On the other hand, the most important aspect of the analysis to this point (and this will become even clearer in the regression analysis) is how surprisingly robust the results are to these choices. While the strength of the relationship between prestige and centrality can change a little in response to these methodological choices, overall there is a strong and significant relationship between centrality and prestige that persists regardless of the approach taken.

We can also test the importance of training using a k-core analysis. A k-core groups together nodes based on both their clustering and their relative popularity, leaving the highest k-core to include the most prestigious departments. First, the graphs are separated into subgraphs where each node has at least degree k within the subnetwork. The subgraphs are calculated by recursively pruning those nodes with degree less than k, producing subnetworks that are interconnected at the same level. The groupings change when training edges are removed. Among the top ten schools, three schools are in the top k-core more

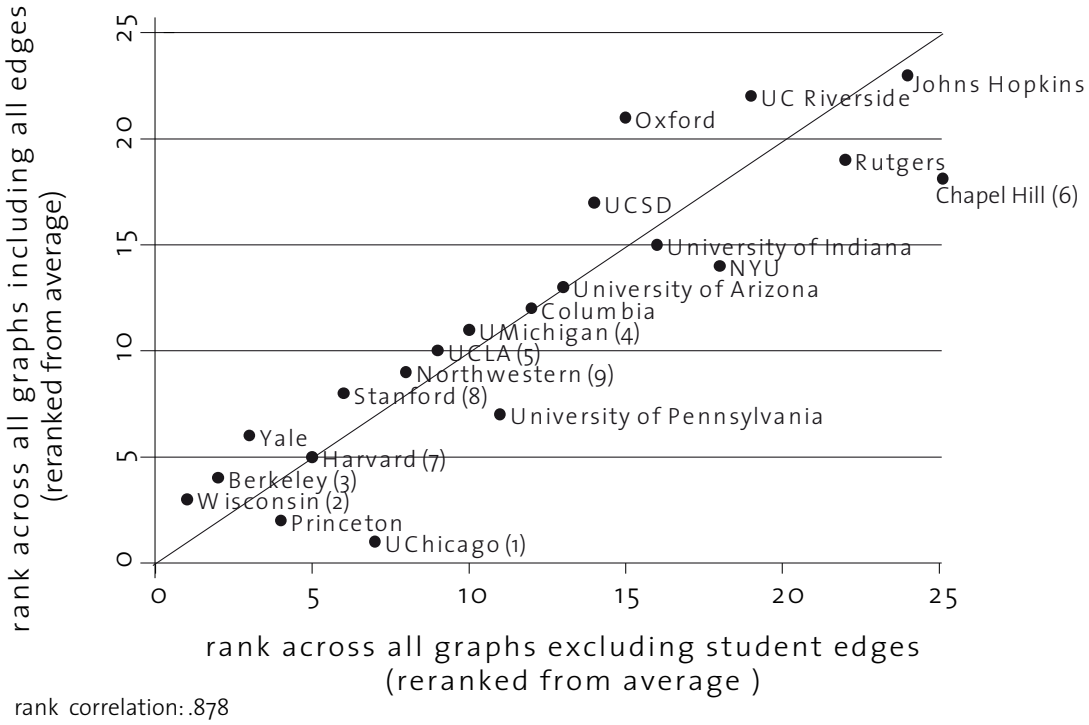

Figure 5.5: Closeness centrality ranks by edge inclusion

frequently among those graphs including all edges than among those excluding student edges. More striking, many more foreign departments enter the top k-core when training edges are excluded (European University Institute, Cambridge, the London School of Economics, and Oxford appear in the top k-cores more when training edges are removed). Foreign institutions are more important when we remove training but include non-tenure edges because academics tend to visit the same foreign universities and since each of these visits are short, many professors can visit bringing prestigious foreign institutions into the center of the graph. Excluding non-tenure track positions, foreign institutions do not enter the top k-core at all.

Figure 5.6 shows the k-cores for the graph excluding PhD training edges from sample 2, an exceptional graph in the k-core analysis because it is the only one where the top schools were not in the top k-core. In this graph, the top k-core (black) was dominated by foreign

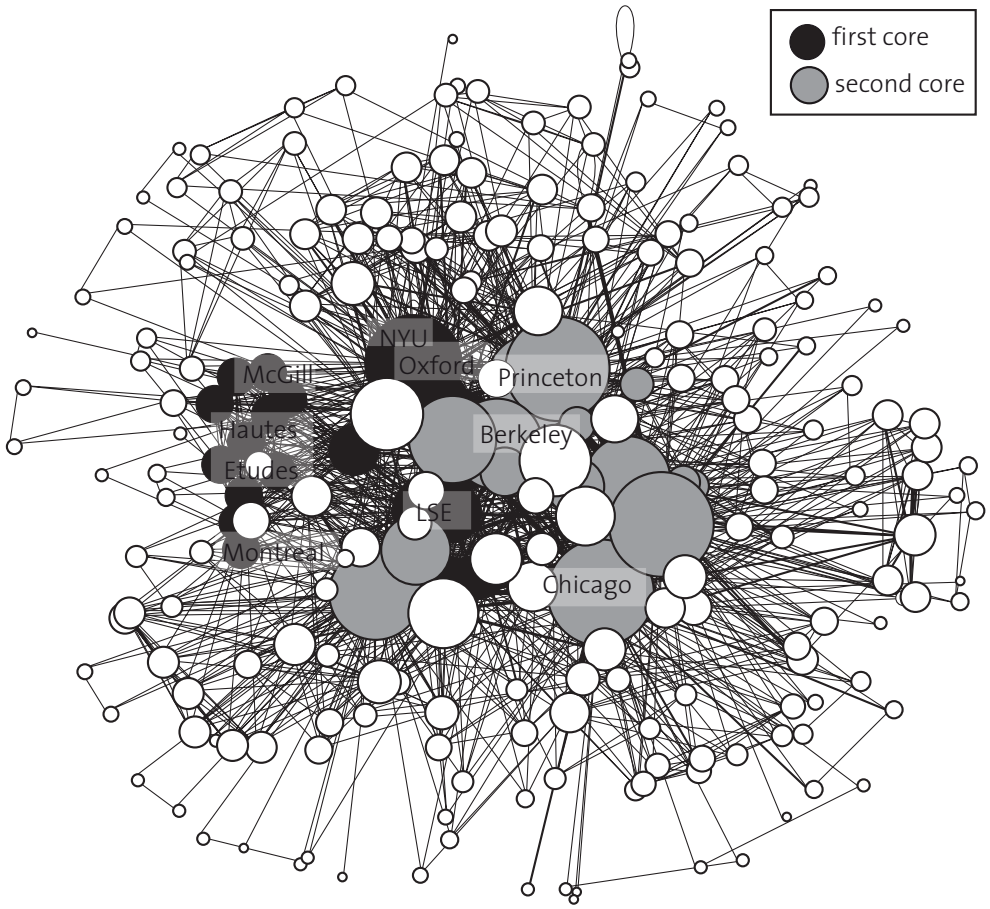

Figure 5.6: K-core for the reduced sample 2 graph excluding PhD training edges

institutions (LSE, Hebrew University, McGill, Universite of Quebec, Montreal, and Paris, University of San Paolo, and Oxford) and also included some domestic institutions (NYU and UCSD). The second highest ranked k-core (grey) includes the traditionally high-ranked schools. Inspecting the raw bipartite graph (not illustrated here), it is clear that there are two main clusters. Both clusters have many prestigious individuals and institutions in them, but one cluster is largely foreign and slightly larger than the second group of traditionally prestigious schools. In sum, the k-core analysis shows that the top ten schools lose some of their dominance without training edges, and that the top British institutions are a central part of the American sociology labor market.

I test the hypothesis of whether department size drives the correlation between aca-

demic departments' prestige and labor market centrality by first running bivariate regression between each of the centrality measure ranks and the actual academic rank. Then faculty size and the variables related to graph specification are added, showing that faculty size accounts for very little of the relationship between hiring network centrality and academic ranks. Finally, all the centrality scores are used as predictors in the same equation followed by a Wald test of equality between the centrality measures' coefficients. Results are illustrated in table 5.5.

The three centrality scores have approximately the same predictive value for prestige regardless of whether or not we control for faculty size. A one-position increase in centrality rank predicts at least a .5 position increase in NRC or Newsweek academic rank, as shown by the coefficients in the first three columns for domestic and international rank in table 5.5. In the first three columns of each entry we see a coefficient in bold. This is the coefficient for the centrality measure in a bivariate regression without any controls. The network data used to calculate these regressions is shown in the appendix. The non-bold entry directly above the bold bivariate coefficient shows the coefficient when controlling for faculty size and graph specification. For closeness centrality, we see that in a bivariate regression a one-position improvement in centrality-generate rank is associated with a .584 position increase in prestige rank. After controlling for department size, this drops to .525. Most interesting, there is a negative coefficient on department size, suggesting that the labor market position is so important that for two equally sized programs in the same position in the labor market graph, the smaller department would actually be more prestigious. The last column of 5.5 shows the results of regressing all three centrality measurements together. (One should note that this introduces the problem of multicollinearity which increases the standard errors of coefficients.) In this joint model, eigenvector centrality seems to provide no information not provided by the other two measures. While we expect that including training edges would increase predicted prestige for the top schools, it is surprising that in fact it increases predicted prestige by about .2 positions in rank in the regression including

all schools.

Going back to the hypothesis that the prestigious schools maintain their positions by overtraining, we find that running the same regression for only the top 50 schools in the sample, excluding PhD training edges increases predicted rank at least 2 positions. This is the opposite of what one might expect if the top schools overtrain and rely on placing fresh PhD students to increase their standing in the field. Finally, running these same regressions for all departments, using only those graphs excluding PhD training edges, a one position increase in eigenvector rank is still correlated with a .42 increase in domestic academic rank and a one point increase in closeness centrality rank is related to a .45 increase in prestige. In sum, excluding student relationships *slightly* weakens the relationship between graph centrality and prestige (about 20%), but overall the relationship is still strong.[8]

In sum, the regression analyses allow us to definitively reject the idea that the relationship between employment network centrality and departmental prestige are driven by department size and prestigious schools' dominance in training new PhDs.

5.4 Conclusion

This paper began with two main hypotheses regarding the relationship between the sociology academic employment network and academic rankings. First, I suggested that the relationship might be driven by department size and by the dominance of a few departments training the bulk of sociologists combined with the general over-training of sociologists. Second, I posited that the relationship could be driven by researchers' methodological choices of how to sample academic sociology networks, what sorts of employment relationships to include, and which centrality measures to use. I found support for the fact that training does play a definitive role in academic prestige. The initial analyses showed that

[8] A Wald test of equality between the centrality scores' coefficients indicates that for both domestic and foreign rank the effects of eigenvector centrality is significantly different from both closeness and degree, though closeness and degrees' effects are statistically indistinguishable from each other.

	domestic rank				international rank			
eigenvector	.481***			-.037	.588***			-.196*
	.542*				**.608***			
closeness		.525***		.339***		.663***		.437**
		.584*				**.669***		
degree			.516***	.241***			.670***	.436***
			.577*				**.686***	
faculty size	-.492***	-.460***	-.499***	-.466***	-.121	-.077	-.067	-.063
all edges	.260	.477	.228	.395	.006	.307	.265	.373
no stud edges	.233*	2.47*	1.86	2.29*	-.891	-.629	-.130	-.202
bipartite	.120	.089	.464	.265	.006	.248	.097	.225
sample one	.461	.461	.954	.650	-1.27	-1.51	-1.85	-1.73
R^2	.514	.571	.560	.583	.253	.312	.319	.330
	.426	**.493**	**.469**		**.239**	**.293**	**.314**	
coefficient tests	$\beta_{eig} = \beta_{degree}$		P:.0013		$\beta_{eig} = \beta_{degree}$		P: .0001	
	$\beta_{eig} = \beta_{closeness}$		P: .0004		$\beta_{eig} = \beta_{closeness}$		P: .0036	
	$\beta_{closeness} = \beta_{degree}$		P: .0004		$\beta_{closeness} = \beta_{degree}$		P: .9789	

bold text indicates bivariate regressions
$***$ indicates significance at the .001 level

Table 5.5: OLS regression predicting academic prestige

the top institutions are somewhat less central using a network excluding training edges, and the regression suggested that network centrality has slightly less predictive power when it is defined exclusive of training edges. There was no support for the hypothesis that department size drives results. With respect to methodology, sample seeds certainly biased predictions (particularly degree centrality), although the results were still more or less accurate. Finally, the decision to analyze the reduced or bipartite graph seems to have no effect.

