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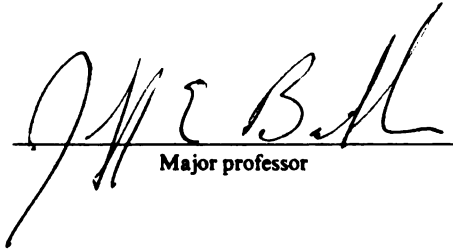
**TWO ESSAYS ON THE CHALLENGES FACING WOMEN AND MINORITIES
IN THE LABOR MARKET**

presented by

JENNIFER ANNE TRACEY

has been accepted towards fulfillment
of the requirements for

Ph.D degree in ECONOMICS


Major professor

Date August 9, 2001

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TWO ESS

TWO ESSAYS ON THE CHALLENGES FACING WOMEN AND MINORITIES IN
THE LABOR MARKET

By

Jennifer Anne Tracey

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

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Department of Economics

2001

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ABSTRACT

TWO ESSAYS ON THE CHALLENGES FACING WOMEN AND MINORITIES IN THE LABOR MARKET

By

Jennifer Anne Tracey

This dissertation contributes to the literature on women and minorities in the labor market by investigating two unique challenges facing these groups of workers that have been overlooked in the past.

The first chapter investigates the relationship between fertility and job search behavior, uncovering a potential reason behind the poorer labor market outcomes for women with young children. Specifically, it looks at how the presence of young children at home affects the amount of time an individual devotes to job search. Although the effect of fertility on job search intensity is analyzed for both men and women, the findings show that children represent a significant constraint on search intensity for women, while the results for men are somewhat mixed.

These are significant findings given the current political attention to child care as a national concern and the importance of search behavior in determining labor market outcomes, such as wages, unemployment duration and the probability of remaining in the labor force. This is true regardless of whether the negative relationship between children and search intensity is primarily due to the cost of child care while searching, or to the expectation of a lower effective wage once work is found due to child care costs while working.

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The second chapter takes a new look at the problem of racial and ethnic job segregation, investigating both its effects and causes. Even after controlling for differences in personal human capital and job characteristics, the results confirm that racial and ethnic job segregation is an important contributor to the lower wages paid to blacks and Hispanics than to similar whites. This study also explores the potential impact of racial and ethnic segregation on the likelihood of receiving various employment benefits, yet finds job segregation to play a smaller role in explaining differences between minorities and whites in the number of employment benefits received than it does in explaining wage differentials. Finally, the potential causes of racial and ethnic job segregation are explored. The results show that while minorities who reside in more segregated neighborhoods are significantly more likely to work in segregated jobs, those who commute longer distances to work are less likely to work in a segregated job, two findings consistent with Kain's (1968) "spatial mismatch hypothesis." The findings also indicate that blacks and Hispanics who work in larger firms are less likely to be in segregated jobs, and that English fluency and citizenship status are strongly associated with the likelihood of job segregation for Hispanics.

For my parents.

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ACKNOWLEDGMENTS

This work could not have been completed without the help of many people.

Harry Holzer has been a tremendous asset. While working for him as a graduate assistant, I learned more about conducting research than I could have in any class. His work inspired both my research style and many of the ideas in this dissertation. As an advisor, he shared both his insight and incredible knowledge of the literature related to this study. I greatly appreciated and benefited from the quality of his feedback on my work. I was also very honored and grateful that he continued to work with me through the years, even after his career took him to the Department of Labor and then on to Georgetown.

My other committee members also helped greatly, especially Jeff Biddle who encouraged me to look at my work more critically. He made a considerable contribution to both the empirical and theory sections of the second chapter, forcing me to think about what the results really meant. In addition, his teaching skills were a tremendous asset both in the classroom and throughout the process of conducting this research. I am also grateful to Steve Woodbury for graciously agreeing to help out late in the process and for redirecting some of the methodology and theory I was unclear about. Finally, David Neumark deserves a word of thanks for his help during the early stages of this work.

I definitely could not have made it through this program without my family. My husband Paul cheered me on even when my work seemed at odds with our personal goals, and my son Max, who arrived during all the craziness, helped me keep things in perspective. Perhaps the most instrumental in encouraging me to finish were my parents.

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I was often inspired by my mother's determination and my father's strong belief in the value of higher education.

I would also like to sincerely thank everyone at Christensen Associates, especially Carl Degen and Dianne Christensen, for generously providing me with office space and computer support under the guise of part-time employment. I can honestly say that this work would never have been completed without their help.

Finally, I am thankful to many of my fellow graduate students. In particular, Dan, Kathleen, Leslie, Jess, and my husband Paul have served as sounding boards for both ideas and the obligatory graduate-student griping. I also thank them for their moral support and sharing their thoughts on earlier drafts of this work.

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Introduction

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Chapter 1

THE EFFECT OF CHILDREN ON JOB SEARCH INTENSITY

Introduction

The labor market status of women with young children has received much attention in the literature over the past several years. Economists have studied the effects of fertility and child care costs on outcomes such as labor force participation, earnings and poverty.¹ Yet surprisingly, studies to date have largely ignored the impact of children on an important determinant of such labor market outcomes – job search activity. The purpose of this paper is to investigate the relationship between fertility and job search behavior. Specifically, I look at how the presence of young children at home may affect the amount of time an individual devotes to job search. Children are expected to impact search intensity, in part, by imposing additional search costs on parents, such as child care expenses while looking for work.

Several factors make this topic both interesting and important. First, women have entered the U.S. labor market in unprecedented numbers over the past few decades. This is especially true of married women with young children. Between 1970 and 1990, labor force participation rates nearly doubled for married women with children under age six, from 30 percent to 59 percent; by 1997, their participation rate had risen to 64 percent (U.S. Bureau of the Census, 1998). Finding adequate and affordable child care arrangements while searching may be a significant barrier to finding employment for these new labor market entrants. The fact that unemployment rates are significantly

¹ See, for example, Ribar, 1992; Klerman and Leibowitz, 1994.

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Second, market child care is a significant expense. The estimated annual cost of care for preschool children is over \$4,000 (Casper, 1995). Such child care expenses are especially burdensome for low-income families, even though federal programs like the 1988 Family Support Act and the enactment of federal child care legislation in October 1990 reduced many of the potential income-related differences in access to child care. Poor working families earning less than \$15,000 annually spend about 25 percent of their income on child care, while families with annual incomes of \$54,000 or more spend only about 6 percent (Casper, 1995). Consequently, child care costs are likely to be a greater barrier to job search, and self-sufficiency, for low-income workers.

An investigation of the relationship between children and job search is particularly important, and timely, given the current political climate. As welfare reform efforts continue to emphasize the transition from welfare to work, child care has become an important national policy issue. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 included a \$3.5 billion increase in funding for child care subsidies to low-income families over six years (U.S. Dept. of Health and Human Services, 1996). However, provisions of this legislation do not provide direct child care subsidies or services to welfare recipients engaged in job search.³ Rather, each

² The most drastic difference in unemployment is among single women, whose overall rate of unemployment in 1997 was 8.8 percent, compared to a rate of 18.8 percent for single women with children under six. The unemployment rate of all married women in 1996 was 3.2 percent, compared to 4.4 percent for married women with children under six (U.S. Bureau of the Census, 1998). Obviously, these differences in unemployment rates may also be influenced by differences in human capital investment, financial resources and other personal characteristics.

³ The Family and Medical Leave Act (FMLA) of 1993 sought to alleviate the likelihood of job loss resulting from interrupted labor force participation due to childbirth by allowing a new parent the right to return to their previous employer after maternity or paternity leave of up to 12 weeks. This legislation could

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state must “develop personal responsibility plans for recipients identifying the education, training, and job placement services needed to move into the workforce” (U.S. Department of Health and Human Services, 1996).

Finally, but perhaps most importantly, labor market outcomes such as wages, benefits and unemployment duration are likely to suffer if search effort is restricted. With fewer hours spent searching, less information is gathered about labor market opportunities and fewer job offers are likely to be received. For example, Keeley and Robins (1985) find that lower search intensity results in a higher duration of unemployment. It is also likely that search effort will impact the duration of employment by affecting the quality of employer-employee matches. Job retention is an important issue in the current welfare reform debate. While policy makers may be less concerned with previous estimates of lower employment and earnings for women with children if they result from personal time-allocation and human-capital investment decisions, there is likely to be greater concern if they arise due to restricted job search activity.

Numerous studies have shown that labor market outcomes for women with young children fall short of outcomes for women without children. While part of these differences results from the fact that on average women with children invest less in market human capital, due in part to interrupted labor force participation, restricted job search activities may also play an important role. For instance, Waldfogel (1997) finds a negative effect of children on women’s wages, even after controlling for such human capital variables as age, experience and education. For men, in contrast, she finds that children have either a positive effect on wages or no effect at all.

potentially reduce the importance of this research. However, its coverage is far from universal. Klerman and Leibowitz (1994) estimate that the federal FMLA covers only about a third of all working mothers.

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Figure 1 illustrates the empirical relationships that have been found between **children** and labor market outcomes for women, along with the proposed role that search **effort** may play.

Because women remain the primary caregivers in our society, it may be **reasonable** to expect that children will affect their search behavior to a greater degree **than** men's job search. Yet child care concerns may potentially impact the search **behavior** of men as well as women. Not only is joint decision making often necessary **given** the increased prevalence of two-income households, but more and more men are **becoming** involved in the rearing of their children. For instance, fathers were the primary caregivers of 18.5 percent of preschool children in families with working mothers during 1994 (Casper, 1997). This study looks at the effect of fertility on the job search intensity of **both** men and women. Perhaps not surprisingly, while the findings show that children represent a significant constraint on search intensity for women, the results for men are **somewhat** mixed.

In the remainder of this paper, I provide what I believe to be the first serious investigation of the relationship between children and search effort. I begin by reviewing the **existing** bodies of literature on job search and child care issues. The following **section** outlines a conceptual model of job search, which suggests the appropriate **statistical** framework later used in the empirical analysis. Next, the data are discussed, **descriptive** information child care issues and search effort is introduced, and the **empirical** results are presented. Finally, the paper concludes with policy implications and **directions** for areas of further research.

Moreover, it does not help new labor market entrants.

Existing Literature

There is some evidence that female labor supply has increased since the 1960s, but it has not flourished from the same sources as in the past. Search behavior has changed. Most of the search is now done by women, and a dichotomous view of the labor market is no longer valid. Sample by sex through the 1960s and 1970s shows that women are likely to be looking for work differently than men. They are more likely to be looking for work than men, and they are more likely to be looking for work than men.

Much of the research on unemployment in the 1960s and 1970s was based on search effort for work and money devoted to search. Factors affecting search effort and the probability of finding work are important. Authors run pooled data sets, and consequently many studies have found that women are more likely to be looking for work than men. Robins (1985) estimates that the probability of finding work is higher for women than for men.

¹ See Hammermesh and Robinson (1985) for a discussion of the importance of insurance benefits in the labor market.