The major finding of this paper was that the relationship between academic rank and centrality in the academic hiring network is very robust. Independent of graph specification (the centrality measure used, the sample seed, or whether the bipartite or reduced graph is used) and independent of prestigious departments' size, or the fact that prestigious schools train most PhD's, the prestigious schools are *still* at the center of the academic labor market.

Other researchers finding a correlation between academic prestige and labor market position interpret this as an academic "caste system" or infer that training and placement

consolidate departments' prestige (Burris, 2004). While I find evidence confirming these patterns, I hesitate to consider it a "caste system" per se and perhaps would consider it a case of positive feedback. If faculty moved strictly in castes (prestigious faculty moving between prestigious institutions and the other faculty moving among other institutions) there would not be this strong relationship between the hiring network centrality and academic rank. Rather, there would be two separate cores: lower ranked schools trading faculty with each other and higher ranked schools trading faculty with each other. Instead, peripheral schools trade faculty with the most prestigious schools rather than with each other. They do this first by hiring graduates from the more prestigious schools, and then by passing their successful professors on to the more prestigious schools. It it is these trades, or academics' preferred career paths, that keep the most prestigious schools in the center of the employment graph (even when training edges are excluded). This pattern of career moves is advantageous for the institutions that are already prestigious. As such, the pattern of the academic employment network could reinforce current prestige rankings.

Chapter 6

Conclusion

This dissertation found three types of stratification. The first type of stratification was an extension of functional stratification, stratification that serves a purpose in the labor market and, in this case, might also work to facilitate information flow. The second type of stratification is simply selection or the sorting of people into different categories based on their incomes and skills. The third sort of stratification is an actual change in the distribution of wages and benefits. The first two types of stratification are often mistaken for the third.

The first type of stratification is from a normative perspective at least neutral if not positive. This type of stratification was evident in the fourth paper (chapter 5) on academic employment networks. There are two typical types of articles on academic rankings: those that critique ranking methodologies and those that lament the static system. This is a sympathetic position; it certainly seems unfair that university administrators can invest a lot of resources into a program but make no progress in the rankings. There are several mechanisms of positive feedback that reinforce highly ranked schools' status and one could be the academic labor market. A stratified academic labor market would look like two separate labor markets: the prestigious and the others, each not trading faculty with the other. But this is not the case; prestigious schools are at the center of a cohesive labor market. While good schools' dominance in training (PhD) and hosting visiting professorships further contribute to this pattern, they are not the whole story. Rather, it seems to possibly be related to preferences to transition to higher ranked schools or not at all. Departments' positions

in the center of the employment network could reinforce prestige through multiple mechanisms. First, those with more publications and more possibilities to get funding move to top ranked schools. This places departments at the center of information in in the field, a distinct advantage in research. While this sort of stratification might be unfortunate for departments, for individual academics it serves a functional role both as a natural career path and it creates a shorter distance across the entire academic field. Thus, it is far from clear that this sort of stratification should be viewed negatively.

The second sort of stratification is essentially selection. For example, in the third paper (chapter 4) we found that in an environment with temp agencies charging fees as a percent of workers' salaries, lower skilled worker would be sorted into atypical jobs. In the second paper (chapter 3) we saw that lower skilled workers are sorted into fixed term jobs. Thus, in the labor market we see that indirectly hired workers (i.e. temp or outsourced workers) and fixed term workers are paid less. This is a form of stratification; atypical employment is a mechanism separating low and high skilled workers. However, this stratification is not necessarily negative if workers earn the same in the atypical positions as they would in regular positions. The introduction of atypical employment increased stratification in the sense that the lower skilled are demarcated from the other workers, but does not necessarily decrease their economic well-being. This type of stratification does not necessarily (though sometimes, such as in the case of residential segregation) call for policy interventions.

The third sort of stratification is the stratification that in itself creates a more unequal income distribution. Atypical employment allows firms to create a class of workers with lower pay and fewer benefits than those same workers would have had in a permanent employment relationship. This stratification is evident in paper 2 (chapter 3) where we found that fixed term workers earn less, independent of selection. This type of stratification calls for policy intervention. Unfortunately, policy is in some sense culpable for this sort of stratification in the first place. Systems of contracts that allow firms to avoid employment protections or let them cash in on tax benefits designed to subsidize wide spread employer-

provided health insurance (even though the firms are not actually providing it) are not in the public interest. Worse, these arrangement further disadvantage those workers already in the weakest positions in the labor market. Many of the policies designed to alleviate this problem actually exacerbate it: bonuses for converting fixed term to permanent contracts ultimately provide an incentive to firms to hire more workers through fixed term work; special savings accounts to insure against unemployed periods for atypical workers ultimately offer the well-off more retirement funding; time limits on fixed term contracts encourage atypical workers to be fired; and diverse judicial rulings fail to systematically penalize firms for misclassifying workers and denying them the benefits of official employment.

Each of these types of stratification is important to understand, though only the third necessarily calls for intervention. This dissertation has taken the first steps to distinguishing and analyzing each type of stratification and outlined relevant policy options.

Appendices

Appendix A

Chapter Two: National context and atypical employment

	observations	mean	std dev	minimum	maximum
self employment	536	18.07	11.11	4.7	62
part-time employment	444	15.17	7.31	1.6	35.7
fixed term employment	351	7.96	5.65	.7	30.4
union density	480	37.51	20.51	8.1	88
strike rate	264	1.69	3.62	0	25.08
temporary EPL	388	1.97	1.35	.3	5.4
regular EPL	388	2.18	.91	.2	4.8
EPL difference	388	.21	1.28	-3.6	3.1
non-compensation costs	407	20.95	7.08	2.6	36.4
unemployment rate	467	7.71	4.11	.5	24.2
PPP manufacturing wage	402	20.43	5.83	7.56	29.77
UI replacement	356	30.18	12.29	3	65
innovation index	198	.38	.20	0	1
patent application rate	552	.030	.078	0	.56
proportion women	561	.44	.041	.34	.49
gini coefficient	470	.29	.043	.207	.41

Table A.1: Descriptive statistics

Data Codebook

- `country`
  The name of the country

- `year`
  The year

- `pSelfEmployed`
  The percent of workers that are self-employed (Source: OECD)

- `stermEuro`
  Percentage of workers who are fixed term (Eurostat)

- `tempComp`
  Percentage of workers who are temps (combined Eurostat and OECD). Not used because the two input variables did not correlate very well.

- `pt_oecd`
  Percentage of workers who are part time workers (OECD source)

- `uDensity`
  The percentage of workers who are members of a union. Ten years are from the OECD, every 5 years data is from Checchi and Lucifora. These two data sources matched. Data post 2000 from Lawrence and Ishikawa were tested, but the numbers do not match.

- `strikesRat`
  Total strikes and lockouts per 100,000 people. Population is from the World Bank's World Development Indicators database. The number of strikes and lockouts are from the ILO's Yearbook of Labour Statistics. A strike is a temporary work stoppage effected by one or more groups of workers with a view to enforcing or resisting demands or expressing grievances, or supporting other workers in their demands or grievances. A lockout is a total or partial temporary closure of one or more places of employment, or the hindering of the normal work activities of employees, with one or more employers with a view to enforcing or resisting demands or expressing grievances or supporting other employers in their demands and grievances. Original data was collected from conciliation services and augmented with newspaper reports, worker's organizations, etc. A national strike is considered the same as a small firm-level strike.

- `ftEPL`
  This is a 0-6 point scale developed by the OECD in the "Employment Outlook" on employment protection legislation for fixed term employees. The index includes restrictions on types of work for which temp agencies is illegal, restrictions on number of renewals of contracts, maximum cumulated duration of contracts. The maximum number of successive contracts ranges from 1 (Netherlands and Belgium) to unlimited (UK, USA) and the maximum contract duration ranges from 12 months in Sweden to no limit in many OECD countries.

- `regEPL`
  This is a 0-6 point scale written by the OECD in the "Employment Outlook" on employment protection legislation for permanent employees. This index includes notification procedures, time delay before the firing process can start, length of notice before dismissal, severance pay, strictness of defining an unfair dismissal, length of probationary period when restrictions do not apply to firing the worker, length of

compensation following the dismissal, and the possibility of reinstatement following the dismissal.

- `difEPL`
  The difference between the two EPL indices.

- `nonCompCost`
  This is the percent of average compensation costs that are not wages. These costs include payroll taxes paid by employers, as well as health insurance and pensions paid by employers. There are two sources for this data, the BLS and the ILO. The estimates from the two organizations are almost identical for most years. As such, they are averaged them together for those years in which there are 2 years of data.

- `unemploy`
  The unemployment rate. This measure is reported by the OECD online database. They define unemployment using the ILO guidelines. These numbers differ from national accounts because they attempt to include those who are not registered with the unemployment office. The number is the unemployed population/civilian labor force and is seasonally adjusted.

- `gini`
  The country's post-transfer gini coefficient. This is pieced together from various sources including the World Development Indicators (World Bank), OECD, Luxembourg Income Study, US Census Bureau, and (Andrew Leigh 2004) for Australia. Almost all of these were almost identical with the exception of the BLS, which estimated much higher inequality than did the other sources. When multiple sources were available the average was used.

- `hWagePPP`
  The country's mean manufacturing wage, in 2006 dollars adjusted for PPP. Mean manufacturing wages come from the BLS Office of Foreign Labor Statistics. The PPP adjustments come from the World Bank's World Development Indicators database.

- `unemplRep`
  The replacement rate of unemployment benefits, indicating what percentage of his salary a worker receives on unemployment. This measure was generated by the OECD and is defined as the average of the gross unemployment benefit replacement rates for a worker with a full record of employment at two earnings levels (67% and 100% of average production worker earnings), in three family situations (single, married with dependent spouse, married with spouse in work), and with three different unemployment spell durations (first year; second and third year; fourth and fifth year).

- `innovate`
  This is a 0 to 1 scale measuring the level of innovation in a country. The first com-

ponent is the total early-stage entrepreneurial activity (TEA index) which measures the total rate of early-stage entrepreneurial activity among the adult population aged 1864 years, inclusive. This is estimated by the Global Entrepreneurship Monitor. The second component is the firm birth rate from Eurostat. Each was standardized on a scale of 0-1 and then the average was used when both were available. The two measures had a correlation of .343** for those observations for which I had both measures. This is not used in presented results.

- `patent`
  The number of patent applications per person (patents from UN Statistical Division, population from the World Bank).

- `women`
  This is a control indicating the percent of a workforce that is women (World Development Indicators)

- *Interpolation Note*
  Linear interpolation was used for gaps between two time periods of data. (i.e. if 1995 and 2000 were available, 1996-9 were interpolated while 90-94 and 2001-6 were left as missing. Labor policy was also interpolated which is inaccurate since policies happen at one moment in time, not gradually. However, this is conservative in that we assume that the policy change did not occur in any particular year.