Existing Literature

There is surprisingly little overlap in the bodies of literature on job search and female labor supply, especially on the empirical side. Past research on job search, which flourished from the late-1970s to the mid-1980s, pays scant attention to the differences in search behavior between men and women and the potential reasons for those differences. Most of the search literature either focuses strictly on men or simply controls for sex with a dichotomous variable. This study includes both men and women, but stratifies the sample by sex throughout the empirical work. Since it is likely that women behave differently than men when it comes to job search, separate behavioral functions are preferred to pooled regression analysis. Due to a weaker attachment to the labor market, women are likely to have fewer business contacts, to possess weaker networking skills, and to be looking for different types of employment.

Much of the existing literature on job search intensity investigates the role of unemployment insurance benefits.⁴ Barron and Mellow (1979) develop a theory of search effort for unemployed individuals, in which effort involves the choice of both time and money devoted to search. Their empirical work, which focuses exclusively on factors affecting the time decision, finds a negative impact of unemployment benefits (and the probability of being recalled from layoff) on hours devoted to job search. The authors run pooled regressions with no controls for sex, fertility or child care costs, and consequently much of the variation in search intensity is left unexplained.⁵ Keeley and Robins (1985) estimate separate search intensity equations for married men, women and

⁴ See Hamermesh and Rees (pg. 223, 1993) for a detailed discussion of how the receipt of unemployment insurance benefits is theorized to impact search intensity.

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youths, but they do control for the impact of fertility. Like Barron and Mellow, the authors find that unemployment benefits lead to a significant decrease in weekly hours of search for married men, however their results for women and youths are insignificant.

Particularly relevant to the findings of this study, Barron and Mellow (1981) look at factors that affect the labor force transition probabilities of an unemployed individual. A major focus of the research is on the impact that an unemployed individual's search intensity has on the probability of becoming employed, dropping out of the labor force, or remaining unemployed. Highlighting the importance of search intensity, the results show that the more time an unemployed individual devotes to job search, the greater the probability of becoming employed and the less likely they are to drop out of the labor force. The authors control for sex using a dichotomous variable, and find unemployed women less likely to become employed and more likely to drop out of the labor force than men, even after controlling for differences in search intensity.

A more recent paper by Blau and Robins (1990) focuses on differences in search behavior between employed and unemployed individuals. The authors look at four components of the job search process: the choice of search methods, the choice of how many firms to contact, the rate at which offers are received, and the acceptance or rejection of an offer. They do not look at the choice of hours devoted to search. Their results show that employed individuals use fewer methods and generate fewer contacts, but receive more offers than unemployed searchers.⁶ Differences are also found between men and women, with women found to use fewer methods and make fewer contacts per week. However, since neither Barron and Mellow (1981) nor Blau and Robins (1990)

⁵ The author's reported R-squared values range from .057 to .083.

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control for fertility in their empirical work, these differences should not be attributed **solely** to sex. One should expect differences in measures of search behavior between the **sexes** if women's search behavior is constrained by child care concerns to a greater extent **than** men's search.

The body of research on female labor supply and child care issues is large and **growing**. Most of the studies to date focus on the labor force participation decision (but **not** on the mechanism through which child care costs may affect the probability of **becoming** employed). The major finding of this research is that child care costs may be a **significant** barrier to labor force participation among women (see, for example, Connelly, 1992; Ribar, 1992; Kimmel, 1996). The estimated child care cost elasticities of labor force participation vary widely among the various studies, however, with estimates ranging from as low as 0 to as high as -0.9.

These cross-sectional studies attempt to measure the direct effect of child care costs on labor force participation.⁷ But the estimates may be biased since they likely pick up the indirect effect of restricted search effort on remaining in the labor force. To illustrate this potential estimation problem, consider a two-period model in which an individual must decide to participate in the labor force. Such an individual initially enters the labor force as unemployed and engages in job search. In the next period, she enters one of three states – she becomes employed, remains unemployed or drops out of the labor force. Child care concerns may impact the probability of transition between these states by affecting a parent's search behavior. If, for example, children lower search

⁶ Holzer (1987), using a different data set and focusing on the behavior of youth only, found a higher offer rate for the unemployed than for the employed.

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intensity by increasing search costs, then an unemployed parent will be more likely to drop out of the labor force and be less likely to become employed (see the result cited above from Barron and Mellow, 1981). Thus, a significant fraction of women in a cross section who are classified as non-participants may be women who dropped out of the labor force because child care restricted their search behavior, not because it directly altered their labor force participation decision.⁸ That is, observed labor force participation may understate true participation in the same manner as official unemployment statistics may understate true unemployment due to the “discouraged worker syndrome.”

Overall, the existing literature has generated some important findings. Research on job search behavior has found that search intensity is a significant factor in determining search outcomes, such as the probability of employment. The literature on female labor supply and child care illustrates that the cost of market care is likely to be a significant barrier to labor force participation for women, although estimates of the magnitude vary considerably. Nevertheless, virtually no study to date has seriously investigated the impact of children on job search behavior. This is a significant shortcoming because policy makers are not only interested in whether women (especially welfare recipients) are participating in the labor force, but also in how society might assist with the process of finding and maintaining jobs. This paper attempts to bridge the gap between these two bodies of research. By investigating the impact of children on job

⁷ Child care costs directly affect the labor force participation decision by imposing fixed costs on work and by lowering the potential net wage (the more a parent works, the more money she must expend on child care). See Hamermesh and Reese (1993) for more details.

⁸ One may also argue that women with children in a cross section are more likely to be viewed as in the labor force (as unemployed) because lower search intensity should lengthen unemployment duration. However, studies investigating the impact of child care costs on labor force participation (LFP) usually use

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search activity, I hope to enrich our understanding of the role that child care constraints play in the employment process for women and men.

Economic Framework: Implications from a Simplified Model of Job Search

Job search is a costly process that involves large investments in time and money, but is necessary due to imperfect information in the labor market. Individuals looking for employment engage in search to acquire information about alternative job opportunities. In turn, these investments in job search are expected to affect both the length of unemployment and the ultimate wage obtained.

Several search models exist that characterize the optimal strategy of an unemployed worker by the choice of a reservation wage and search intensity.⁹ In the basic job search model, the job possibilities of an individual worker are characterized as a distribution of possible wage offers, with search efforts serving to generate information about these alternative wage offers. This information, denoted by θ , is characterized as representing the probability that a wage offer is sampled from the wage-offer distribution in a given period. The production function for θ can be depicted as

$$(1) \quad \theta = \theta(\tau, \beta),$$

where τ represents the fraction of time per period an individual searches, and the term β is a shift parameter to capture factors changing the likelihood of a wage offer for a given search intensity, such as the individual's search productivity.¹⁰ The standard assumption

employment status as a proxy of LFP status because child care costs are often observed for employed women only.

⁹ See, for example, Mortensen (1977) and Barron and Mellow (1979).

¹⁰ Barron and Mellow (1979) also model the effects of monetary expenditures on θ . However, since time is often considered the most significant search cost, other monetary expenditures on search (such as the cost of transportation, stamps, newspapers, etc.) are ignored in this discussion.

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is that $\theta_1 > 0$, indicating that the more time per period an individual searches, the greater the likelihood of generating a wage offer (Barron and Mellow, 1981).

It is well established that the optimal decision rule for sampling from a distribution of wage offers is characterized by the choice of a reservation wage. The worker accepts any wage obtained that meets or exceeds the reservation wage, and rejects all others. The decision rule that determines the optimal search time, τ , is derived from a constrained maximization problem in which the unemployed worker maximizes expected discounted utility from income and leisure, subject to budget and time constraints.¹¹ The results of the maximization problem show that the individual should expand time spent searching until the marginal cost of search time equals the marginal expected gain of search.

Therefore, search intensity, or the fraction of time an individual devotes to search, is modeled as a function of the costs and benefits of job search. Given the focus of this paper, I model the time spent searching for employment as

$$(2) \quad S = f(\text{CHILD}, X)$$

where S is search intensity measured by hours searched per week,¹² CHILD is a vector of fertility measures, and X is a vector of variables reflecting other costs and benefits of search. Variables expected to influence search contained in X include factors that affect the mean of the wage offer distribution; variables to measure changes in β , the shift parameter affecting the productivity of search; along with other control variables (these factors are discussed in more detail below).

¹¹ See Barron and Mellow (1979) for a formal presentation of the model.

¹² Obviously, one could obtain the exact form of τ , the fraction of time per period an individual searches, by dividing S by the constant 168 hours per week.

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While it would be surprising if the presence of children had no impact on job search behavior, economic theory alone does not allow us to predict the sign of the effect of fertility on search effort, *a priori*. On the one hand, children are likely to impose unique search costs on parents in the form of child care expenses while searching. Child care costs will reduce a searcher's means to finance the transportation and other monetary costs of looking for work, especially for those with very little savings, producing an income effect. Child care costs may also produce a substitution effect for those with other sources of income. Assuming an hourly fee per child for day care services, each additional child in a household will reduce the searcher's net income, raising the relative cost of looking for work (although perhaps not by the same amount per child). The income and substitution effects work in the same direction, both increasing the marginal cost of search, so that fertility is expected to reduce time spent searching.¹³

Likewise, time allocation theory (Becker, 1965) suggests that fertility will reduce search intensity. Because children are thought to increase the value of an individual's time spent at home, this view implies that the presence of children will increase the marginal cost of search by increasing the opportunity cost of searching.¹⁴ Therefore, searchers with children are predicted to spend less time searching than searchers without children, *ceteris paribus*.

¹³ Arguably, some parents have the less expensive option of using a spouse or adult family member to supervise their young children while searching, and can utilize "free" child care services for 6-10 year olds while they are in school. Yet, as labor force participation among women has increased and the existence of extended families has diminished, there has been a growing trend toward the use of formal child care arrangements. What is desired to test the above hypothesis empirically is a measure of child care costs while searching. Because such data are not available, measures of fertility may be thought of as proxies for child care costs in the following analyses.

¹⁴ The time allocation theory also predicts that this effect should be stronger for women than for men if women are more productive at rearing children (Killingsworth and Heckman, 1986). However, I expect this effect to be relatively small. The empirical work that follows includes only current searchers – those who have already decided to participate in the labor force.

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On the other hand, the financial responsibility associated with children may provide an additional incentive to search more intensely for work. Individuals with children must provide extra consumption (for their children) so that the value of a job rises. Here it may help to think of parents as being altruistic, such that their utility function depends positively on the well-being of their children.¹⁵ By increasing search intensity, individuals will shorten expected unemployment duration and therefore have more resources to spend on their children. Consequently, this effect suggests that children will increase the marginal benefit of search and therefore increase search intensity, all else equal.

For women, one might expect the first two effects described above to outweigh the third effect, and the presence of children to be associated with lower search intensity. Women are still more likely to take on the responsibility of child rearing. Men, on the other hand, have traditionally been responsible for the financial well-being of the family. Although this allocation of responsibility has changed a great deal over the past few decades, on average men with children may feel more pressure to support their family and thus search more intensely than men without children. In other words, the third effect may be strong enough to outweigh the first two effects for men.

The Data

The data used in this study are from the Multi-City Study of Urban Inequality (MCSUI). The MCSUI survey was administered to adult household residents in four metropolitan areas: Atlanta, Boston, Detroit and Los Angeles. Interviewing was

¹⁵ The degree of altruism should determine the discount rate on consumption (Becker, 1993), implying the more altruistic parents are, the more they may attempt to concentrate their lifetime earnings during the

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completed in the summer of 1992 in Detroit, one year later in Atlanta, and in the summer of 1994 in Boston and Los Angeles.¹⁶

The survey consisted of a probability sample of households, stratified by race-ethnicity and poverty-status composition of the 1990 Census. Blacks were oversampled to yield roughly equal numbers of whites and blacks in all locations; Latinos and Asians were similarly oversampled in Los Angeles, as were Asians in Boston. In addition, concentrated poverty areas were oversampled in all metropolitan areas. The project also used a multistage sampling procedure, utilizing cluster sampling with three levels of clustering. This process generated a total of 8,916 observations – 1,528 in Atlanta, 1,820 in Boston, 1,543 in Detroit, and 4,025 in Los Angeles. Restricting the sample to non-retired respondents reduces the full sample to 7,570 observations – 1,283 in Atlanta, 1,630 in Boston, 1,182 in Detroit, and 3,475 in Los Angeles.¹⁷

In the following analyses, measures are taken to ensure that the data can be used to draw inferences regarding the underlying population. First, analysis weights for respondents are used, which were set inversely proportional to the household sampling weight. Analysis weights also reflect nonresponse (if nonresponse is not uniformly distributed) and the number of persons eligible for interview in the respective household.

period in which their children are most financially dependant.

¹⁶ The U.S. economy was recovering from the recession of the early 1990s when the survey was administered. Monthly unemployment rates during this period averaged under six percent in Atlanta and Boston, approximately eight percent in Detroit and under 10 percent in Los Angeles. To control for differences in local labor market conditions, dummy variables for metropolitan area are included in the regression analyses that follow.

¹⁷ Unfortunately, the sample of current searchers is significantly smaller, as discussed below. (Please also note that three observations were dropped due to missing sex information.)

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Second, in all analyses robust standard errors are calculated that are also adjusted for the clustering and stratification of the survey design.¹⁸

One goal of the Multi-City Study of Urban Inequality was to test hypotheses concerning the status of women and minorities in urban labor markets. The data thus provide a rich source of information on labor market histories, including an extensive collection of variables related to job search behavior. In particular, MCSUI provides a direct measure of search intensity – time spent searching for work. The data set also contains detailed information on family background and child care issues.

The primary shortcoming of these data concerns a lack of uniformity between the surveys used in the four cities. For example, in Detroit the question regarding search intensity was phrased differently and pertained to a shorter time period than the search intensity question asked in Atlanta, Boston and Los Angeles. The sample of those asked the question in Detroit was also different than in the other three cities. Specifically, only respondents who had looked for work within the past 30 days in Detroit were asked “How many hours per week have you spent looking for work?” In Atlanta, Boston and Los Angeles, all respondents who had searched any time within the last year were asked “In total, about how many hours did you spend looking for work in (the last thirty days/the last month) of your job search?”

As a consequence of these differences, the search intensity variable for Atlanta, Boston and Los Angeles may be plagued with two sources of measurement error.¹⁹ First,

¹⁸ This was accomplished using Stata survey (svy) commands (see StataCorp, 1997, pp. 305-312). It was necessary to calculate a robust variance estimator because an examination of the model's residuals plotted against the fitted values indicated the potential for heteroskedasticity, a violation of the least-squares assumptions.

¹⁹ If the measurement error is just mean-zero “white noise,” it can be absorbed in the disturbance of the regression and ignored. Although such classical measurement error in the dependent variable does not bias the coefficient estimates, it will lead to less precise estimates since the errors will inflate the standard error

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because respondents were asked to recall events that occurred as much as twelve months in the past (and the question pertains to a longer time period), recall bias is likely to be a significant issue. Unless the period of job search was very recent, the exact number of hours searched is likely to be forgotten – the more time that has elapsed since the respondent last looked for work the greater the likelihood of forgetting. Past researchers using unemployment data have found recall errors to be a significant problem even over an interval as short as one month (Horvath, 1982).

Second, the potential for measurement error is exacerbated by the fact that respondents in Atlanta, Boston and Los Angeles are asked to report hours searched during the last 30 days of their job search, as opposed to hours searched per week. This is likely because the interpretation of the search intensity question may vary among respondents whose search duration was less than 30 days. To illustrate, say two individuals were both unemployed for only two weeks, during which time they both searched 20 hours. When asked “In total, about how many hours did you spend looking for work in (the last thirty days/the last month) of your job search?” the first respondent may simply report 20 hours. The second respondent, however, realizing that he probably would have searched more hours had he been unemployed for the full 30 days, may extrapolate and report 40 hours.²⁰

To mitigate the potential for recall bias, the sample of searchers in Atlanta, Boston and Los Angeles was restricted to searchers who had looked for work within the

of the regression (see for example Greene, 1990, p. 295, and Angrist and Krueger, 1998, pp. 70-71). However, in this case, the measurement error is probably not “white noise” since it is likely to depend on the level of search intensity, since search intensity and search duration are presumably related, although the exact relationship is likely to be quite complicated (See Barron and Mellow, 1979, p. 398).

²⁰ A comparison of average search intensity by search duration, along with the results of a Kolmogorov-Smirnov test (see StataCorp, 1997, pp. 301-303), revealed evidence of these measurement-error problems in the data.

last 30 days in the analyses that follow (this also made the samples more uniform across all four cities). To address the second potential source of measurement error, misinterpretation of the search intensity variable among those who had not searched a full month, the sample was further restricted to those whose search duration was at least 30 days. These two restrictions reduced the sample of searchers in Atlanta, Boston and Los Angeles from 1613 to 729.²¹

This presents another potential shortcoming of the data: the size of the sample. Even before making the adjustments discussed above, the sample of current job searchers was fairly modest. But when we focus our attention on those who have looked for work within the past 30 days, impose the search-duration restriction discussed above, and are forced to analyze the Detroit data separately from the other cities due to inconsistencies in the survey instruments,²² the potential for small sample size issues intensifies. The consequences of small sample size can be quite serious. In general, precision of estimation is reduced. Estimates may have large errors, such that investigators will sometimes be led to falsely accept null hypotheses of no relationship between two variables. Estimates will also be very sensitive to sample data, such that a single observation can sometimes produce drastic shifts in the sample mean. Unfortunately, there are no easy remedies for small sample size problems, short of collecting a larger sample from the same population. Since this is not a possibility, in order to partially mitigate problems associated with the size of the sample, a few overly influential

²¹ The latter restriction may lead to sample selection bias if individuals with search duration exceeding 30 days are different from searchers in general. Note, however, that there were no observations in the Detroit data for which search duration was less than one month, so this restriction also makes the two samples more uniform.

²² A Kolmogorov-Smirnov test (see StataCorp, 1997, pp. 301-303) determined that there are still significant differences in the distribution of the search intensity variable between Detroit and the other cities, even after the two sample restrictions were imposed.

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observations (for which the model did not fit) were diagnosed, and outliers were subsequently omitted from the sample as discussed in Appendix A.²³

To avoid reducing the size of the sample any further, however, employed searchers are pooled with nonemployed searchers in the search intensity regressions that follow, and employment status is included as an explanatory variable. This approach, which was also used by Blau and Robins (1990), assumes that employment status is exogenous to the choice of search intensity. Some may question the validity of this assumption, since the employment status of a job searcher could be related to the perceived productivity of searching while employed versus unemployed. As Blau and Robins (1990) point out, employment status while searching should be treated as an endogenous variable if there is heterogeneity in unobserved components of search productivity. However, in this study a simultaneous system of equations was not used and additional stratification of the sample by employment status was not entertained due to the paucity of observations.

Despite these shortcomings, the fact that MCSUI is a relatively new data set that provides a rich source of information on job search behavior and child care issues makes it a useful starting point. Moreover, the search intensity results that follow seem quite robust to specification in spite of the size of the sample.

Descriptive Information on Child Care Concerns and Search Effort

An examination of summary statistics disaggregated by gender provides considerable evidence that women's search effort is more likely to be affected by the

²³ This was accomplished using the fit command in Stata and DFITS, Cook's Distance and Welsch Distance diagnostic tests (see StataCorp, 1997, pp. 372-397).

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presence of children at home than men's search effort. Table 1 provides descriptive information on perceived child care constraints in Atlanta, Boston and Los Angeles. Respondents with children under eighteen were asked whether child care concerns had affected several aspects of their labor market experience in the last year.²⁴ The first four rows of Table 1 illustrate that women are much more likely than men to feel that child care concerns have constrained their ability to gain and maintain employment. For example, almost one-third of women with children under eighteen reported that child concerns caused them to "not look or apply for work" over the past year, compared to only 8 percent of men. Moreover, almost 20 percent of women reported that they could not participate in school or training programs due to child care concerns. Only 7 percent of men, by contrast, felt this way.

The last four rows of Table 1 indicate that, of those parents who had worked sometime within the past year, women were significantly more likely than men to report that child care concerns caused them to be late or absent from work, to change their hours of work, or to lose out on a promotion or raise. Overall, however, Table 1 shows that a surprisingly large percentage of men felt that child care concerns had affected several aspects of their own employment over the past year.

Table 2 contains summary statistics generated from similar, but less-pointed questions that were asked in the Detroit survey. Respondents in Detroit with children under eighteen were asked: "Has the cost, availability or quality of child care ever influenced your employment or that of your (spouse/partner) in any way?" Thirty-one percent of women answered yes to this question, compared to 25 percent of men. It

²⁴ These questions, which are listed in the first column of Table 1, are admittedly subjective and may be subject to a significant amount of reporting bias. For this reason, the term "constraint" should be

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seems reasonable to believe that the majority of Detroit men who answered yes may have been referring to constraints on the employment of their spouse or partner, especially when one compares the results from Table 2 with Table 1. Moreover, the results presented in rows two and three of Table 2 reveal that women were significantly more likely than men to mention that the cost and quality of child care were areas of concern.

As a follow-up question, respondents in Detroit who felt that child care had impacted their (or their spouse's/partner's) employment were asked "In what ways did these issues influence your (or your spouse's/partner's) employment?" The most common responses to this secondary question included mention of constraints on when and the amount of time they could work and constraints on the ability to enter the labor force or maintain a job, with men more likely than women to report the former (32 percent v. 25 percent) and women more likely to report the latter (43 percent v. 35 percent). As illustrated in the fifth row of Table 2, roughly 16 percent of women and men felt that child care concerns restricted their ability (or the ability of their spouse or partner) to gain employment or to choose a certain type of employment.