Appendix B

Chapter Three: The fixed term wage gap

	country									
	AT	BE	FI	FR	DE	EL	IE	IT	NL	ES
number observations	1,721	1,804	7,817	8,468	8,677	1,892	1,827	5,589	3,593	3,120
fixed term(%)	3.72	6.21	7.71	8.43	6.13	11.21	3.61	8.21	8.77	21.47
family										
w/ ptnr & kids (%)	43.46	50.61	48.29	47.96	38.99	46.72	53.91	44.75	45.48	40.22
w/ ptnr, no kids (%)	29.39	22.25	37.42	31.67	30.71	31.2	22.17	29.51	24.86	32.54
no ptnr & kids (%)	3.14	7.98	2.62	3.8	6.45	1.59	2.79	1.4	6.48	1.57
alone (%)	11.27	16.13	10.53	13.13	19.89	10.94	8.65	9.66	22.15	8.78
ptnr & parents (%)	2.27	0.22	0.013	0.071	0.38	1.22	0.33	0.43	0	1.09
w/ parents (%)	11.04	3.27	0.87	3.77	3.28	14.01	12.21	15.05	0.97	17.05
education										
no secondary(%)	0.23	5.32	19.00	17.94	2.94	23.2	15.54	41.98	3.30	19.07
1st secondary(%)	13.13	16.57	36.08	8.87	64.56	12.79	25.34	8.36	14.31	24.29
2nd secondary(%)	73.73	32.04	0.08	43.62	5.06	34.57	32.51	34.76	48.17	19.1
tertiary(%)	4.24	5.93	21.21	0.402	12.81	6.45	9.85	1.41	23.21	11.44
university(%)	8.13	25.39	21.95	22.7	14.63	18.66	10.18	13.31	9.91	11.86
postgrad(%)	0.52	14.08	1.69	6.47	-	4.33	6.57	1.79	1.09	14.10
effort										
mean hrs/wk	38.16	38.93	-	35.05	39.87	40.54	37.89	38.89	34.03	40.6
wks/yr	49.02	-	49.26	50.17	49.2	48.37	49.32	50.42	49.54	48.1
income										
net wage($)	15,003	17,488	-	15,626	-	5,136	15,824	12,373	-	11,269
gross wage($)	18,168	24,789	17,695	-	25,458	3,526	19,110	-	24,784	11,096
occupation										
agricultural (%)	11.55	1.97	12.25	4.19	13.9	14.53	18.66	10.59	1.35	19.54
elementary (%)	7.44	3.27	1.23	8.43	2.63	6.57	9.52	32.95	7.06	10.51
service (%)	46.09	48.01	32.03	32.2	39.88	29.68	21.98	27.83	21.57	20.81
skilled (%)	22.28	17.59	21.26	27.09	23.72	24.71	23.84	-	15.12	27.39
manager (%)	7.06	7.93	8.41	19.08	5.48	10.11	10.64	16.15	28.83	7.87
professional (%)	5.58	21.24	14.68	8.99	14.4	14.4	15.36	12.49	26.06	13.88
misc										
mean age	40.31	40.24	43.96	41.66	42.03	39.69	41.36	41.92	40.64	38.95
male (%)	56.71	53.21	47.72	53.00	55.64	61.47	54.73	59.42	53.97	61.22
gov't employ(%)	30.62	37.69	30.24	30.89	27.06	37.84	36.51	28.72	25.69	23.97
supervisor (%)	35.90	33.69	-	23.48	22.00	12.73	34.03	-	28.93	-
native born(%)	80.65	92.63	-	89.67	86.33	96.09	91.84	97.03	-	-

Austria (AT), Belgium (BE), Finland (FI), France (FR), Germany (DE), Greece (EL), Ireland (IE), Italy (IT), Netherlands (NL), Spain (ES)
Currency is 2000 PPP dollars
In Italy blue collar workers were not distinguished by skill. LIS coding for the 12 category variable "pskill" was used as a basis.
In Germany university and post-grad are indistinguishable

Table B.1: Descriptive statistics by country

Appendix C

Chapter Four: Employment intermediaries

Parameter list

The first part of the list includes the parameters which were not tested in their entire parameter space. The second part of the list includes the 5 parameters that were tested. Many in the first list (like model length or grid size) are arbitrary and do not influence the model outcomes (they were not tested rigorously, but were varied in a few trial runs). Some of these parameters, like the distribution of jobs across firms, are based on specific empirical US data, while others, like the search radius of a worker, are more loosely based on empirical research (i.e. studies find that skilled workers look for jobs in a broader radius.) Skill floors, the continual generation of contractors, and the contractor's startup grace period were tested and found to have no effect on the model's findings, so the various tested parameter settings are not shown here.

Parameter	Definition	Default Value
stopTicks	length of run	600 or 1000
numWorkers	number of worker agents	1000
numFirms	number of firm agents	142
sizeX sizeY	grid size	100
feeRVar	variance for contractor fee rates	.05
maxCDistance	firms' search radius for contractors	.2
ξ	exponent distributing jobs across firms	2.1
γ	history weighting	.75
tPerm	contract worker's transition to direct hire	5
rDeath	revenue a contractor must maintain	.1
vSContractors	vacancy rate generating contractors	.04
oSContractors	outsourcing rates generating contractors	.02
cSTime	contractors' startup period to generate revenue	3
ceiling	a ceiling on unemployment and vacancy	.15
sSearchRWorker	worker's skill effect on search radius	5
maxWSTolerance	maximum deviation for job floor	3
minWSTolerance	minimum deviation for job floor	1
maxJSTolerance	maximum deviation for worker floor	3
minJSTolerance	minimum deviation for worker floor	1
hWeighting	weights firm's utility histories	.75
fRFloor	a floor on contractors' fee rates	.025
vDisutility	disutility for firms for vacancies	$-.1$
settings for tested parameters in those experiments in which they were not varied		
fRMean	contractors' mean fee rates	.2 (exp 2) .05 to .35 (exp 1)
cRWorker	contractors' mean search radius	.5 (exp 2) .1 to 1.0 (exp 1)
wSDist	workers' edu distribution	all labor (exp 1)
	all, accountant, programmer, min wage	(exp 2)
jSDist	jobs' edu distribution	all labor (exp 1)
	same as wsDist	(exp 2)
cAlphaMean	contracting's effect on compensation	NA (exp 1) .9 to 1.1 (exp 2)
cAlphaVar	variance of above	NA (exp 1) .05 to .15 (exp 2)
wVar	firms' workload fluctuations	.05 (exp 1) .05 to .1 (exp 2)

In the first experiment feeRateMean and contractorRadiusWorker were varied
In the second worker experiment SkillDist, jobSkillDist, contractedAlphaMean, contractedAlphaVar, and workVar were varied .

Table C.1: Simulation parameters

Classes and their instance variables

- Firms have:
  - X and Y locations
  - a list of their jobs

  - a list of their vacant jobs
  - a list of their employees
  - a change in workload (updated each round)
  - a pointer to their contractor
  - a utility (from contracted and direct hires as well as vacancies)

- Jobs have:

  - a pointer to their firm
  - a pointer to the contractor
  - a skill level
  - a skill floor for the least qualified worker they will accept
  - a pointer to their worker
  - the tick the job was last filled
  - the tick the job was last vacated
  - a comparator used to sort workers by how well they match the job
  - a list of unemployed workers, sorted by how well they match the job

- Workers have:

  - x and y locations
  - skill levels
  - a skill floor for the lowest job that they would accept
  - a quit propensity
  - the date they were last employed if currently unemployed
  - the date they were last hired
  - a list of vacant, visible jobs
  - their employer
  - their job
  - an effect on their salaries for a contractor match

- Intermediaries have:

  - x and y locations
  - a list of the firms employing them
  - a list of their assigned jobs
  - a fee rate (a percent of the worker's skill level)
  - the percent of assigned jobs they matched in the last round
  - revenue (based on their fee rate and their employees' skills)

Equations

In the notation below normal(x,y) means a draw from a normal distribution with mean x and standard deviation y. Similarly, uniform(x,y) is a draw from a uniform distribution ranging between x and y. The notation uses the following: i indicates indirect hires, d indicates direct hires, w indexes workers, f indexes firms, j indexes jobs, z indexes intermediaries, and t indexes time. Equations refer back to the parameter table C.1 as necessary.

- Initial job creation

  For each firm f, draw a number of jobs at the firm. If the number of jobs exceeds 10% if the workforce, redraw. In table C.1, ξ is the parameter that distributes jobs across firms.

$$nJobs_f = [1 - \text{uniform}(0,1)]^{\frac{-1}{\xi - 1}} \tag{C.1}$$

- Probability of worker w quitting in time t

  Note that variables with no subscript t are drawn just once, during the model setup.

  - Experiment 1

$$pQuit_{w,t} = .333(\rho_{w,t} + \tau_{w,t} + \sigma_{w,j,t}) \tag{C.2}$$

  - Experiment 2

$$pQuit = .5 * (\rho_{w,t} + \sigma_{w,j,t}) \tag{C.3}$$

  - For both:

$$\begin{aligned} &\textit{iff } pQuit > \text{uniform(0,1)}, \text{ quit} \\ &\textit{iff } pQuit < \text{uniform(0,1)}, \text{ stay} \end{aligned}$$

    where,

$\rho_{w,t}$	random quits	normal($qP_{w,i}$, .05)
qP_w	quit propensity	uniform(0, .3)
$\tau_{w,t}$	tenure effect	normal(1-$\frac{currentjobsticks}{total\,life\,ticks}$,.05)
$\sigma_{w,j,t}$	match quality	normal(ψ,.05)
ψ	if $ws_w > js_j$	$\frac{ws_w - js_j}{ws_w}$
ψ	if $ws_w < js_j$	$\frac{(js_j - ws_w)^2}{(js_j)^2}$
ws_w	worker skill	
js_j	job skill	

There are four parameters here not listed in the initial table including the variance of random quits (.05), the variance of the quit propensity (.3), the variance in match quality (.05), and the variance in tenure effect (.05). All are the variance of another main parameter and do not strongly effect the model.

- Fluctuation in the number of jobs at firm f in time t

$$\psi_{f,t+1} = \psi_{f,t} + \Delta * \psi_{f,t} \tag{C.4}$$

ψ_{ft}	=	firm f's number of jobs in time t
Δ	=	normal(0,wVar)
if unemployment $> 15\%$		$\Delta = \lvert\Delta\rvert$
if vacancy $> 15\%$		$\Delta = -1 * \lvert\Delta\rvert$

Note that wVar was a swept parameter. It is listed in table C.1. Also note that the rules limiting unemployment and vacancies are a simple proxy for economic dynamics in the real world that hold unemployment and vacancies in a tolerable range, a fact that is empirically observable.

- Worker's and job's skills

Skill distributions are set based on empirical educational distributions for workers in different occupations. Skill floors are assigned to workers or firms in the beginning of the model and remain constant. The skill floor is a uniform deviation from -1 to -3 plus the worker's or job's skill (the education scale ranges from 1 (less than fifth grade) to 11 (PhD)) to a minimum of 1.

- How firms search for intermediaries

Firms find the intermediary within a static search radius and pick the one who had the best match rate last round.

- Workers apply to all jobs they "see"

$$P_{wjt} = e^{\frac{-\delta * d_{wjt}}{ws_w * v}} \tag{C.5}$$

P_{wjt} = probability of worker w seeing job j in time t
ws_w = worker skill level for worker w
d_{wjt} = distance between worker w and job j in time t
ν = sSearchRWorker (skill's effect on search radius)
δ = if $\neq$ indirect job = 1
if = indirect job = cRWorker

- The cost of contracting

Firm f's cost of employing worker w in job j though intermediary z in time t is

$$TCC_{w,j,z} = ws_w^{\alpha} * (1 + fRate_z) \tag{C.6}$$

$WCI_{w,f,z}$ = worker w's cost to firm f using intermediary z
ws_w = worker w's skill level
$fRate_z$ = normal(fRMean, .05) (fee rate from firm z)
$\alpha_{w,z}$ = normal(cAlphaMean, cAlphaVar), for an indirect hire
$\alpha_{w,z}$ = 1 for a direct hire
$\alpha_{w,z}$ = 1 in experiment 1

Intermediary z's fee rate is held constant throughout its life. In experiment 1, the fee rate is varied, but α is held at one. In experiment 2, the fee rate is held at .2. value $\alpha_{w,z}$ measures how much relatively more or less the worker gets paid through an intermediary. This α is redrawn every time a worker is rematched through an intermediary, although the settings for alpha's distribution are held constant through every experiment.