Table 3 reports summary statistics on search intensity by sex and the presence of young children. In Detroit, there is a dramatic difference in time spent searching between those with and those without children. The means presented in panel A of Table 3 indicate that parents of young children in Detroit, whether they are mothers or fathers, search significantly fewer hours per week than those without young children. This result is particularly strong for women; on average women with children under six search roughly four hours per week, while those without any children ten or under search almost

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eleven hours per week.²⁵ Perhaps surprisingly, those with children aged 6-10 reported fewer hours searched per week on average than parents of children under six, although the finding is much stronger for men than for women. This relationship also holds true for men in Atlanta, Boston and Los Angeles, as illustrated in Panel B of Table 3, but not for women in these cities.²⁶

A potential explanation for this seemingly peculiar finding may be that fathers are more likely to become involved in the rearing of children who are slightly older. Evidence that men take on more of the child care responsibilities of children aged 6-10, and that this responsibility may affect their career, can be seen in panel B of Table 4, which presents the same information contained in Table 1, but stratified by child age groupings. The results speak for themselves. In virtually every case fathers with children aged 6-10 are more likely than fathers of children under six to feel that child care concerns have affected their employment in the past year (although in some cases the differences are not statistically significant). For example, 15 percent of men with children 6-10 reported that child care concerns caused them to not look or apply for work in the past 12 months, compared to only 7 percent of men with preschool children. Comparatively, panel A of Table 4 illustrates that women with preschool aged children were slightly more likely to report that child care concerns have affected their work life than women with 6-10 year olds.

²⁵ The relatively low hours of search per week has been noted by other authors (Keeley and Robins, 1985; Barron and Mellow, 1979). The results in this sample are slightly lower than in prior studies because I include employed searchers, who search less intensely on average.

²⁶ It is interesting to note that Connelley (1991) finds that cost per hour of care is higher for school-aged children (although average weekly expenditures are less).

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Econometric Specification and Empirical Results

Having documented the prevalence of perceived child care constraints, along with significant differences in mean search time by gender and fertility status, I now turn to the central question of this paper: Whether children effect the search intensity of men and women.

Overall, Tables 1-4 provide some initial evidence that children, even slightly older children who are in school for a portion of the day, are likely to impact job search behavior. To test this hypothesis further, I estimate separate equations for men and women of the form:

$$(3) \quad S_i = \alpha + \beta CHILD_i + \delta PWAGE_i + \gamma PTIME_i + \lambda X_i + \epsilon_i,$$

where S_i represents hours searched per week, $CHILD_i$ is a vector of fertility measures, $PWAGE_i$ is a predicted wage, $PTIME_i$ is an indicator of part-time employment, X_i is a vector of individual and household control variables, and ϵ_i is an error term which includes unobserved characteristics. Each of the independent variables is discussed in more detail below.

The explanatory variables contained in X_i include education categorical variables (high school diploma or GED; Associates degree, vocational or trade school certificate; Bachelor's degree; Graduate degree; with high school dropout the omitted category), employment status and a measure of nonwage income. Nonwage income is expected to have a negative impact on search intensity, since higher nonwage income reduces the gain to locating a wage offer through search.²⁷ Employed searchers are predicted to

²⁷ In the model presented by Barron and Mellow (1979), nonwage income leads to an unambiguous reduction in search time. This is due to the fact that they assume time and money inputs are combined to produce θ additively (i.e., the level of market expenditures does not affect the marginal productivity of search time and vice versa). The model presented by Tannery (1983) assumes a more general production

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search fewer hours per week than unemployed searchers, both due to time constraints, which imply a higher marginal cost of search, and the lower expected benefits of search.²⁸ Since employed searchers are likely to react differently to nonwage income than unemployed searchers, X_i also includes an interaction term between employment status and nonwage income.

The education variables are included to measure changes in β , the shift parameter affecting the productivity of search. A higher education level may indicate higher search productivity, although this does not necessarily imply one would predict a positive coefficient on the included education categories. In other words, because an individual with a higher level of education may be more efficient at searching for employment, they may not need to spend as much time searching as a high-school dropout does.²⁹

Two proxy variables were used to reflect changes in the mean of the wage-offer distribution, m . First, $PWAGE_i$ is a predicted wage based on the individual's age, race, education, sex and metropolitan area, which should be directly related to m . Because an increase in one's mean earnings potential represents an increase in the benefits of search, the theory suggests a positive coefficient on $PWAGE_i$. A second variable used to measure the wage-offer distribution is $PTIME_i$, a dummy variable for individuals likely to be searching for part-time work. Part-time work is typically associated with lower wages and benefits, all else equal, and therefore is expected to lower the benefits of search and have a negative coefficient. Unfortunately, respondents were not asked explicitly if they

function for θ so that time and money can be complementary inputs. Under this assumption, an increase in nonwage income will raise the productivity of time spent searching, and thus increase search intensity. Therefore, the overall impact of nonwage income may be ambiguous.

²⁸ By definition, employed searchers are only looking to *improve* their compensation or non-monetary aspects of their job.

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were searching for part time work. $PTIME_i$ is only an indicator of whether the searcher's current or past job was part time.³⁰

Vector $CHILD_i$ includes two binary variables, the first indicating the presence of children aged 0-5 and the second indicating the presence of children aged 6-10. As discussed above, economic theory does not allow us to predict the impact of the fertility measures contained in $CHILD_i$ on search intensity, although the information provided on child care concerns suggests we are likely to find a negative coefficient on the fertility indicators, at least for women. The information provided above also indicates that slightly older children, those aged 6-10, seemed to have a bigger impact on employment issues for men than preschool-aged children.

Because children are expected to influence search behavior in part due to the costs they impose on parents in the form of child care expenses while searching, vector X_i also contains family structure variables, meant to serve as proxies for the availability of unpaid care. Access to child care is a function of family structure, or the number and characteristics of other potential providers available to care for children, such as older children or an elderly parent.³¹ I include two measures of non-parental adults in the household – the number of young adults aged 18-34 and the number of older adults aged 35-64 – to control for the availability of unpaid care. Unfortunately, information on potential care providers outside the home is not observed.

²⁹ Barron and Mellow (1979) include a variable measuring years of education, as opposed to the categorical variables used here, and predict and find a positive correlation between years of education and hours searched per week.

³⁰ See Appendix A for an exact definition of this variable.

³¹ Using data from the 1985 Survey of Income and Program Participation (SIPP), Connelly (1992) finds that the presence of a teenager, an adult woman other than the mother, or an adult male other than the father, lowers the probability of a family paying for child care.

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For the Atlanta-Boston-Los Angeles sample, X_i also includes binary indicators of whether the searcher received Unemployment Insurance (UI) benefits and/or welfare payments (at the time of the surveys, welfare payments were administrated under the Aid to Families with Dependant Children program, or AFDC). These variables were not included in the Detroit model because survey questions in Detroit only asked if *anyone* in the household received welfare payments (the survey did not ask if the respondent herself received AFDC, nor did it ask about UI benefits for either the respondent or the household). Empirically, most studies have found UI benefits to have a negative effect on search intensity. However, one cannot predict, *a priori*, the impact of UI on weekly hours searched. Although UI benefits provide a means of support that reduces the incentives to look for work, they may also provide unemployed workers the means to finance the transportation and other monetary costs of looking for work. Moreover, one of the conditions to receive benefits is an active search for work, as most states require beneficiaries to register with their state employment service and demonstrate that they have contacted employers (see Hamermesh and Rees, 1993, for a more detailed discussion). The dummy indicator for AFDC receipt is likely have a negative impact on search intensity, since such nonwage income reduces the incentives to search for work. However many states also required AFDC recipients to register with state employment agencies and demonstrate that they are actively engaged in job search, so the expected sign of the AFDC coefficient is also ambiguous.

Table 5 presents means of the analysis variables for the final sample of searchers, stratified by sex and metropolitan area (Detroit versus Atlanta, Boston and Los Angeles). The final sample consists of 201 observations on Detroit households and 727

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observations on households in Atlanta, Boston and Los Angeles. Appendix A contains a more detailed description of how the analysis variables discussed above were created, along with a discussion of the omission of outlier values. The method used to construct *PWAGE*, the proxy variable used to reflect changes in the mean of the wage offer distribution, deserves additional attention due to the potential for sample selection bias, and thus is presented separately in Appendix B.

Empirical Results

Tables 6-7 report OLS regression estimates of the search intensity equation (3) for the two samples. The results seem to support the theory fairly well; moreover, they provide almost indisputable evidence that young children have a significant impact on job search behavior. As shown in columns (1) and (2) of Table 6, regression analysis using the Detroit sample indicates that children aged 0-5 and children 6-10 have a significant negative effect on hours searched per week for women. Also as predicted, women who are employed while searching spend significantly fewer hours looking, and nonwage income has a significant negative effect on search intensity. It is interesting to note that the presence of an older adult in the household may have a mitigating effect. Women with an older adult present search more intensely, perhaps because older adults may serve as in-house child care providers or simply take over normal household duties, freeing up time for women to search. As shown in column (2), *PWAGE* also enters with the expected sign, although its coefficient is insignificant. The only variable that does not seem to support the theory presented is *PTIME*, the indicator of part time work, which was predicted to have a negative impact on search intensity due to the lower benefits associated with part time jobs. However, the coefficient is insignificant, and the variable

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is not necessarily a true indication of whether the individual is seeking part time work, only that they were previously employed part time.³²

For men in Detroit, the results are equally striking. While slightly older children aged 6-10 have a significant negative impact on men's search intensity, very young children 0-5 actually are estimated to have positive effect on weekly hours searched. This seems to support what we saw in Tables 3 and 4: that slightly older children are perceived to represent a greater constraint on the search behavior of men than very young children do. Again, this may be due to the fact that fathers take on more of the child rearing responsibilities once their children get older. It is also possible that men with very young children feel a stronger sense of financial responsibility, since they are probably not yet acclimated to the idea of "an extra mouth to feed."

Employed men are estimated to search significantly fewer hours than unemployed men, and nonwage income has a negative effect on search intensity (although the latter effect loses its significance when the predicted wage variable is added). Among men who are employed, however, nonwage income is estimated to have a significant positive impact on hours searched. The interaction term may be picking up individual heterogeneity, indicating that employed searchers who have accumulated more nonwage income are more ambitious on average, and thus have a higher propensity to search. The coefficient on *PTIME* also enters with the correct sign and is significant at the .01 level. However, since this variable is not an exact measure of whether the individual is currently searching for a part time job, but only an indicator of whether one's current or

³² The education dummies should be thought of as merely controls for β , since several of these categories contain very few individuals. For example, among current searchers in Detroit, there were only four women and four men with a graduate degree. There is also the possibility of multicollinearity with the predicted wage variable.

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most recent job was part time, we may again be measuring some type of individual heterogeneity. Men who have worked or are working part time (particularly since part time work is much less likely among men) may simply have a lower propensity to search, as opposed to the dummy variable only measuring the lower expected returns associated with searching for part time work.

Table 7 presents results for the sample of searchers in Atlanta, Boston and Los Angeles. Potentially due to the way the survey question regarding search intensity was phrased in these cities, the data do not seem to fit the model as well as it did for the Detroit sample, despite the steps taken to address the potential measurement error problems discussed above. Nevertheless, the results support the theory that young children have a significant impact on job search behavior.

For women, the presence of very young children aged 0-5 has a significant negative impact on search intensity. In the specification presented in column (1), the only other variable estimated to have a substantial impact on search for women is employment status, again showing that those who are employed search significantly fewer hours. Nonwage income and the part time work dummy also enter with the predicted sign, yet they are insignificant. When the predicted wage is included, however, the part time dummy becomes significant at the 10 percent level.

Columns (2) and (4) of Table 7 also include binary indicators of whether the searcher received Unemployment Insurance benefits and/or welfare payments (AFDC). It is interesting to note that women who collect UI benefits are estimated to search more intensely than those who do not receive benefits. This finding is contrary to most previous empirical studies on the issue (as indicated above, most previous studies found

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UI to weaken search incentives), but may be due to the search requirements associated with many state unemployment insurance programs.³³

The results for men in Atlanta, Boston and Los Angeles are similar to what we saw among the Detroit sample of men in that very young children aged 0-5 are associated with greater search intensity, while children 6-10 are estimated to reduce search intensity (although the latter impact is only marginally significant in the model presented in column (3) and insignificant in column (4)). Again, employed searchers look less intensely (with employment status losing significance when the UI and AFDC dummies are added, possibly due to multicollinearity), and part time work is estimated to significantly reduce search intensity. Adults present in the household and the predicted wage also seem to have a positive impact on hours searched as the model predicts, although the coefficient on PWAGE is insignificant.³⁴

Potential Econometric Problems

In addition to the measurement error problems discussed previously, the present analysis is potentially affected by four separate, but related, econometric problems. The first pertains to whether fertility measures can be safely treated as exogenous within a search intensity equation. Some may argue that fertility and job search decisions are made simultaneously, and thus would be more properly modeled as contemporaneous

³³ The empirical work by Tannery (1983) also finds UI benefits to increase search intensity, theorizing that UI benefits might encourage unemployed workers to allocate greater market expenditures on search activities. The author's data set, however, differs from most studies in that it contains observations only on those who have successfully found work (not on unsuccessful job hunters and those on layoff waiting to be recalled). Blau and Robins (1990) find that those who receive either UI benefits or AFDC use more methods of search (which they attribute to the requirement to register with a state employment service), but make fewer contacts with potential employers and receive fewer job offers.

³⁴ Introducing three additional demographic variables does not significantly change the findings presented in Tables 6 and 7. Interestingly, the age of an individual is not correlated with search time. Marital status does not impact the search intensity of women or men in Detroit, but negatively impacts hours searched for

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endogenous variables. However, since fertility is a long-term (in fact, life-long) decision, whereas job search is a relatively short-term prospect, this problem is not likely to be a significant issue.³⁵ Nevertheless, one should always be careful when interpreting regression results to imply causation.

The second potential econometric problem centers on the limitations of the available data. Without a direct measure of child care costs while searching, it is difficult to discern what the fertility variables in equation (3) are really measuring. As discussed in the theory section, there are many reasons why one would expect children to impact job search intensity. I suspect the strongest reason for the negative relationship between children and search intensity among women centers around the difficulty of finding affordable child care while searching (i.e., the unique search costs children impose on parents). Kisker et al. (1991) find that family day care providers "tend to charge substantially higher hourly rates for part-time than full-time care," so the costs of child care while searching may indeed be significant. It is also possible, however, that women with young children search less intensely, not only because current child care issues directly restrict their search behavior, but also because their effective wage will be lower once they receive a job, due to child care costs while working. That is, we can think of a parent's effective wage as their hourly wage minus the cost per hour worked of child care. Because parents have lower expected effective wages than nonparents do, the benefits of job search are lower.³⁶ This is something that the predicted wage variable does not

men in the Atlanta-Boston-Los Angeles sample. Race does not generally impact search intensity, but white men in the Atlanta-Boston-Los Angeles sample spend more time engaged in job search.

³⁵ Fertility, on the other hand, is likely to be endogenous with respect to labor supply because both are long-term issues.

³⁶ Child care costs will reduce the effective wage of the parent who is the "designated caregiver," traditionally the mother.

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control for. It is important to keep in mind, however, that searchers have already determined that their expected effective wage is high enough to participate (i.e., it exceeds their reservation wage), and the above analyses condition on such labor force decisions. Thus, I expect child care costs while searching to have a stronger impact on search intensity than the expectation of a lower effective wage once a job is found.

The third estimation issue concerns whether the included fertility controls are picking up longer-term labor supply decisions, rather than short-term search constraints. In other words, decisions regarding search intensity for women with children may be driven by decisions about labor supply. For example, women with children who have decided to find work may be restricting their search to certain types of employment (such as part-time or flex-time jobs) because of their children. And since these types of jobs often are associated with lower wages and benefits, the marginal benefit of search time will be lower. Therefore, the negative effects of young children on hours searched for women seen in Tables 6 and 7 may not be due solely to the search constraints imposed by children, but may be partly due to decisions regarding labor supply. To the extent that the dummy variable for working part time in the past controls for such labor supply decisions, the estimates of the effects of children on search intensity are indeed picking up true search constraints for women.

A more generalized version of this econometric problem is endogeneity due to unobserved heterogeneity, or omitted variable bias. Women with children are likely to differ from those without children in many ways, including perhaps in their propensity to search for jobs (or propensity to work hard in the short term). In a reduced-form hours searched equation like (3), one may expect correlation between the fertility measures and

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the error term, ϵ_i , which includes unobservables such as the propensity to search. Consequently, omitting controls for variables like “propensity to search” is likely to result in overestimation of the effect of fertility on search intensity if such variables are negatively correlated with the included fertility measures. Of course there may be other unobservables, such as responsibility, that are positively correlated with fertility, implying the impact of children on search intensity was underestimated. One approach to address this potential heterogeneity problem is to use a proxy variable for unmeasurable characteristics.³⁷ MCSUI does not contain many compelling choices for appropriate proxy variables. However, since the above analyses condition on variables that are likely to be closely related to propensity to search and/or responsibility, such as educational attainment and labor supply decisions (since all searchers have already made the decision to participate), the potential for such a heterogeneity problem is likely mitigated.

Another approach for dealing with unobserved heterogeneity, which I attempt here, is to use a “difference-in-differences” (DD) estimate.³⁸ I include interactions between the fertility measures and a dummy indicator representing the sex of the parent in the search intensity equation:

$$(4) \quad S_i = \alpha + \beta CHILD_i + \phi CHILD_i FEM_i + \eta FEM_i + \delta PWAGE_i + \gamma PTIME_i + \lambda X_i + \epsilon_i,$$

where FEM_i indicates the parent is female.

Equation (4) allows identification of the impact of children on search intensity under certain assumptions on the form of unobserved heterogeneity. That is, assuming the effects of children on search intensity for fathers reflect only unobserved heterogeneity and not a true search constraint, then the difference in the estimated effects

³⁷ Alternatively, one could use panel data and difference out time-invariant factors such as propensity to search.

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between mothers and fathers yields an estimate of the effect of children on search intensity. This DD estimate is unbiased so long as no gender-specific unobservables are correlated with the fertility measures.

Table 8 presents DD estimates of the effects of children on search intensity from regressions of equation (4) above, which include interactions between sex of parent and the child dummies, a dummy for sex of parent, and pool men and women together. Results are presented from two estimated equations for each sample (Detroit v. Atlanta-Boston-Los Angeles), corresponding to the specifications used in Tables 6 and 7 (Appendix D contains coefficients on the control variables). The coefficients on the interaction terms indicate the difference in the effect children have on mothers in comparison to fathers, and thus equal the DD estimates. If one assumes that the father effect reflects unobserved heterogeneity, then we can interpret the results from Table 8 as indicating that only very young children aged 0-5 have a negative impact on job search intensity. Note that the magnitudes of the DD estimates for younger children actually exceed those presented in Tables 6 and 7.

The results for men presented in Tables 1 and 2 and Tables 6 and 7 may call into question the assumption that the effects of children on search intensity for fathers reflect only unobserved heterogeneity and not a true search constraint. Nevertheless, if the father effect is picking up some heterogeneity, we can conclude that the results for women presented in Tables 6 and 7 represent a lower-bound estimate of the impact that young children 0-5 have on search intensity.

³⁸ See Gruber (1994) and Holzer and Ihlanfeldt (1998).

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Extensions and Concluding Remarks

This study provides considerable evidence that young children, and the child care concerns associated with them, significantly restrict the job search intensity of women.³⁹ These are significant findings given the importance of search behavior in determining labor market outcomes, such as wages, unemployment duration and the probability of remaining in the labor force, coupled with the current political attention to child care as a national concern. This is true regardless of whether the negative relationship between children and search intensity is primarily due to the cost of child care while searching or to the expectation of a lower effective wage once work is found due to child care costs while working. Determining the relative importance of these two factors, however, should influence the proper policy prescriptions.

Politicians have recently begun to understand the importance of child care issues. In fact, several important steps have already been taken to meet the challenges that working parents face. For example, the Family and Medical Leave Act (FMLA) of 1993 attempts to alleviate the likelihood of job loss resulting from interrupted labor force participation due to childbirth by allowing a recent parent the right to return to their previous employer after maternity or paternity leave of up to 12 weeks. Unfortunately, the federal FMLA covers only about a third of all working mothers (Klerman and Leibowitz, 1994) and does nothing to help new labor market entrants with children. Politicians have also significantly increased the amount of money targeted directly toward child care in recent years. The welfare reform law of 1996, for example, included \$14 billion in funding for child care subsidies to low-income families over six years – a

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Nevertheless, a recent Department of Health and Human Services (DHHS) report found that only 10 percent of low-income families eligible for federal child care assistance in 1998 received it (U.S. DHHS, 1999a). Former DHHS Secretary Donna Shalala commented on the report, noting "This timely and important report provides conclusive evidence that millions of low-income parents eligible for child care assistance are not getting the critical support they need to stay employed" (U.S. DHHS, 1999a).

Moreover, while welfare reform has shown some success in moving people from welfare into the labor market, with the percentage of welfare recipients working increasing from seven percent in 1992 to 27 percent in 1998 (U.S. Department of Health and Human Services, 1999b), there is clearly room for improvement. It is widely acknowledged that one of the biggest remaining challenges of welfare reform is ensuring that those who leave welfare for jobs remain employed. But what is not widely known is the important role job search can potentially play in meeting this challenge. Search intensity, measured by weekly hours searched, has been shown to impact both the duration of unemployment and the probability of dropping out of the labor force (Keeley and Robins, 1985; Barron and Mellow, 1981). Job seekers spend time searching to gather information about prospective employers, such as the wages and benefits offered, the work environment provided, and the expected satisfaction associated with a potential job. Thus it is also likely that hours searched will affect ultimate wages received and the success of employee-employer matches, which in turn should impact the probability of remaining in the labor force.