- The decision to use an intermediary:

$$ind = \frac{\beta_{ipast}}{\beta_{ipast} + \beta_{dpast}} \tag{C.7}$$

iff $ind >$ normal(.5,.2), use intermediary
iff $ind \leq$ normal(.5,.2), hire directly

In experiment 1 a standard deviation of .1 was tested while experiment 2 tested a standard deviation of .2.

$\beta past_{i,t}$ = $\gamma * \beta past_{i,t-1} + (1-\gamma) * \beta_{i,t-1}$
$\beta past_{d,t}$ = $\gamma * \beta past_{d,t-1} + (1-\gamma) * \beta_{d,t-1}$
γ = history weighting
β_i = $\overline{MQ}_i$ - averagefeecost$_i$
β_d = $\overline{MQ}_d$

The average fee cost$_i$ is the average fee payment over all workers hired through intermediaries (n_i is the number of indirect workers) $= \frac{\sum feerate_{i,z,j} * ws}{n_i}$

The match quality from the firm's perspective for matching worker w with job j, $MQ_{w,j}$, where js_j is job skill for job j and ws_w is worker skill for worker w, is:

$$\text{if the worker is underskilled: } 1 - \frac{js_j - ws_w}{js_j}$$
$$\text{if the worker is overskilled: } 1 - \frac{(ws_w - js_j)^2}{ws_w^2}$$

For experiment two the calculations are the same, except the TCC cost function is used instead of the simple fee rate times skill. The means here:

$\beta_{i,t} = \overline{MQ}_i - .5\overline{TCC}_i$ is used instead of $\overline{MQ}_i - averagefeecost_i$

- Utility

Firm utility was calculated as a model output. This is basically the same calculation as used in the decision of whether or not to use an intermediary, except a small negative amount is added for vacancies.

$$U_f = N_{i,f}(\overline{MQ}_i - .5\overline{TCC}_i) + N_{d,f}(\overline{MQ}_d - .5\overline{TCC}_d) + (-.1N_{v,f}) \tag{C.8}$$

U	utility
$N_{i,f}$	number indirect hires at firm f
$N_{d,f}$	number direct hires at firm f
$N_{v,f}$	number vacancies at firm f
$\overline{MQ}_{i,f}$	average match quality for indirect hires at firm f
$\overline{MQ}_{d,,f}$	average match quality for direct hires at firm f
$\overline{TCC}_{i,f}$	average total compensation cost indirect hires at firm f
$\overline{TCC}_{d,f}$	average total compensation cost direct hires at firm f

- Intermediary death

Intermediaries' economic health is measured by dividing their total revenue by the number of jobs they have been assigned. If this revenue is less than 10% of the average worker's skill (remember that depending on the experiment being run, fee rates average around 20% of a worker's skill), the contractor dies. Thus the contractor's health depends on their ability to match workers with jobs and their fee rates.

Appendix D

Chapter Five: Academic employment networks

Table D.1: Data from 12 employment networks

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
UChicago	1	all edges	false	1	20	2	3	3
Wisconsin	1	all edges	false	2	28	3	1	1
Berkeley	1	all edges	false	3	5	4	6	5
UMichigan	1	all edges	false	4	11	1	4	2
UCLA	1	all edges	false	5	12	5	2	6
UNC Chapel Hill	1	all edges	false	6	41	55	21	16
Harvard	1	all edges	false	7	1	57	5	4
Stanford	1	all edges	false	8	2	6	7	7
Northwestern	1	all edges	false	9	35	10	12	12
U of Washington	1	all edges	false	10	22	81	35	45
U of Pennsylvania	1	all edges	false	11	13	74	15	17
U Indiana Bloomington	1	all edges	false	12		40	19	22
Princeton	1	all edges	false	13	15	8	10	10
U of Arizona	1	all edges	false	14		9	9	8
Columbia	1	all edges	false	15	10	69	13	13
UT Austin	1	all edges	false	16	27	53	20	27
Johns Hopkins	1	all edges	false	17	24	21	33	34
Penn State	1	all edges	false	18	40	32	45	43
Yale	1	all edges	false	19	3	12	11	11
Duke	1	all edges	false	20	14	52	30	33
NYU	1	all edges	false	21	39	23	28	29
UCSD	1	all edges	false	22	23	11	23	15
UC Santa Barbara	1	all edges	false	23	59	19	25	21
U of Minnesota	1	all edges	false	24	30	59	36	40
SUNY Stoneybrook	1	all edges	false	25.5		22	31	26
Ohio State U	1	all edges	false	25.5		42	37	38
Vanderbilt	1	all edges	false	27.5	66	82	82	80
U Illinois Urbana	1	all edges	false	29	48	36	34	62
U of Albany	1	all edges	false	30		51	49	57
Rutgers	1	all edges	false	31		15	22	25
Washington State U	1	all edges	false	32	33	43	40	61
U of Maryland	1	all edges	false	33	45	56	77	66
SUNY Binghamton	1	all edges	false	34		49	44	36
Cornell	1	all edges	false	35	19	62	16	19
CUNY	1	all edges	false	37		66	59	79
Brown	1	all edges	false	38	56	7	8	9
UMass Amherst	1	all edges	false	39	99	30	67	74
U of Iowa	1	all edges	false	40.5		73	48	30
USC	1	all edges	false	40.5	54	63	72	58
Michigan State U	1	all edges	false	42	62	14	18	23
U of Florida	1	all edges	false	43		79	70	70
Boston U	1	all edges	false	44	65	27	38	55
U Illinois Chicago	1	all edges	false	45		48	75	46
Notre Dame	1	all edges	false	46		46	74	65
U of Virginia	1	all edges	false	47.5	80	35	66	78
U of Georgia	1	all edges	false	47.5		34	51	28
UConn	1	all edges	false	49		31	47	39
U of San Francisco	1	all edges	false	50.5	9	64	39	64
UC Santa Cruz	1	all edges	false	53		47	73	52
Boston College	1	all edges	false	55		54	53	49
U of Oregon	1	all edges	false	56.5		33	43	60
Syracuse	1	all edges	false	58		60	52	72
Brandeis	1	all edges	false	60		24	26	20
Iowa State U	1	all edges	false	61.5		68	63	54
U Missouri Columbia	1	all edges	false	63		76	68	67
Louisiana State U	1	all edges	false	65		44	42	41
Loyola	1	all edges	false	68		29	56	35
Tulane	1	all edges	false	72		80	76	68
U of Tokyo	1	all edges	false		16	78	65	44
U of Amsterdam	1	all edges	false		89	50	50	56
U of Bristol	1	all edges	false		49	71	69	69
Caltech	1	all edges	false		4	38	46	63
Oxford	1	all edges	false		8	16	14	14
McGill	1	all edges	false		42	77	80	81
U of Vienna	1	all edges	false		72	61	81	82
U of Edinburgh	1	all edges	false		47	18	32	37
U of Zurich	1	all edges	false		46	58	60	76
Uppsala	1	all edges	false		88	45	54	47
U of Lund	1	all edges	false		76	72	61	42
U of Munich	1	all edges	false		63	25	79	77
U of Newcastle	1	all edges	false		97	13	64	51
Hong Kong U	1	all edges	false		60	26	58	71
Cambridge	1	all edges	false		6	20	17	18
Emory	1	all edges	false		93	70	57	48
Hebrew U Jerusalem	1	all edges	false		82	41	41	59

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
Chinese U Hong Kong	1	all edges	false		96	65	55	73
Australian National U	1	all edges	false		38	39	29	32
LSE	1	all edges	false		34	28	24	24
U College London	1	all edges	false		25	67	62	50
U of Queensland	1	all edges	false		91	37	71	53
MIT	1	all edges	false		7	17	27	31
U of Heidelberg	1	all edges	false		90	75	78	75
UChicago	1	all edges	true	1	20	3	2	2
Wisconsin	1	all edges	true	2	28	1	1	1
Berkeley	1	all edges	true	3	5	5	6	5
UMichigan	1	all edges	true	4	11	4	4	4
UCLA	1	all edges	true	5	12	6	5	6
UNC Chapel Hill	1	all edges	true	6	41	13	16	11
Harvard	1	all edges	true	7	1	2	3	3
Stanford	1	all edges	true	8	2	7	7	7
Northwestern	1	all edges	true	9	35	11	11	13
U of Washington	1	all edges	true	10	22	36	38	27
U of Pennsylvania	1	all edges	true	11	13	15	14	14
U Indiana Bloomington	1	all edges	true	12		24	17	19
Princeton	1	all edges	true	13	15	8	9	10
U of Arizona	1	all edges	true	14		9	10	8
Columbia	1	all edges	true	15	10	12	13	12
UT Austin	1	all edges	true	16	27	21	19	21
Johns Hopkins	1	all edges	true	17	24	28	33	28
Penn State	1	all edges	true	18	40	39	41	29
Yale	1	all edges	true	19	3	14	12	16
Duke	1	all edges	true	20	14	26	30	34
NYU	1	all edges	true	21	39	27	27	24
UCSD	1	all edges	true	22	23	16	23	17
UC Santa Barbara	1	all edges	true	23	59	18	24	20
U of Minnesota	1	all edges	true	24	30	38	34	43
Ohio State U	1	all edges	true	25.5		45	40	45
SUNY Stoneybrook	1	all edges	true	25.5		34	31	31
Vanderbilt	1	all edges	true	27.5	66	82	82	66
U Illinois Urbana	1	all edges	true	29	48	40	35	32
U of Albany	1	all edges	true	30		54	49	61
Rutgers	1	all edges	true	31		17	21	25
Washington State U	1	all edges	true	32	33	43	36	47
U of Maryland	1	all edges	true	33	45	75	80	79
SUNY Binghamton	1	all edges	true	34		46	48	36
Cornell	1	all edges	true	35	19	22	15	15
CUNY	1	all edges	true	37		64	59	75
Brown	1	all edges	true	38	56	10	8	9
UMass Amherst	1	all edges	true	39	99	60	61	63
USC	1	all edges	true	40.5	54	72	72	80
U of Iowa	1	all edges	true	40.5		57	55	33
Michigan State U	1	all edges	true	42	62	19	18	22
U of Florida	1	all edges	true	43		71	70	52
Boston U	1	all edges	true	44	65	35	37	42
U Illinois Chicago	1	all edges	true	45		74	73	38
Notre Dame	1	all edges	true	46		73	74	56
U of Virginia	1	all edges	true	47.5	80	62	63	53
U of Georgia	1	all edges	true	47.5		32	51	26
UConn	1	all edges	true	49		33	47	41
U of San Francisco	1	all edges	true	50.5	9	44	39	67
UC Santa Cruz	1	all edges	true	53		59	71	44
Boston College	1	all edges	true	55		50	54	77
U of Oregon	1	all edges	true	56.5		53	42	81
Syracuse	1	all edges	true	58		51	53	82
Brandeis	1	all edges	true	60		25	25	18
Iowa State U	1	all edges	true	61.5		68	66	62
U Missouri Columbia	1	all edges	true	63		69	68	57
Louisiana State U	1	all edges	true	65		48	44	39
Loyola	1	all edges	true	68		42	56	46
Tulane	1	all edges	true	72		81	81	55
Uppsala	1	all edges	true		88	49	46	74
Chinese U Hong Kong	1	all edges	true		96	47	52	54
U of Munich	1	all edges	true		63	76	76	73
U of Tokyo	1	all edges	true		16	65	67	70
Oxford	1	all edges	true		8	23	20	23
U of Zurich	1	all edges	true		46	63	60	78
U College London	1	all edges	true		25	66	64	64
U of Lund	1	all edges	true		76	58	62	50
Cambridge	1	all edges	true		6	20	22	35
Hong Kong U	1	all edges	true		60	56	57	71
Caltech	1	all edges	true		4	41	43	51
U of Heidelberg	1	all edges	true		90	79	77	60
U of Vienna	1	all edges	true		72	77	79	68
U of Amsterdam	1	all edges	true		89	52	50	65
U of Queensland	1	all edges	true		91	80	75	69
McGill	1	all edges	true		42	78	78	58
Hebrew U Jerusalem	1	all edges	true		82	55	45	76
Australian National U	1	all edges	true		38	37	29	49
U of Bristol	1	all edges	true		49	70	69	59
Emory	1	all edges	true		93	61	58	40
U of Newcastle	1	all edges	true		97	67	65	72
U of Edinburgh	1	all edges	true		47	29	32	48
MIT	1	all edges	true		7	30	26	37