³⁹ The results presented in Tables 6 and 7 also indicate that for men, while children aged 6-10 may negatively impact search behavior, very young children, those aged 0-5, are associated with an increase in

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While this paper establishes the significant impact that children have on search intensity, further research should be directed at a thorough investigation of the impact of restricted search effort due to child care issues on received wages and the duration of employment. Future research should also investigate the relationships between children and other aspects of search behavior. Due to data limitations I explore just one dimension of the search process – the time devoted to job search. This paper looked specifically at time spent searching under the assumption that children are most likely to impact this aspect of search effort due to the costs associated with employing child care services while looking for work. It is certainly possible that children may affect other aspects of search effort as well, such as the number of employers contacted.

Although time devoted to job search has been shown to significantly affect labor market outcomes, Keeley and Robins (1985) found that in terms of reducing the duration of unemployment, forms of job search associated with direct employer contacts are even more productive. That is, weekly rates of employer contacts, employer visits and job applications were more strongly related to unemployment duration than more indirect aspects of job search, such as weekly hours of search and the number of search methods used. It is possible that individuals with young children may employ less time-intensive methods of search, such as sending out a standard cover letter and resume to several employers from their home, as opposed to "pounding the pavement" while paying for child care services.⁴⁰

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⁴⁰ Unfortunately, MCSUI is not the best data set to address these issues. Although respondents were asked about employers contacted, in Detroit the question was open ended, while in the other cities the question pertained to the last month of job search.

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It is clear that more work is needed to fully understand the relationship between children and job search behavior. By investigating the impact of children on search intensity, however, I hope to have at least enriched our understanding of the role that child care constraints play in the employment process for women and men. The potential impact of search constraints on mothers trying to make the welfare-to-work transition and the obvious policy-making implications make this paper an important first step. The findings presented here provide significant evidence that policy makers need to be aware that child care concerns not only affect parents once employed, but that they may also significantly impede the process of finding a job in the first place and maintaining employment in the future.

APPENDICES

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APPENDIX A

Construction of Variables and Discussion of Outliers

- 1) Search Intensity (S) – For Detroit, this variable represents search intensity measured by hours devoted to job search per week. The variable ranges from 0 to 48 hours per week, after omitting two outlier responses of 72 and 50 (discussed below). In Atlanta, Boston and Los Angeles, respondents were asked “In total, about how many hours did you spend looking for work in (the last thirty days/the last month) of your job search?” I divided the resulting variable by four to get a weekly measure. As mentioned above, I restricted the sample to those who had looked for work within the past 30 days and to those with search duration of at least one month. The variable ranges from 0 to 75 hours per week after omitting 3 observations ranging from 90 to 200 hours per week. In addition, three diagnostic tests, Cook's distance, Welsch distance and DFITS, were performed to detect potential outliers (see Stata Corp. pp. 372-396). As a result, six observations that failed all three of these tests (based on the suggested cutoffs) were omitted: specifically, case ids 1375, 354, 2335, 2747, 4901 and 4570. These observations were quite influential and the model did not fit them well. Direct inspection of the characteristics of the individuals detected by the three formal tests was used as a secondary test to determine omission. For example, the Detroit respondent who reported 50 hours had previously reported that she had not searched for work within the past 30 days, and thus should not have been asked the search intensity question.
- 2) Nonwage Income – Nonwage income is a categorical variable ranging from 1 to 20 meant to measure income sources not derived from labor in order to give an

indication of one's means of support while searching for a job. It was created by subtracting employment earnings from family income. Since family income was reported as a categorical variable ranging from 1 to 20 (the actual income categories corresponding to these numbers are available upon request), earnings from employment was transformed into a similarly-defined categorical variable before it was subtracted from family income. Both family income and earnings from employment pertain to the full year prior to the date of the survey. However, if prior-year earnings from employment were not reported, annual earnings on current/last main job were used in its place. (In Detroit, respondents were only asked to report prior-year earnings if they were not currently working at the time of the survey, but had worked sometime in the previous two years.)

- 3) Hourly Wage (HWAGE) – The hourly wage variable was constructed from an earnings variable that pertains to the respondent's current or last main job. If the respondent did not report earnings as an hourly figure, but used another unit of measure such as annual earnings, earnings were transformed into an hourly measure based upon the individual's usual hours worked per week, if available. In cases for which weekly hours worked were not reported, the earnings variable was transformed into an hourly rate assuming the individual worked full time, or 40 hours per week. Wages less than \$2 per hour (78 cases) or exceeding \$500 per hour (6 cases) were classified as outliers. In these cases the hourly wage variable was replaced with a missing value. In defense of this trimming technique, Angrist and Krueger (1998) found that "extreme wage values are likely to be mistakes," when they investigated the impact of trimming outliers using CPS data.

The potential for recall bias arises since the earnings variable is based upon a retrospective question. In Atlanta and Detroit all those who had worked sometime within the past 5 years were asked the earnings question (referring to their current or last main job), while in Boston and Los Angeles everyone who had worked sometime within the past 6 years was asked the earnings question.

- 4) Current Wage (WAGE) – WAGE is an hourly wage variable created from the log of HWAGE, using observations on those currently employed only. This conversion was meant to mitigate the potential recall bias described above. To control for any remaining differences in local labor market conditions, dummy variables for metropolitan area are included in all of the regression analyses involving this variable.
- 5) Part time employment (PTIME) - This variable was constructed from a question that asked respondents how many hours per week they usually worked at their current or most recent job (respondents who had not worked within the past five years in Detroit, or since 1987 in Atlanta, Boston and Los Angeles, were not asked this question). A binary variable was created equaling one if the respondent reported that they worked less than 35 hours a week.

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APPENDIX B

Predicting Wages and Sample Selection Bias

In order to predict a wage for the sample of searchers, a model was first specified to describe the wages of respondents in the full sample. The log wage equations, run separately for men and women, took the following form:

$$(5) \quad \text{WAGE}_i = \beta_0 + \beta_1 Z_{1i} + U_{1i},$$

where the vector Z_{1i} includes controls for age, education, race and metropolitan area. Typically, the estimated coefficients from (5) run on the full sample would be applied to the characteristics of the sample of searchers to calculate their predicted wages.

It is important to note, however, that WAGE_i is only observed for a nonrandom sample of individuals who were employed at the time of the survey, which potentially gives rise to the classic sample selection problem. If individuals who are employed are not representative of the general population, failure to control for the differences will lead to biased estimates of the coefficients in (5).⁴¹

Since current wages are observed only if the individual is employed, the employment selection equation will take the form

$$(6) \quad \text{EMP}_i = 1 [Z_i \delta + U_{2i} > 0],$$

where U_{2i} has a standard normal distribution, and is potentially correlated with U_{1i} . Sample selection bias will be a problem -- that is, OLS applied to equation (5) will lead to inconsistent results -- if U_{1i} and U_{2i} are correlated.

⁴¹ Prior research has found that sample selection bias is usually only an issue for women in such models.

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To correct for potential sample selection bias, I employ the Heckman selection correction procedure⁴², which provides consistent, asymptotically efficient estimates for such models. In order to identify the parameters of the model, variables that strongly affect employment status but not the wage offer must be included in Z_i but not in Z_{1i} . In this analysis, Z_i includes a dummy variable for marital status and measures of the number of children in the household aged 0-5, 6-10 and 11-17 for identification.⁴³ Column (3) of Appendix C presents the results from the maximum likelihood estimation of the model specified by equations (5) and (6) utilizing the Heckman correction procedure. As an alternative test for sample selection bias (since the Heckman procedure is known to be sensitive to misspecification), I also employed a two-step least squares procedure; the results of the second step are found in column (4).⁴⁴ For comparison purposes, columns (1) and (2) of Appendix C present the results from OLS estimation of equation (5) for men and women, uncorrected for sample selection bias. The estimated coefficients from model (5) presented in columns (1) and (3) of Appendix C were used to construct the predicted wage, PWAGE, for the sample of searchers. However, the results of Appendix C indicate that there is no strong evidence sample selection bias; the two correction procedures resulted in little change in the size and significance of estimated coefficients

⁴² See Heckman (1979) for a more detailed discussion of the problem, and StataCorp (1997, pp. 187-195) for a description of the correction procedure used in this analysis.

⁴³ There is much controversy in the literature regarding the appropriateness of variables typically used for identification. Obviously, some may argue that the variables included in (5) for identification should also be included in the wage equation (5). Ideally, a measure of nonwage income would be used for identification, however, this variable is missing for approximately 17 percent of the full sample (and is potentially plagued with a similar self-selection problem).

⁴⁴ In the first stage, a probit model is specified for employment, which includes the complete set of regressors used in (5), along with a marriage dummy and indicators of the number of children in the household for identification. This employment probit was run on the full sample, and used to compute the inverse Mills ratio. In the second stage, the inverse Mills ratio was included as a Z_i regressor in the log wage regression (5), which was run on the sub-sample of workers. Several different specifications were tested, and in virtually every case there was no evidence of selectivity bias.

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compared to the model that does not control for sample selection bias, and in both cases the estimated coefficient on the inverse Mills ratio is insignificant.

APPENDIX C
Log-of-Wages Equations by Gender, Full Sample

	Log Wage Model		Heckman	Two-Step
	<u>Men</u>	<u>Women</u>	Correction Model	Correction Model
	(1)	(2)	<u>Women</u>	<u>Women</u>
	(1)	(2)	(3)	(4)
Age	.057 (.011)	.048 (.014)	.048 (.005)	.049 (.013)
Age squared	-.0005 (.0001)	-.0005 (.0002)	-.0005 (.0001)	-.0005 (.0002)
Atlanta	-.150 (.063)	-.129 (.045)	-.129 (.033)	-.120 (.049)
Boston	-.081 (.067)	.043 (.061)	.047 (.028)	.057 (.064)
Detroit	-.023 (.052)	-.165 (.049)	-.162 (.028)	-.154 (.051)
Black	-.157 (.050)	-.056 (.040)	-.058 (.029)	-.057 (.040)
Asian	-.174 (.075)	.061 (.091)	.054 (.055)	.057 (.092)
Hispanic	-.317 (.061)	-.237 (.059)	-.237 (.032)	-.235 (.060)
High school/GED	.250 (.049)	.377 (.058)	.383 (.037)	.396 (.070)
Associates/ Vocational/Trade	.365 (.060)	.523 (.063)	.534 (.044)	.552 (.086)
College degree	.536 (.065)	.722 (.060)	.723 (.043)	.752 (.087)
Graduate degree	.644 (.101)	.772 (.096)	.786 (.053)	.809 (.126)
Inverse Mills	-	-	.055 (.099)	.064 (.118)
Intercept	1.011 (.214)	.949 (.252)	.918 (.124)	.865 (.281)
R ²	.346	.282	.282	.282

Note: Sample consists of non-retired respondents. Sample sizes are 2,027 and 1,919 for women and men, respectively. All estimates are sample weighted. Robust standard errors appear in parentheses and are adjusted for survey design. Omitted categories for region, race and education, are Los Angeles, white and high school drop out, respectively.

APPENDIX D
Coefficients (Standard Errors) on Control Variables for Table 8

	Detroit		Atlanta-Boston-Los Angeles	
	(1)	(2)	(3)	(4)
Presence of children 0-5	3.41* (1.78)	4.30 (2.16)	3.32 (2.57)	4.31** (2.22)
Presence of children 6-10	-3.81* (1.96)	-4.42** (2.04)	-3.38 (2.10)	-3.15* (1.86)
Nonwage income	-.712** (.318)	-.680** (.337)	.302 (.332)	.226 (.323)
Employed	-7.79*** (1.80)	-7.66*** (1.82)	-4.50*** (1.49)	-4.04** (1.73)
(Nonwage income) x (employed)	.935*** (.354)	.921*** (.345)	-.271 (.413)	-.223 (.398)
Adults 36-55	1.01 (1.34)	1.65 (1.40)	1.52 (1.39)	1.71 (1.41)
Young adults 19-35	2.23** (1.05)	2.23*** (1.03)	-.613 (.600)	-.275 (.603)
Part time job	-1.33 (1.58)	-1.27 (1.53)	-3.84*** (1.47)	-3.71*** (1.45)
AFDC	-	-	-	-2.91** (1.45)
UI benefits	-	-	-	3.35* (1.80)
Predicted (log) wage	-	4.86 (4.70)	-	2.14 (3.84)
<u>Controls:</u>				
Education	Y	Y	Y	Y
Metropolitan area	-	-	N	Y
Constant	13.32	2.53	11.46	4.82
R ²	.238	.242	.210	.254

Note: Sample sizes are 201, 200, 720, 714 for specifications 1-4, respectively. Regressions include a missing value dummy variable for part time. All estimates are sample weighted. Robust standard errors appear in parentheses and are adjusted for survey design. Omitted categories are high school drop out for education and Los Angeles for metropolitan area. (* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.)

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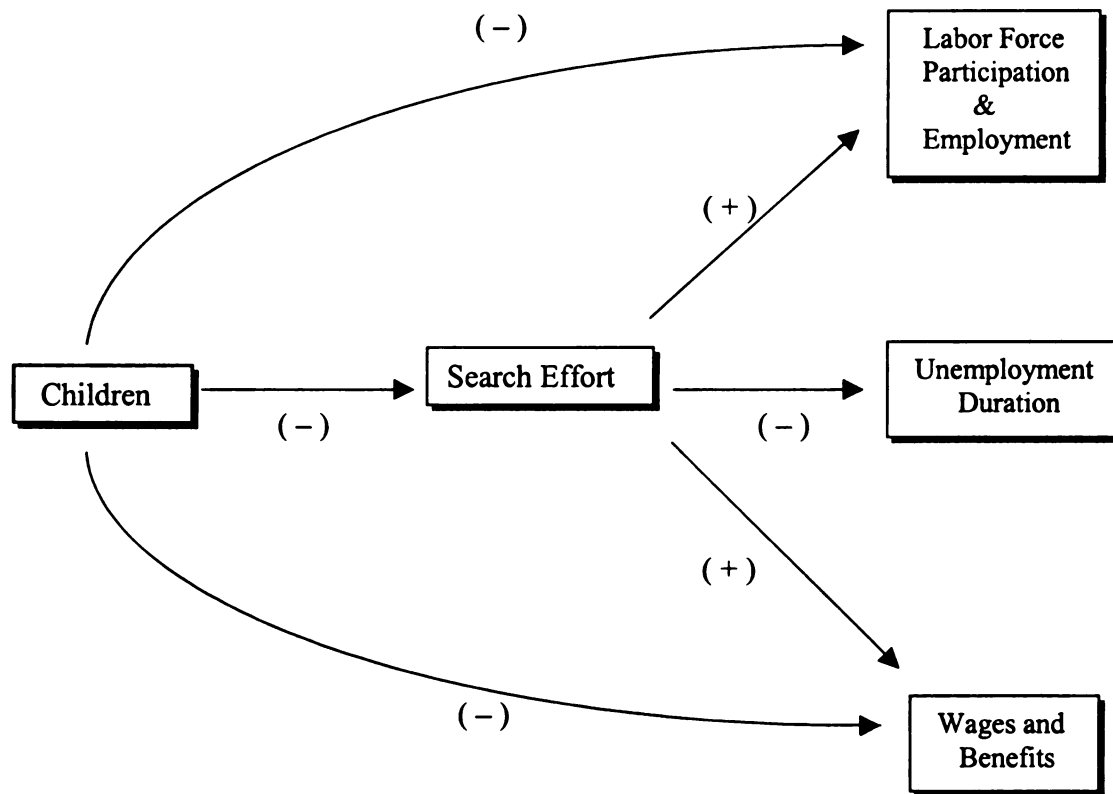
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Figure 1
The Relationships between Children and Labor Market Outcomes of Women
and the Predicted Job Search Link



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Table 1
How Child Care Affects Employment:
Perceived Child Care Constraints in Atlanta, Boston and Los Angeles by Sex

<u>"In past 12 months, have child care concerns caused you to":</u>	<u>Women</u>	<u>Men</u>
Not look or apply for work	.320 (.024)	.083 (.017)
Turn down a job offer	.127 (.016)	.038 (.010)
Not participate in school/training	.194 (.020)	.069 (.015)
Quit or be fired	.063 (.011)	.014 (.007)
Be late for work	.352 (.032)	.268 (.028)
Be absent from work	.394 (.032)	.259 (.028)
Change hours of work	.340 (.031)	.217 (.027)
Lose out on promotion/raise	.052 (.014)	.030 (.012)

Note: Respondents with a child under 18 at home were asked questions generating the first four rows of data. Sample sizes are 2,197 for women and 1,000 men, with some cells containing fewer observations due to missing values. Respondents with a child less than 18 at home who had worked in the last 12 months were asked questions generating the last four rows of data. Sample sizes for these rows are 1,213 for women and 880 for men, with some cells containing fewer observations due to missing values. Standard errors appear in parentheses. All results are sample-weighted and standard errors are adjusted for stratification and clustering of survey design.

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Table 2
How Child Care Affects Employment:
Perceived Child Care Constraints in Detroit by Sex

	<u>Women</u>	<u>Men</u>
Perception of child care constraints on employment	.310 (.023)	.250 (.029)
Cost mentioned	.615 (.046)	.390 (.066)
Quality mentioned	.204 (.038)	.104 (.039)
<u>Perceived constraints on:</u>		
Amount & timing of work	.254	.316
Getting job or choice of job	.165	.156
Ability to enter the labor force or maintain job	.432	.347
Other	.149	.181

Note: Survey question asked of respondents with children under 18 was the following: "Has the cost, availability or quality of child care ever influenced your employment or that of your (spouse/partner) in any way?" Data in the last four rows contain responses to the following question: "In what ways did these issues influence you or your (spouse's/partner's) employment?" Sample sizes are 590 for women and 305 for men in rows one through three, and 143 for women and 52 for men in rows four through seven. Some cells contain fewer observations due to missing values. Other category includes responses discussing the difficulty of raising children and working and other non-specific responses. Standard errors appear in parentheses. All results are sample-weighted and standard errors are adjusted for stratification and clustering of survey design.

Table 3
Hours Searched per Week by Sex and Presence of Children

A. Detroit	<u>Child under 6</u>	<u>Child 6-10</u>	<u>No children <10</u>
Women	3.98 (.795)	3.40 (.533)	10.59 (1.94)
Men	6.67 (1.52)	4.60 (1.53)	8.97 (1.41)
B. Atlanta, Boston and Los Angeles	<u>Child under 6</u>	<u>Child 6-10</u>	<u>No children <10</u>
Women	3.36 (.552)	5.98 (1.58)	6.00 (1.09)
Men	11.10 (2.36)	6.85 (1.94)	9.81 (1.91)

Note: Sample sizes are 111 and 92 for women and men in Detroit, respectively, and 401 and 319 for women and men in the other cities, respectively. Standard errors appear in parentheses. All results are sample-weighted and standard errors are adjusted for stratification and clustering of survey design.

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Table 4
Perceived Child Care Constraints in Atlanta, Boston and Los Angeles
by Sex and Presence of Children

A. Women			
<u>"In past 12 months, have child care concerns caused you to":</u>	<u>Child under 6 at home</u>	<u>Child 6-10 at home</u>	<u>Child 11-17 at home</u>
Not look or apply for work	.454 (.037)	.411 (.041)	.236 (.029)
Turn down a job offer	.153 (.023)	.149 (.028)	.090 (.021)
Not participate in school/training	.269 (.028)	.247 (.035)	.170 (.033)
Quit or be fired	.090 (.020)	.046 (.015)	.042 (.014)
Be late for work	.462 (.050)	.444 (.054)	.236 (.035)
Be absent from work	.526 (.051)	.479 (.052)	.268 (.038)
Change hours of work	.428 (.047)	.476 (.051)	.282 (.041)
Lose out on promotion/raise	.083 (.025)	.065 (.027)	.041 (.020)

Note: Respondents with a child under 18 at home were asked questions generating the first four rows of data. Sample size is 2,197 for women, with some rows containing fewer observations due to missing values. Respondents with a child less than 18 at home who had worked in the last 12 months were asked questions generating the last four rows of data. Sample size for these rows is 1,213, with some rows containing fewer observations due to missing values. Standard errors appear in parentheses. All results are sample-weighted and standard errors are adjusted for stratification and clustering of survey design.

Table 4 (cont'd)

B. Men			
	Child under 6 <u>at home</u>	Child 6-10 <u>at home</u>	Child 11-17 <u>at home</u>
<u>“In past 12 months, have child care concerns caused you to”:</u>			
Not look or apply for work	.073 (.022)	.151 (.041)	.075 (.031)
Turn down a job offer	.038 (.013)	.057 (.026)	.024 (.015)
Not participate in school/training	.091 (.024)	.070 (.026)	.036 (.017)
Quit or be fired	.006 (.003)	.037 (.023)	.013 (.011)
Be late for work	.325 (.023)	.332 (.028)	.224 (.023)
Be absent from work	.293 (.022)	.307 (.027)	.225 (.023)
Change hours of work	.255 (.021)	.287 (.026)	.163 (.020)
Lose out on promotion/raise	.031 (.008)	.066 (.015)	.001 (.001)

Note: Respondents with a child under 18 at home were asked questions generating the first four rows of data. Sample size is 1,000 men, with some rows containing fewer observations due to missing values. Respondents with a child less than 18 at home who had worked in the last 12 months were asked questions generating the last four rows of data. Sample size for these rows is 880, with some rows containing fewer observations due to missing values. Standard errors appear in parentheses. All results are sample-weighted and standard errors are adjusted for stratification and clustering of survey design.