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
LSE	1	all edges	true		34	31	28	30
UChicago	1	no non-tenure	false	1	20	2	3	3
Wisconsin	1	no non-tenure	false	2	28	4	1	1
Berkeley	1	no non-tenure	false	3	5	5	4	5
UMichigan	1	no non-tenure	false	4	11	3	6	4
UCLA	1	no non-tenure	false	5	12	6	5	6
UNC Chapel Hill	1	no non-tenure	false	6	41	17	17	15
Harvard	1	no non-tenure	false	7	1	1	2	2
Stanford	1	no non-tenure	false	8	2	7	7	7
Northwestern	1	no non-tenure	false	9	35	10	9	10
U of Washington	1	no non-tenure	false	10	22	48	27	43
U of Pennsylvania	1	no non-tenure	false	11	13	13	14	16
U Indiana Bloomington	1	no non-tenure	false	12		20	15	18
Princeton	1	no non-tenure	false	13	15	11	10	11
U of Arizona	1	no non-tenure	false	14		8	8	8
Columbia	1	no non-tenure	false	15	10	12	12	12
UT Austin	1	no non-tenure	false	16	27	19	18	20
Johns Hopkins	1	no non-tenure	false	17	24	70	45	33
Penn State	1	no non-tenure	false	18	40	23	32	25
Yale	1	no non-tenure	false	19	3	18	22	17
Duke	1	no non-tenure	false	20	14	26	41	37
NYU	1	no non-tenure	false	21	39	31	26	36
UCSD	1	no non-tenure	false	22	23	14	20	13
UC Santa Barbara	1	no non-tenure	false	23	59	22	19	26
U of Minnesota	1	no non-tenure	false	24	30	35	28	35
Ohio State U	1	no non-tenure	false	25.5		63	63	59
SUNY Stoneybrook	1	no non-tenure	false	25.5		21	38	21
Vanderbilt	1	no non-tenure	false	27.5	66	61	64	63
U Illinois Urbana	1	no non-tenure	false	29	48	25	25	30
U of Albany	1	no non-tenure	false	30		38	35	40
Rutgers	1	no non-tenure	false	31		27	21	29
Washington State U	1	no non-tenure	false	32	33	29	24	31
U of Maryland	1	no non-tenure	false	33	45	58	61	62
SUNY Binghamton	1	no non-tenure	false	34		47	36	42
Cornell	1	no non-tenure	false	35	19	15	13	14
CUNY	1	no non-tenure	false	37		64	74	81
Brown	1	no non-tenure	false	38	56	9	11	9
UMass Amherst	1	no non-tenure	false	39	99	39	43	45
U of Iowa	1	no non-tenure	false	40.5		36	52	24
USC	1	no non-tenure	false	40.5	54	49	57	39
Michigan State U	1	no non-tenure	false	42	62	16	16	23
U of Florida	1	no non-tenure	false	43		54	48	50
Boston U	1	no non-tenure	false	44	65	40	79	48
U Illinois Chicago	1	no non-tenure	false	45		60	58	44
Notre Dame	1	no non-tenure	false	46		56	69	72
U of Virginia	1	no non-tenure	false	47.5	80	68	70	69
U of Georgia	1	no non-tenure	false	47.5		24	34	19
UConn	1	no non-tenure	false	49		30	33	38
U of San Francisco	1	no non-tenure	false	50.5	9	67	76	78
UC Santa Cruz	1	no non-tenure	false	53		59	59	57
Boston College	1	no non-tenure	false	55		79	37	71
U of Oregon	1	no non-tenure	false	56.5		37	30	46
Syracuse	1	no non-tenure	false	58		50	46	53
Brandeis	1	no non-tenure	false	60		28	23	22
Iowa State U	1	no non-tenure	false	61.5		51	47	54
U Missouri Columbia	1	no non-tenure	false	63		42	49	51
Louisiana State U	1	no non-tenure	false	65		32	39	34
Loyola	1	no non-tenure	false	68		62	62	58
Tulane	1	no non-tenure	false	72		71	80	66
U of Tokyo	1	no non-tenure	false		16	80	75	79
Australian National U	1	no non-tenure	false		38	77	67	77
U of Amsterdam	1	no non-tenure	false		89	65	68	75
Caltech	1	no non-tenure	false		4	44	51	49
LSE	1	no non-tenure	false		34	33	31	32
Hong Kong U	1	no non-tenure	false		60	41	42	41
U of Edinburgh	1	no non-tenure	false		47	75	66	82
Hebrew U Jerusalem	1	no non-tenure	false		82	72	71	73
U of Lund	1	no non-tenure	false		76	82	65	74
Oxford	1	no non-tenure	false		8	76	60	61
U of Newcastle	1	no non-tenure	false		97	81	72	80
U of Queensland	1	no non-tenure	false		91	69	77	68
Chinese U Hong Kong	1	no non-tenure	false		96	45	50	55
U College London	1	no non-tenure	false		25	74	81	65
U of Heidelberg	1	no non-tenure	false		90	52	53	56
U of Zurich	1	no non-tenure	false		46	46	44	52
MIT	1	no non-tenure	false		7	34	29	27
McGill	1	no non-tenure	false		42	57	55	60
Cambridge	1	no non-tenure	false		6	78	73	70
Emory	1	no non-tenure	false		93	43	40	28
U of Bristol	1	no non-tenure	false		49	66	78	76
U of Munich	1	no non-tenure	false		63	53	54	47
Uppsala	1	no non-tenure	false		88	73	82	67
U of Vienna	1	no non-tenure	false		72	55	56	64
UChicago	1	no non-tenure	true	1	20	2	2	2
Wisconsin	1	no non-tenure	true	2	28	1	1	1
Berkeley	1	no non-tenure	true	3	5	5	4	5
UMichigan	1	no non-tenure	true	4	11	4	5	4
UCLA	1	no non-tenure	true	5	12	6	6	6

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
UNC Chapel Hill	1	no non-tenure	true	6	41	15	15	13
Harvard	1	no non-tenure	true	7	1	3	3	3
Stanford	1	no non-tenure	true	8	2	7	7	7
Northwestern	1	no non-tenure	true	9	35	11	10	12
U of Washington	1	no non-tenure	true	10	22	37	28	29
U of Pennsylvania	1	no non-tenure	true	11	13	13	14	14
U Indiana Bloomington	1	no non-tenure	true	12		19	16	20
Princeton	1	no non-tenure	true	13	15	10	8	10
U of Arizona	1	no non-tenure	true	14		8	9	8
Columbia	1	no non-tenure	true	15	10	12	11	11
UT Austin	1	no non-tenure	true	16	27	16	18	17
Johns Hopkins	1	no non-tenure	true	17	24	48	44	32
Penn State	1	no non-tenure	true	18	40	27	29	21
Yale	1	no non-tenure	true	19	3	23	22	18
Duke	1	no non-tenure	true	20	14	29	35	36
NYU	1	no non-tenure	true	21	39	34	26	25
UCSD	1	no non-tenure	true	22	23	14	21	19
UC Santa Barbara	1	no non-tenure	true	23	59	22	19	22
U of Minnesota	1	no non-tenure	true	24	30	30	27	39
Ohio State U	1	no non-tenure	true	25.5		63	61	63
SUNY Stoneybrook	1	no non-tenure	true	25.5		42	42	28
Vanderbilt	1	no non-tenure	true	27.5	66	64	62	43
U Illinois Urbana	1	no non-tenure	true	29	48	28	25	27
U of Albany	1	no non-tenure	true	30		35	36	57
Rutgers	1	no non-tenure	true	31		20	20	26
Washington State U	1	no non-tenure	true	32	33	26	24	37
U of Maryland	1	no non-tenure	true	33	45	58	59	54
SUNY Binghamton	1	no non-tenure	true	34		39	38	40
Cornell	1	no non-tenure	true	35	19	17	13	15
CUNY	1	no non-tenure	true	37		82	78	82
Brown	1	no non-tenure	true	38	56	9	12	9
UMass Amherst	1	no non-tenure	true	39	99	43	43	64
USC	1	no non-tenure	true	40.5	54	57	57	42
U of Iowa	1	no non-tenure	true	40.5		52	52	30
Michigan State U	1	no non-tenure	true	42	62	18	17	24
U of Florida	1	no non-tenure	true	43		49	50	44
Boston U	1	no non-tenure	true	44	65	36	37	56
U Illinois Chicago	1	no non-tenure	true	45		61	64	47
Notre Dame	1	no non-tenure	true	46		78	79	70
U of Virginia	1	no non-tenure	true	47.5	80	65	69	76
U of Georgia	1	no non-tenure	true	47.5		21	39	23
UConn	1	no non-tenure	true	49		24	34	38
U of San Francisco	1	no non-tenure	true	50.5	9	73	70	67
UC Santa Cruz	1	no non-tenure	true	53		59	58	55
Boston College	1	no non-tenure	true	55		68	68	69
U of Oregon	1	no non-tenure	true	56.5		33	30	49
Syracuse	1	no non-tenure	true	58		45	48	52
Brandeis	1	no non-tenure	true	60		25	23	16
Iowa State U	1	no non-tenure	true	61.5		50	49	51
U Missouri Columbia	1	no non-tenure	true	63		46	51	59
Louisiana State U	1	no non-tenure	true	65		38	33	34
Loyola	1	no non-tenure	true	68		62	63	60
Tulane	1	no non-tenure	true	72		76	73	71
Uppsala	1	no non-tenure	true		88	77	80	80
U of Newcastle	1	no non-tenure	true		97	80	76	75
Hebrew U Jerusalem	1	no non-tenure	true		82	66	75	66
LSE	1	no non-tenure	true		34	31	31	33
Hong Kong U	1	no non-tenure	true		60	41	40	61
Oxford	1	no non-tenure	true		8	60	60	41
U College London	1	no non-tenure	true		25	71	81	68
U of Heidelberg	1	no non-tenure	true		90	54	54	62
Emory	1	no non-tenure	true		93	40	41	31
U of Bristol	1	no non-tenure	true		49	74	71	81
U of Vienna	1	no non-tenure	true		72	56	55	58
Australian National U	1	no non-tenure	true		38	70	65	73
U of Zurich	1	no non-tenure	true		46	51	45	53
U of Amsterdam	1	no non-tenure	true		89	75	82	77
McGill	1	no non-tenure	true		42	55	56	46
MIT	1	no non-tenure	true		7	32	32	35
Cambridge	1	no non-tenure	true		6	67	67	65
U of Munich	1	no non-tenure	true		63	53	53	45
U of Tokyo	1	no non-tenure	true		16	72	77	74
Chinese U Hong Kong	1	no non-tenure	true		96	47	47	48
U of Lund	1	no non-tenure	true		76	81	66	79
U of Queensland	1	no non-tenure	true		91	79	72	78
Caltech	1	no non-tenure	true		4	44	46	50
U of Edinburgh	1	no non-tenure	true		47	69	74	72
UChicago	1	no student edges	false	1	20	5	5	5
Wisconsin	1	no student edges	false	2	28	2	1	1
Berkeley	1	no student edges	false	3	5	6	6	6
UMichigan	1	no student edges	false	4	11	4	4	4
UCLA	1	no student edges	false	5	12	3	2	2
UNC Chapel Hill	1	no student edges	false	6	41	23	28	28
Harvard	1	no student edges	false	7	1	1	3	3
Stanford	1	no student edges	false	8	2	8	8	10
Northwestern	1	no student edges	false	9	35	11	13	12
U of Washington	1	no student edges	false	10	22	66	56	68
U of Pennsylvania	1	no student edges	false	11	13	29	21	19

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
U Indiana Bloomington	1	no student edges	false	12		19	17	22
Princeton	1	no student edges	false	13	15	9	10	11
U of Arizona	1	no student edges	false	14		12	14	8
Columbia	1	no student edges	false	15	10	14	11	13
UT Austin	1	no student edges	false	16	27	21	16	32
Johns Hopkins	1	no student edges	false	17	24	24	40	33
Penn State	1	no student edges	false	18	40	60	66	62
Yale	1	no student edges	false	19	3	10	7	9
Duke	1	no student edges	false	20	14	28	22	30
NYU	1	no student edges	false	21	39	31	41	31
UCSD	1	no student edges	false	22	23	13	20	16
UC Santa Barbara	1	no student edges	false	23	59	20	18	17
U of Minnesota	1	no student edges	false	24	30	43	54	46
SUNY Stoneybrook	1	no student edges	false	25.5		33	29	34
Ohio State U	1	no student edges	false	25.5		44	47	36
Vanderbilt	1	no student edges	false	27.5	66	79	82	78
U Illinois Urbana	1	no student edges	false	29	48	41	49	61
U of Albany	1	no student edges	false	30		56	44	56
Rutgers	1	no student edges	false	31		17	19	20
Washington State U	1	no student edges	false	32	33	77	76	79
U of Maryland	1	no student edges	false	33	45	72	72	60
SUNY Binghamton	1	no student edges	false	34		47	32	35
Cornell	1	no student edges	false	35	19	25	23	21
CUNY	1	no student edges	false	37		68	64	71
Brown	1	no student edges	false	38	56	7	9	7
UMass Amherst	1	no student edges	false	39	99	51	62	66
U of Iowa	1	no student edges	false	40.5		38	55	24
USC	1	no student edges	false	40.5	54	62	67	53
Michigan State U	1	no student edges	false	42	62	18	25	18
U of Florida	1	no student edges	false	43		73	75	73
Boston U	1	no student edges	false	44	65	36	59	55
U Illinois Chicago	1	no student edges	false	45		48	58	43
Notre Dame	1	no student edges	false	46		59	61	58
U of Georgia	1	no student edges	false	47.5		32	35	25
U of Virginia	1	no student edges	false	47.5	80	69	70	70
UConn	1	no student edges	false	49		35	33	39
U of San Francisco	1	no student edges	false	50.5	9	40	30	57
UC Santa Cruz	1	no student edges	false	53		61	68	59
Boston College	1	no student edges	false	55		64	51	49
U of Oregon	1	no student edges	false	56.5		63	39	52
Syracuse	1	no student edges	false	58		76	81	75
Brandeis	1	no student edges	false	60		70	65	72
Iowa State U	1	no student edges	false	61.5		54	38	42
U Missouri Columbia	1	no student edges	false	63		75	78	76
Louisiana State U	1	no student edges	false	65		46	42	45
Loyola	1	no student edges	false	68		30	50	26
Tulane	1	no student edges	false	72		67	63	65
Chinese U Hong Kong	1	no student edges	false		96	50	60	64
U of Bristol	1	no student edges	false		49	65	53	69
U of Amsterdam	1	no student edges	false		89	57	43	54
U of Newcastle	1	no student edges	false		97	53	36	44
U of Lund	1	no student edges	false		76	37	45	50
Cambridge	1	no student edges	false		6	15	15	15
McGill	1	no student edges	false		42	82	74	74
Caltech	1	no student edges	false		4	42	52	63
U of Munich	1	no student edges	false		63	81	73	80
Hebrew U Jerusalem	1	no student edges	false		82	39	31	51
U of Edinburgh	1	no student edges	false		47	22	26	29
U of Heidelberg	1	no student edges	false		90	78	79	82
Hong Kong U	1	no student edges	false		60	49	69	67
U of Zurich	1	no student edges	false		46	74	80	81
U of Tokyo	1	no student edges	false		16	55	48	37
Australian National U	1	no student edges	false		38	34	24	27
U of Vienna	1	no student edges	false		72	80	77	77
LSE	1	no student edges	false		34	26	27	23
Uppsala	1	no student edges	false		88	45	34	38
MIT	1	no student edges	false		7	27	46	40
Oxford	1	no student edges	false		8	16	12	14
U of Queensland	1	no student edges	false		91	58	57	48
Emory	1	no student edges	false		93	71	71	47
U College London	1	no student edges	false		25	52	37	41
UChicago	1	no student edges	true	1	20	5	6	4
Wisconsin	1	no student edges	true	2	28	1	1	1
Berkeley	1	no student edges	true	3	5	6	5	6
UMichigan	1	no student edges	true	4	11	3	4	3
UCLA	1	no student edges	true	5	12	2	2	2
UNC Chapel Hill	1	no student edges	true	6	41	17	28	16
Harvard	1	no student edges	true	7	1	4	3	5
Stanford	1	no student edges	true	8	2	10	8	8
Northwestern	1	no student edges	true	9	35	11	12	12
U of Washington	1	no student edges	true	10	22	44	54	58
U of Pennsylvania	1	no student edges	true	11	13	27	20	27
U Indiana Bloomington	1	no student edges	true	12		24	21	18
Princeton	1	no student edges	true	13	15	7	10	10
U of Arizona	1	no student edges	true	14		12	13	7
Columbia	1	no student edges	true	15	10	15	11	13
UT Austin	1	no student edges	true	16	27	22	15	21
Johns Hopkins	1	no student edges	true	17	24	45	35	28

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
Penn State	1	no student edges	true	18	40	64	63	30
Yale	1	no student edges	true	19	3	9	9	11
Duke	1	no student edges	true	20	14	25	22	41
NYU	1	no student edges	true	21	39	47	39	25
UCSD	1	no student edges	true	22	23	21	18	14
UC Santa Barbara	1	no student edges	true	23	59	18	19	23
U of Minnesota	1	no student edges	true	24	30	55	47	65
SUNY Stoneybrook	1	no student edges	true	25.5		72	27	40
Ohio State U	1	no student edges	true	25.5		53	53	48
Vanderbilt	1	no student edges	true	27.5	66	77	77	75
U Illinois Urbana	1	no student edges	true	29	48	52	42	33
U of Albany	1	no student edges	true	30		39	40	62
Rutgers	1	no student edges	true	31		14	17	20
Washington State U	1	no student edges	true	32	33	75	73	81
U of Maryland	1	no student edges	true	33	45	71	72	56
SUNY Binghamton	1	no student edges	true	34		31	32	36
Cornell	1	no student edges	true	35	19	26	23	15
CUNY	1	no student edges	true	37		63	67	54
Brown	1	no student edges	true	38	56	8	7	9
UMass Amherst	1	no student edges	true	39	99	59	61	52
USC	1	no student edges	true	40.5	54	66	65	45
U of Iowa	1	no student edges	true	40.5		58	59	24
Michigan State U	1	no student edges	true	42	62	33	24	29
U of Florida	1	no student edges	true	43		81	74	79
Boston U	1	no student edges	true	44	65	51	57	43
U Illinois Chicago	1	no student edges	true	45		60	55	31
Notre Dame	1	no student edges	true	46		65	62	46
U of Virginia	1	no student edges	true	47.5	80	68	70	67
U of Georgia	1	no student edges	true	47.5		16	37	17
UConn	1	no student edges	true	49		19	33	35
U of San Francisco	1	no student edges	true	50.5	9	32	30	59
UC Santa Cruz	1	no student edges	true	53		67	64	47
Boston College	1	no student edges	true	55		41	52	49
U of Oregon	1	no student edges	true	56.5		42	36	66
Syracuse	1	no student edges	true	58		79	81	73
Brandeis	1	no student edges	true	60		62	68	61
Iowa State U	1	no student edges	true	61.5		37	44	53
U Missouri Columbia	1	no student edges	true	63		73	75	78
Louisiana State U	1	no student edges	true	65		54	50	32
Loyola	1	no student edges	true	68		49	46	38
Tulane	1	no student edges	true	72		61	66	60
U of Tokyo	1	no student edges	true		16	35	49	70
U of Bristol	1	no student edges	true		49	43	51	63
Oxford	1	no student edges	true		8	20	14	19
Australian National U	1	no student edges	true		38	29	26	42
U College London	1	no student edges	true		25	36	45	68
U of Heidelberg	1	no student edges	true		90	78	76	77
McGill	1	no student edges	true		42	76	78	80
Uppsala	1	no student edges	true		88	34	34	72
U of Zurich	1	no student edges	true		46	80	82	74
U of Vienna	1	no student edges	true		72	82	79	82
Caltech	1	no student edges	true		4	48	56	44
U of Newcastle	1	no student edges	true		97	38	43	71
Emory	1	no student edges	true		93	70	71	39
Chinese U Hong Kong	1	no student edges	true		96	57	58	55
Hebrew U Jerusalem	1	no student edges	true		82	30	31	50
Cambridge	1	no student edges	true		6	13	16	26
Hong Kong U	1	no student edges	true		60	69	69	69
U of Amsterdam	1	no student edges	true		89	40	41	64
U of Queensland	1	no student edges	true		91	56	60	57
U of Lund	1	no student edges	true		76	46	48	37
LSE	1	no student edges	true		34	23	29	22
U of Munich	1	no student edges	true		63	74	80	76
MIT	1	no student edges	true		7	50	38	51
U of Edinburgh	1	no student edges	true		47	28	25	34
UChicago	2	all edges	false	1	20	6	7	8
Wisconsin	2	all edges	false	2	28	10	11	9
Berkeley	2	all edges	false	3	5	1	5	1
UMichigan	2	all edges	false	4	11	13	17	15
UCLA	2	all edges	false	5	12	12	15	13
UNC Chapel Hill	2	all edges	false	6	41	23	18	18
Harvard	2	all edges	false	7	1	5	8	6
Stanford	2	all edges	false	8	2	7	9	10
Northwestern	2	all edges	false	9	35	8	6	5
U of Washington	2	all edges	false	10	22	22	24	28
U of Pennsylvania	2	all edges	false	11	13	3	3	3
Princeton	2	all edges	false	13	15	2	1	2
U of Arizona	2	all edges	false	14		38	22	25
Columbia	2	all edges	false	15	10	11	14	11
UT Austin	2	all edges	false	16	27	27	37	32
Johns Hopkins	2	all edges	false	17	24	14	10	12
Penn State	2	all edges	false	18	40	63	61	70
Yale	2	all edges	false	19	3	4	2	4
Duke	2	all edges	false	20	14	20	27	21
NYU	2	all edges	false	21	39	9	4	7
UCSD	2	all edges	false	22	23	17	12	16
UC Santa Barbara	2	all edges	false	23	59	28	34	34
U of Minnesota	2	all edges	false	24	30	39	36	33

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
SUNY Stoneybrook	2	all edges	false	25.5		45	51	50
Ohio State U	2	all edges	false	25.5		55	68	58
Vanderbilt U	2	all edges	false	27.5	66	53	58	64
UC Riverside	2	all edges	false	27.5		15	21	20
U Illinois Urbana	2	all edges	false	29	48	44	55	47
Rutgers	2	all edges	false	31		16	19	22
U of Maryland	2	all edges	false	33	45	50	57	56
SUNY Binghamton	2	all edges	false	34		57	52	42
Cornell	2	all edges	false	35	19	26	26	24
Florida State U	2	all edges	false	36		35	44	38
CUNY	2	all edges	false	37		31	28	36
Brown	2	all edges	false	38	56	40	53	52
UMass Amherst	2	all edges	false	39	99	54	60	60
USC	2	all edges	false	40.5	54	29	25	37
U of Iowa	2	all edges	false	40.5		47	47	48
U of Florida	2	all edges	false	43		66	65	66
Boston U	2	all edges	false	44	65	64	64	61
U Illinois Chicago	2	all edges	false	45		67	66	67
Notre Dame	2	all edges	false	46		70	63	71
U of Virginia	2	all edges	false	47.5	80	32	31	41
U of Georgia	2	all edges	false	47.5		51	50	46
U of San Francisco	2	all edges	false	50.5	9	62	62	68
UC Santa Cruz	2	all edges	false	53		58	43	49
U of Kentucky	2	all edges	false	54		65	67	57
Boston College	2	all edges	false	55		52	49	45
Syracuse	2	all edges	false	58		60	59	62
Brandeis	2	all edges	false	60		34	35	31
Temple U	2	all edges	false	61.5		36	33	27
U of New Hampshire	2	all edges	false	70		69	71	65
LSE	2	all edges	false		34	18	16	19
Emory	2	all edges	false		93	46	45	59
U of Toronto	2	all edges	false		18	42	39	43
Oxford	2	all edges	false		8	25	23	14
Ecole Polytechnique	2	all edges	false		43	59	56	54
cole Normale Suprieure	2	all edges	false		79	48	46	51
U Indiana Bloomington	2	all edges	false		12	30	29	26
McGill	2	all edges	false		42	61	48	35
U of Alberta	2	all edges	false		55	56	41	29
U of Lund	2	all edges	false		76	49	40	53
MIT	2	all edges	false		7	68	69	55
Cambridge	2	all edges	false		6	33	32	40
U of Edinburgh	2	all edges	false		47	24	30	30
U of Rochester	2	all edges	false		67	71	70	69
Hebrew U Jerusalem	2	all edges	false		82	19	13	17
U of Louvain	2	all edges	false		92	43	54	63
Hong Kong U of S&T	2	all edges	false		60	41	38	39
Australian National U	2	all edges	false		38	21	20	23
Chinese U Hong Kong	2	all edges	false		96	37	42	44
UChicago	2	all edges	true	1	20	6	7	7
Wisconsin	2	all edges	true	2	28	10	9	9
Berkeley	2	all edges	true	3	5	3	6	5
UMichigan	2	all edges	true	4	11	12	15	13
UCLA	2	all edges	true	5	12	13	13	14
UNC Chapel Hill	2	all edges	true	6	41	24	19	16
Harvard	2	all edges	true	7	1	8	8	8
Stanford	2	all edges	true	8	2	9	10	11
Northwestern	2	all edges	true	9	35	5	4	4
U of Washington	2	all edges	true	10	22	25	23	25
U of Pennsylvania	2	all edges	true	11	13	1	1	1
Princeton	2	all edges	true	13	15	2	2	2
U of Arizona	2	all edges	true	14		19	22	30
Columbia	2	all edges	true	15	10	11	12	10
UT Austin	2	all edges	true	16	27	26	36	27
Johns Hopkins	2	all edges	true	17	24	14	11	12
Penn State	2	all edges	true	18	40	61	62	54
Yale	2	all edges	true	19	3	4	3	3
Duke	2	all edges	true	20	14	22	28	15
NYU	2	all edges	true	21	39	7	5	6
UCSD	2	all edges	true	22	23	18	14	22
UC Santa Barbara	2	all edges	true	23	59	31	31	31
U of Minnesota	2	all edges	true	24	30	38	35	38
Ohio State U	2	all edges	true	25.5		66	67	61
SUNY Stoneybrook	2	all edges	true	25.5		48	46	35
UC Riverside	2	all edges	true	27.5		21	21	32
Vanderbilt U	2	all edges	true	27.5	66	56	58	50
U Illinois Urbana	2	all edges	true	29	48	55	56	65
Rutgers	2	all edges	true	31		15	17	17
U of Maryland	2	all edges	true	33	45	52	55	70
SUNY Binghamton	2	all edges	true	34		57	53	28
Cornell	2	all edges	true	35	19	23	24	19
Florida State U	2	all edges	true	36		41	44	40
CUNY	2	all edges	true	37		30	26	33
Brown	2	all edges	true	38	56	42	52	39
UMass Amherst	2	all edges	true	39	99	59	60	47
U of Iowa	2	all edges	true	40.5		51	45	44
USC	2	all edges	true	40.5	54	32	25	43
U of Florida	2	all edges	true	43		62	64	67

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
Boston U	2	all edges	true	44	65	64	66	63
U Illinois Chicago	2	all edges	true	45		63	65	51
Notre Dame	2	all edges	true	46		71	63	60
U of Virginia	2	all edges	true	47.5	80	35	33	62
U of Georgia	2	all edges	true	47.5		49	51	68
U of San Francisco	2	all edges	true	50.5	9	60	61	53
UC Santa Cruz	2	all edges	true	53		53	43	46
U of Kentucky	2	all edges	true	54		70	68	71
Boston College	2	all edges	true	55		50	50	59
Syracuse	2	all edges	true	58		58	59	58
Brandeis	2	all edges	true	60		33	37	29
Temple U	2	all edges	true	61.5		37	34	34
U of New Hampshire	2	all edges	true	70		68	71	69
Ecole Polytechnique	2	all edges	true		43	54	54	66
Chinese U Hong Kong	2	all edges	true		96	36	40	41
MIT	2	all edges	true		7	67	69	57
U of Alberta	2	all edges	true		55	43	42	36
U of Toronto	2	all edges	true		18	39	39	42
McGill	2	all edges	true		42	65	57	48
U of Edinburgh	2	all edges	true		47	28	29	26
Emory	2	all edges	true		93	44	48	56
cole Normale Suprieure	2	all edges	true		79	47	47	55
Australian National U	2	all edges	true		38	20	18	23
Oxford	2	all edges	true		8	29	27	20
Hong Kong U of S&T	2	all edges	true		60	40	38	45
U of Louvain	2	all edges	true		92	45	49	64
LSE	2	all edges	true		34	16	20	18
Cambridge	2	all edges	true		6	34	32	49
U Indiana Bloomington	2	all edges	true		12	27	30	24
U of Lund	2	all edges	true		76	46	41	37
Hebrew U Jerusalem	2	all edges	true		82	17	16	21
U of Rochester	2	all edges	true		67	69	70	52
UChicago	2	no non-tenure	false	1	20	5	10	8
Wisconsin	2	no non-tenure	false	2	28	9	6	9
Berkeley	2	no non-tenure	false	3	5	4	5	7
UMichigan	2	no non-tenure	false	4	11	11	8	12
UCLA	2	no non-tenure	false	5	12	14	14	13
UNC Chapel Hill	2	no non-tenure	false	6	41	24	16	14
Harvard	2	no non-tenure	false	7	1	6	11	10
Stanford	2	no non-tenure	false	8	2	12	15	16
Northwestern	2	no non-tenure	false	9	35	3	3	4
U of Washington	2	no non-tenure	false	10	22	23	24	24
U of Pennsylvania	2	no non-tenure	false	11	13	1	2	1
Princeton	2	no non-tenure	false	13	15	2	1	3
U of Arizona	2	no non-tenure	false	14		20	23	33
Columbia	2	no non-tenure	false	15	10	10	12	11
UT Austin	2	no non-tenure	false	16	27	19	21	27
Johns Hopkins	2	no non-tenure	false	17	24	13	9	6
Penn State	2	no non-tenure	false	18	40	39	43	48
Yale	2	no non-tenure	false	19	3	8	7	5
Duke	2	no non-tenure	false	20	14	22	19	21
NYU	2	no non-tenure	false	21	39	7	4	2
UCSD	2	no non-tenure	false	22	23	32	26	25
UC Santa Barbara	2	no non-tenure	false	23	59	21	20	19
U of Minnesota	2	no non-tenure	false	24	30	27	25	20
SUNY Stoneybrook	2	no non-tenure	false	25.5		48	47	49
Ohio State U	2	no non-tenure	false	25.5		49	44	51
UC Riverside	2	no non-tenure	false	27.5		18	18	18
Vanderbilt U	2	no non-tenure	false	27.5	66	35	35	39
U Illinois Urbana	2	no non-tenure	false	29	48	36	36	30
Rutgers	2	no non-tenure	false	31		16	22	22
U of Maryland	2	no non-tenure	false	33	45	42	37	43
SUNY Binghamton	2	no non-tenure	false	34		46	38	29
Cornell	2	no non-tenure	false	35	19	15	17	17
Florida State U	2	no non-tenure	false	36		33	41	45
CUNY	2	no non-tenure	false	37		17	13	15
Brown	2	no non-tenure	false	38	56	26	33	36
UMass Amherst	2	no non-tenure	false	39	99	38	45	52
U of Iowa	2	no non-tenure	false	40.5		29	28	23
USC	2	no non-tenure	false	40.5	54	52	49	38
U of Florida	2	no non-tenure	false	43		41	40	50
Boston U	2	no non-tenure	false	44	65	54	54	54
U Illinois Chicago	2	no non-tenure	false	45		53	55	46
Notre Dame	2	no non-tenure	false	46		70	58	62
U of Virginia	2	no non-tenure	false	47.5	80	37	34	34
U of Georgia	2	no non-tenure	false	47.5		40	42	42
U of San Francisco	2	no non-tenure	false	50.5	9	68	68	63
UC Santa Cruz	2	no non-tenure	false	53		64	59	69
U of Kentucky	2	no non-tenure	false	54		56	56	47
Boston College	2	no non-tenure	false	55		60	70	66
Syracuse	2	no non-tenure	false	58		43	39	40
Brandeis	2	no non-tenure	false	60		25	32	26
Temple U	2	no non-tenure	false	61.5		57	57	65
U of New Hampshire	2	no non-tenure	false	70		51	51	56
U of Louvain	2	no non-tenure	false		92	50	50	53
Hebrew U Jerusalem	2	no non-tenure	false		82	28	31	31
U of Rochester	2	no non-tenure	false		67	61	69	60
McGill	2	no non-tenure	false		42	66	71	70

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
Australian National U	2	no non-tenure	false		38	69	64	59
Oxford	2	no non-tenure	false		8	44	48	35
U of Alberta	2	no non-tenure	false		55	34	30	32
U of Lund	2	no non-tenure	false		76	55	53	55
Emory	2	no non-tenure	false		93	59	67	67
cole Normale Suprieure	2	no non-tenure	false		79	71	61	64
Chinese U Hong Kong	2	no non-tenure	false		96	63	60	58
U Indiana Bloomington	2	no non-tenure	false		12	30	29	37
LSE	2	no non-tenure	false		34	45	46	44
U of Toronto	2	no non-tenure	false		18	31	27	28
Cambridge	2	no non-tenure	false		6	62	62	61
Ecole Polytechnique	2	no non-tenure	false		43	58	65	57
Hong Kong U of S&T	2	no non-tenure	false		60	65	66	68
MIT	2	no non-tenure	false		7	67	63	71
U of Edinburgh	2	no non-tenure	false		47	47	52	41
UChicago	2	no non-tenure	true	1	20	4	8	6
Wisconsin	2	no non-tenure	true	2	28	9	7	9
Berkeley	2	no non-tenure	true	3	5	5	4	7
UMichigan	2	no non-tenure	true	4	11	11	9	12
UCLA	2	no non-tenure	true	5	12	29	14	13
UNC Chapel Hill	2	no non-tenure	true	6	41	22	16	16
Harvard	2	no non-tenure	true	7	1	8	10	8
Stanford	2	no non-tenure	true	8	2	13	13	14
Northwestern	2	no non-tenure	true	9	35	2	2	2
U of Washington	2	no non-tenure	true	10	22	25	22	25
U of Pennsylvania	2	no non-tenure	true	11	13	1	1	1
Princeton	2	no non-tenure	true	13	15	3	3	4
U of Arizona	2	no non-tenure	true	14		20	24	24
Columbia	2	no non-tenure	true	15	10	10	12	11
UT Austin	2	no non-tenure	true	16	27	17	23	20
Johns Hopkins	2	no non-tenure	true	17	24	12	11	10
Penn State	2	no non-tenure	true	18	40	38	42	49
Yale	2	no non-tenure	true	19	3	7	6	5
Duke	2	no non-tenure	true	20	14	19	20	18
NYU	2	no non-tenure	true	21	39	6	5	3
UCSD	2	no non-tenure	true	22	23	57	27	30
UC Santa Barbara	2	no non-tenure	true	23	59	21	19	21
U of Minnesota	2	no non-tenure	true	24	30	30	25	28
Ohio State U	2	no non-tenure	true	25.5		41	45	52
SUNY Stoneybrook	2	no non-tenure	true	25.5		49	47	57
Vanderbilt U	2	no non-tenure	true	27.5	66	33	35	38
UC Riverside	2	no non-tenure	true	27.5		16	17	23
U Illinois Urbana	2	no non-tenure	true	29	48	39	37	45
Rutgers	2	no non-tenure	true	31		15	21	17
U of Maryland	2	no non-tenure	true	33	45	35	36	37
SUNY Binghamton	2	no non-tenure	true	34		47	40	27
Cornell	2	no non-tenure	true	35	19	14	18	15
Florida State U	2	no non-tenure	true	36		40	41	51
CUNY	2	no non-tenure	true	37		18	15	19
Brown	2	no non-tenure	true	38	56	27	32	31
UMass Amherst	2	no non-tenure	true	39	99	37	43	40
U of Iowa	2	no non-tenure	true	40.5		31	29	36
USC	2	no non-tenure	true	40.5	54	51	50	39
U of Florida	2	no non-tenure	true	43		36	39	47
Boston U	2	no non-tenure	true	44	65	53	53	56
U Illinois Chicago	2	no non-tenure	true	45		50	55	44
Notre Dame	2	no non-tenure	true	46		66	71	60
U of Georgia	2	no non-tenure	true	47.5		42	44	54
U of Virginia	2	no non-tenure	true	47.5	80	32	33	42
U of San Francisco	2	no non-tenure	true	50.5	9	65	66	58
UC Santa Cruz	2	no non-tenure	true	53		58	64	68
U of Kentucky	2	no non-tenure	true	54		55	56	48
Boston College	2	no non-tenure	true	55		67	61	65
Syracuse	2	no non-tenure	true	58		44	38	43
Brandeis	2	no non-tenure	true	60		23	34	22
Temple U	2	no non-tenure	true	61.5		56	57	53
U of New Hampshire	2	no non-tenure	true	70		45	48	50
Australian National U	2	no non-tenure	true		38	71	70	69
Hong Kong U of S&T	2	no non-tenure	true		60	61	59	63
U of Edinburgh	2	no non-tenure	true		47	52	52	34
Oxford	2	no non-tenure	true		8	43	51	32
U of Toronto	2	no non-tenure	true		18	28	26	33
cole Normale Suprieure	2	no non-tenure	true		79	62	62	62
Chinese U Hong Kong	2	no non-tenure	true		96	63	63	67
U of Lund	2	no non-tenure	true		76	54	54	46
U of Rochester	2	no non-tenure	true		67	59	68	64
Cambridge	2	no non-tenure	true		6	64	60	59
Hebrew U Jerusalem	2	no non-tenure	true		82	24	31	35
MIT	2	no non-tenure	true		7	60	69	61
Emory	2	no non-tenure	true		93	70	58	70
LSE	2	no non-tenure	true		34	48	46	41
U Indiana Bloomington	2	no non-tenure	true		12	26	28	26
U of Louvain	2	no non-tenure	true		92	46	49	55
U of Alberta	2	no non-tenure	true		55	34	30	29
McGill	2	no non-tenure	true		42	68	67	71
Ecole Polytechnique	2	no non-tenure	true		43	69	65	66
UChicago	2	no student edges	false	1	20	11	12	15
Wisconsin	2	no student edges	false	2	28	7	8	9

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
Berkeley	2	no student edges	false	3	5	4	4	5
UMichigan	2	no student edges	false	4	11	16	17	22
UCLA	2	no student edges	false	5	12	12	15	14
UNC Chapel Hill	2	no student edges	false	6	41	19	19	18
Harvard	2	no student edges	false	7	1	6	11	11
Stanford	2	no student edges	false	8	2	5	7	7
Northwestern	2	no student edges	false	9	35	9	6	6
U of Washington	2	no student edges	false	10	22	24	23	30
U of Pennsylvania	2	no student edges	false	11	13	3	2	1
Princeton	2	no student edges	false	13	15	1	3	4
U of Arizona	2	no student edges	false	14		20	18	23
Columbia	2	no student edges	false	15	10	10	14	13
UT Austin	2	no student edges	false	16	27	41	46	40
Johns Hopkins	2	no student edges	false	17	24	14	10	8
Penn State	2	no student edges	false	18	40	66	67	67
Yale	2	no student edges	false	19	3	2	5	3
Duke	2	no student edges	false	20	14	22	28	24
NYU	2	no student edges	false	21	39	8	1	2
UCSD	2	no student edges	false	22	23	15	13	12
UC Santa Barbara	2	no student edges	false	23	59	37	39	55
U of Minnesota	2	no student edges	false	24	30	38	34	27
SUNY Stoneybrook	2	no student edges	false	25.5		56	49	58
Ohio State U	2	no student edges	false	25.5		46	58	60
Vanderbilt U	2	no student edges	false	27.5	66	51	56	61
UC Riverside	2	no student edges	false	27.5		13	22	19
U Illinois Urbana	2	no student edges	false	29	48	40	45	41
Rutgers	2	no student edges	false	31		25	29	29
U of Maryland	2	no student edges	false	33	45	47	50	54
SUNY Binghamton	2	no student edges	false	34		61	60	47
Cornell	2	no student edges	false	35	19	28	33	28
Florida State U	2	no student edges	false	36		54	57	52
CUNY	2	no student edges	false	37		29	25	32
Brown	2	no student edges	false	38	56	44	43	50
UMass Amherst	2	no student edges	false	39	99	58	51	56
USC	2	no student edges	false	40.5	54	27	24	33
U of Iowa	2	no student edges	false	40.5		48	42	46
U of Florida	2	no student edges	false	43		67	66	63
Boston U	2	no student edges	false	44	65	71	71	69
U Illinois Chicago	2	no student edges	false	45		62	62	62
Notre Dame	2	no student edges	false	46		68	68	68
U of Virginia	2	no student edges	false	47.5	80	31	30	34
U of Georgia	2	no student edges	false	47.5		53	52	37
U of San Francisco	2	no student edges	false	50.5	9	65	63	66
UC Santa Cruz	2	no student edges	false	53		55	37	45
U of Kentucky	2	no student edges	false	54		59	59	57
Boston College	2	no student edges	false	55		52	53	38
Syracuse	2	no student edges	false	58		60	65	65
Brandeis	2	no student edges	false	60		39	38	44
Temple U	2	no student edges	false	61.5		34	27	20
U of New Hampshire	2	no student edges	false	70		70	70	71
Ecole Polytechnique	2	no student edges	false		43	50	48	48
Oxford	2	no student edges	false		8	23	21	17
MIT	2	no student edges	false		7	64	61	53
McGill	2	no student edges	false		42	45	40	26
U Indiana Bloomington	2	no student edges	false		12	33	32	25
U of Louvain	2	no student edges	false		92	69	69	70
LSE	2	no student edges	false		34	17	16	16
U of Rochester	2	no student edges	false		67	63	64	64
Cambridge	2	no student edges	false		6	30	31	36
Emory	2	no student edges	false		93	43	44	59
U of Edinburgh	2	no student edges	false		47	26	26	39
U of Alberta	2	no student edges	false		55	49	54	43
Hebrew U Jerusalem	2	no student edges	false		82	18	9	10
Chinese U Hong Kong	2	no student edges	false		96	32	47	42
Hong Kong U of S&T	2	no student edges	false		60	36	36	31
U of Lund	2	no student edges	false		76	57	55	51
Australian National U	2	no student edges	false		38	21	20	21
U of Toronto	2	no student edges	false		18	35	35	35
cole Normale Suprieure	2	no student edges	false		79	42	41	49
UChicago	2	no student edges	true	1	20	67	9	12
Wisconsin	2	no student edges	true	2	28	9	8	10
Berkeley	2	no student edges	true	3	5	4	6	8
UMichigan	2	no student edges	true	4	11	13	14	13
UCLA	2	no student edges	true	5	12	12	16	14
UNC Chapel Hill	2	no student edges	true	6	41	21	21	17
Harvard	2	no student edges	true	7	1	8	11	7
Stanford	2	no student edges	true	8	2	7	7	6
Northwestern	2	no student edges	true	9	35	5	3	3
U of Washington	2	no student edges	true	10	22	22	20	21
U of Pennsylvania	2	no student edges	true	11	13	1	1	1
Princeton	2	no student edges	true	13	15	3	4	5
U of Arizona	2	no student edges	true	14		20	17	27
Columbia	2	no student edges	true	15	10	10	13	11
UT Austin	2	no student edges	true	16	27	47	49	55
Johns Hopkins	2	no student edges	true	17	24	14	10	9
Penn State	2	no student edges	true	18	40	66	66	68
Yale	2	no student edges	true	19	3	2	5	2
Duke	2	no student edges	true	20	14	24	29	22

institution	sample	edge inclusion	bi-partite	domestic prestige	foreign prestige	eigen rank	closeness rank	degree rank
NYU	2	no student edges	true	21	39	6	2	4
UCSD	2	no student edges	true	22	23	15	15	16
UC Santa Barbara	2	no student edges	true	23	59	53	37	42
U of Minnesota	2	no student edges	true	24	30	38	34	36
Ohio State U	2	no student edges	true	25.5		55	57	66
SUNY Stoneybrook	2	no student edges	true	25.5		42	42	31
UC Riverside	2	no student edges	true	27.5		36	22	25
Vanderbilt U	2	no student edges	true	27.5	66	44	55	43
U Illinois Urbana	2	no student edges	true	29	48	46	50	57
Rutgers	2	no student edges	true	31		19	27	20
U of Maryland	2	no student edges	true	33	45	39	47	47
SUNY Binghamton	2	no student edges	true	34		63	64	33
Cornell	2	no student edges	true	35	19	23	30	24
Florida State U	2	no student edges	true	36		50	58	54
CUNY	2	no student edges	true	37		27	24	23
Brown	2	no student edges	true	38	56	43	43	37
UMass Amherst	2	no student edges	true	39	99	57	54	67
USC	2	no student edges	true	40.5	54	28	25	35
U of Iowa	2	no student edges	true	40.5		49	44	32
U of Florida	2	no student edges	true	43		65	65	60
Boston U	2	no student edges	true	44	65	69	70	70
U Illinois Chicago	2	no student edges	true	45		60	61	64
Notre Dame	2	no student edges	true	46		68	68	44
U of Georgia	2	no student edges	true	47.5		51	53	50
U of Virginia	2	no student edges	true	47.5	80	30	32	48
U of San Francisco	2	no student edges	true	50.5	9	62	62	46
UC Santa Cruz	2	no student edges	true	53		11	38	39
U of Kentucky	2	no student edges	true	54		59	59	65
Boston College	2	no student edges	true	55		52	52	62
Syracuse	2	no student edges	true	58		61	67	61
Brandeis	2	no student edges	true	60		37	39	34
Temple U	2	no student edges	true	61.5		34	28	38
U of New Hampshire	2	no student edges	true	70		70	69	71
Chinese U Hong Kong	2	no student edges	true		96	31	40	40
cole Normale Suprieure	2	no student edges	true		79	41	41	51
McGill	2	no student edges	true		42	54	46	45
U of Toronto	2	no student edges	true		18	33	35	30
Australian National U	2	no student edges	true		38	18	19	19
MIT	2	no student edges	true		7	64	63	53
U Indiana Bloomington	2	no student edges	true		12	32	33	29
U of Edinburgh	2	no student edges	true		47	26	26	28
Oxford	2	no student edges	true		8	25	23	26
LSE	2	no student edges	true		34	16	18	15
U of Rochester	2	no student edges	true		67	56	60	49
U of Alberta	2	no student edges	true		55	48	51	58
U of Louvain	2	no student edges	true		92	71	71	69
Emory	2	no student edges	true		93	45	48	56
U of Lund	2	no student edges	true		76	58	56	59
Hong Kong U of S&T	2	no student edges	true		60	35	36	41
Cambridge	2	no student edges	true		6	29	31	63
Ecole Polytechnique	2	no student edges	true		43	40	45	52
Hebrew U Jerusalem	2	no student edges	true		82	17	12	18

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