Table 5
Means and Standard Errors of Variables Used for Job Searchers
by Sex and Metropolitan Area

	Detroit		Atlanta-Boston-Los Angeles	
	<u>Women</u>	<u>Men</u>	<u>Women</u>	<u>Men</u>
Hours searched per week	7.91 (1.42)	8.33 (1.25)	5.58 (.816)	8.75 (1.48)
Presence of children 0-5	.243 (.055)	.175 (.042)	.171 (.038)	.133 (.036)
Presence of children 6-10	.232 (.053)	.157 (.039)	.210 (.054)	.174 (.044)
Employed	.614 (.066)	.549 (.062)	.398 (.070)	.508 (.054)
Nonwage income	3.98 (.546)	4.16 (.686)	3.57 (.396)	3.52 (.533)
# of adults 35-64	.236 (.097)	.490 (.123)	.341 (.133)	.371 (.107)
# of young adults 18-34	.364 (.105)	.353 (.061)	.530 (.106)	.595 (.151)
High school drop out	.218 (.060)	.065 (.024)	.145 (.042)	.175 (.033)
High school/GED	.448 (.065)	.599 (.068)	.324 (.053)	.437 (.059)
Associates/ Vocational/Trade	.128 (.037)	.144 (.052)	.249 (.071)	.099 (.030)
Bachelor's degree	.139 (.054)	.133 (.046)	.198 (.046)	.190 (.044)
Advanced degree	.067 (.034)	.059 (.029)	.083 (.027)	.099 (.031)
Log of predicted Wage	2.17 (.047)	2.56 (.045)	2.36 (.031)	2.53 (.044)

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Table 5 (cont'd)

Unemployment insurance benefits	-	-	.248 (.046)	.324 (.050)
AFDC	-	-	.143 (.039)	.042 (.019)
Atlanta	-	-	.136 (.027)	.080 (.017)
Boston	-	-	.360 (.063)	.330 (.060)
Los Angeles	-	-	.504 (.057)	.590 (.057)

Note: Sample sizes are 111 and 92 for women and men in Detroit, respectively, and 401 and 319 for women and men in the other cities, respectively. Some cells contain fewer observations due to missing values. Standard errors appear in parentheses. All results are sample-weighted and standard errors are adjusted for stratification and clustering of survey design.

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Table 6
Search Intensity Equations by Sex, Detroit Sample

	Women		Men	
	(1)	(2)	(3)	(4)
Presence of children 0-5	-4.84*** (1.32)	-4.67*** (1.53)	5.73*** (1.33)	6.69*** (1.53)
Presence of children 6-10	-5.32*** (1.57)	-5.39*** (1.55)	-7.28*** (1.35)	-7.99*** (1.42)
Nonwage income	-.960*** (.285)	-.976*** (.339)	-.550** (.219)	-.362 (.255)
Employed	-7.40** (3.10)	-7.57** (3.54)	-10.29*** (1.87)	-9.50*** (1.75)
(Nonwage income) x (employed)	.705* (.424)	.722 (.453)	1.25*** (.355)	1.05*** (.361)
Adults 36-55	4.94** (1.96)	5.02** (2.13)	-.856 (1.19)	-.193 (1.38)
Young adults 19-35	.838 (.800)	.852 (.795)	3.34* (1.77)	3.37* (1.75)
Part time job	2.30 (1.73)	2.30 (1.73)	-5.83*** (2.05)	-5.71*** (2.04)
High school/GED	.480 (2.22)	-.123 (3.52)	-2.75 (2.41)	-4.41* (2.68)
Associates degree/ Vocational/Trade	.433 (2.25)	-.415 (4.85)	2.20 (3.39)	-.340 (3.02)
College degree	9.09 (5.74)	8.05 (9.76)	-4.40 (2.87)	-9.05** (3.85)
Advanced degree	-4.66* (2.53)	-6.16 (8.32)	.974 (3.49)	-3.66 (4.51)
Predicted (log) wage	-	1.56 (9.09)	-	4.04 (4.08)
Constant	13.58	10.92	14.50	5.28
R ²	.381	.381	.471	.487

Note: Sample sizes are 109 and 91 for women and men, respectively. Regressions include a missing value dummy variable for the part time. All estimates are sample weighted. Robust standard errors appear in parentheses and are adjusted for survey design. Omitted category for education is high school drop out. (* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.)

Table 6
Search Intensity Equations by Sex, Detroit Sample

	Women		Men	
	(1)	(2)	(3)	(4)
Presence of children 0-5	-4.84*** (1.32)	-4.67*** (1.53)	5.73*** (1.33)	6.69*** (1.53)
Presence of children 6-10	-5.32*** (1.57)	-5.39*** (1.55)	-7.28*** (1.35)	-7.99*** (1.42)
Nonwage income	-.960*** (.285)	-.976*** (.339)	-.550** (.219)	-.362 (.255)
Employed	-7.40** (3.10)	-7.57** (3.54)	-10.29*** (1.87)	-9.50*** (1.75)
(Nonwage income) x (employed)	.705* (.424)	.722 (.453)	1.25*** (.355)	1.05*** (.361)
Adults 36-55	4.94** (1.96)	5.02** (2.13)	-.856 (1.19)	-.193 (1.38)
Young adults 19-35	.838 (.800)	.852 (.795)	3.34* (1.77)	3.37* (1.75)
Part time job	2.30 (1.73)	2.30 (1.73)	-5.83*** (2.05)	-5.71*** (2.04)
High school/GED	.480 (2.22)	-.123 (3.52)	-2.75 (2.41)	-4.41* (2.68)
Associates degree/ Vocational/Trade	.433 (2.25)	-.415 (4.85)	2.20 (3.39)	-.340 (3.02)
College degree	9.09 (5.74)	8.05 (9.76)	-4.40 (2.87)	-9.05** (3.85)
Advanced degree	-4.66* (2.53)	-6.16 (8.32)	.974 (3.49)	-3.66 (4.51)
Predicted (log) wage	-	1.56 (9.09)	-	4.04 (4.08)
Constant	13.58	10.92	14.50	5.28
R ²	.381	.381	.471	.487

Note: Sample sizes are 109 and 91 for women and men, respectively. Regressions include a missing value dummy variable for the part time. All estimates are sample weighted. Robust standard errors appear in parentheses and are adjusted for survey design. Omitted category for education is high school drop out. (* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.)

Table 7
Search Intensity Equations by Sex, Atlanta-Boston-Los Angeles Sample

	Women		Men	
	(1)	(2)	(3)	(4)
Presence of children 0-5	-4.15*** (1.13)	-3.86*** (.993)	4.65* (2.40)	5.77*** (2.19)
Presence of children 6-10	1.40 (1.70)	1.72 (1.63)	-2.91 (2.03)	-2.42 (2.08)
Nonwage income	-.145 (.199)	-.173 (.223)	.782 (.461)	.659 (.458)
Employed	-5.47*** (1.80)	-5.02*** (1.80)	-3.38* (1.85)	-3.60 (2.32)
(Nonwage income) x (employed)	.021 (.330)	-.045 (.357)	-.822 (.541)	-.834 (.541)
Adults 36-55	-.214 (.925)	.069 (1.06)	3.18* (1.68)	3.92** (1.78)
Young adults 19-35	-.490 (.571)	-.196 (.623)	-.667 (.996)	.195 (.884)
Part time job	-2.29 (1.51)	-2.43* (1.35)	-5.99*** (2.05)	-4.90** (2.08)
AFDC	-	-1.51 (1.48)	-	-2.36 (3.12)
UI benefits	-	3.65** (1.77)	-	2.69 (2.45)
Predicted (log) wage	-	-1.88 (3.25)	-	2.20 (4.81)
<i>Controls:</i>				
Education	Y	Y	Y	Y
Metropolitan area	N	Y	N	Y
Constant	7.97	10.41	11.08	3.72
R ²	.242	.298	.314	.371

Note: Sample sizes are 401 and 319 for women and men, respectively. Regressions include a missing value dummy variable for part time. All estimates are sample weighted. Robust standard errors appear in parentheses and are adjusted for survey design. Omitted categories are high school drop out for education and Los Angeles for metropolitan area. (* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.)

Table 8
Difference-in-Differences Estimates of
the Effects of Children on Search Intensity

A. Detroit Sample	Difference-in-differences (DD) estimates:	
	(1)	(2)
(Children 0-5) x (female parent)	-9.05*** (2.26)	-9.14*** (2.33)
(Children 6-10) x (female parent)	-1.28 (2.81)	-.821 (3.00)
R ²	.238	.242
B. Atlanta-Boston-Los Angeles Sample		
(Children 0-5) x (female parent)	-7.88*** (2.87)	-7.96*** (2.59)
(Children 6-10) x (female parent)	5.06* (2.81)	4.58 (2.80)
R ²	.210	.254

Note: Sample sizes are 109 and 91 for women and men, respectively. Included controls correspond to those used in Tables 6 and 7, and the estimated coefficients on these controls are reported in Appendix D. All estimates are sample weighted. Robust standard errors appear in parentheses and are adjusted for survey design. Omitted category for education is high school drop out. (* Significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.)

Chapter 2

RACIAL AND ETHNIC JOB SEGREGATION: ITS CAUSES AND CONSEQUENCES

Introduction

Although considerable progress has been made in reducing racial inequality since passage of the Civil Rights Act of 1964, the employment and earnings of blacks and other minority groups continue to fall short of their white counterparts. Most empirical studies find a residual wage gap between whites and minorities even after controlling for standard productivity proxies such as education and experience. Several of the dominant theories put forth to explain these persistent wage differentials – including Becker’s (1971) “taste” models of discrimination and the “spatial mismatch hypothesis” – imply that minorities are likely to be segregated from whites in the labor market. Just as women are thought to be crowded into lower-paying “female occupations,” minority groups may be segregated into jobs with lower wages and benefits for a variety of reasons. This study investigates the importance of racial and ethnic job segregation on hourly earnings and the likelihood of receiving various employment benefits. The study also explores the factors likely to contribute to the job segregation of blacks and Hispanics in the labor market.

While there is a large body of literature on the effect of sex occupational segregation on the wage gap between women and men (e.g., Macpherson and Hirsh, 1995), evidence on the effects of racial and ethnic segregation on labor market outcomes for minorities is fairly limited. A potential reason for this shortfall is that prior studies find that occupational segregation between races and ethnic groups is significantly less

pronounced than occupational segregation between the sexes (Watts, 1995; Bayard et al., 1999). For example, Sorensen (1989) investigates the effect of occupational segregation by sex and race on earnings differentials between demographic groups. She finds that occupational segregation by sex significantly affects earnings, accounting for roughly 20 percent of the wage gap between men and women (both white and minority), and 3 percent of the wage gap between white and minority men. However, occupational segregation by race is not found to be a significant factor influencing earnings.

Bayard et al. (1999) study the wage effects of racial and ethnic segregation by industry, occupation, establishment, and job, in an attempt to explain the larger wage gaps found for men than for women.¹ While they do not find racial and ethnic segregation to be pervasive along occupation or industry lines, they find a great deal of segregation by establishment and at the job level. They conclude that the negative segregation effect on minority wages stems primarily from job-level segregation.

Carrington and Troske (1998) use a measure of racial composition at the establishment level to decompose the black-white wage gap in the manufacturing industry into within- and between-plant components. The authors find that most of the racial wage gap among men is accounted for by within-plant differences in pay. A shortcoming of these two studies, however, is that the data used provide only limited productivity-related controls; neither Carrington and Troske (1998) nor Bayard et al. (1999) control for potentially

¹ Bayard et al. (1999) use matched employer-employee data created from the Sample Detail File (SDF) of the 1990 Decennial Census One-in Six Long Form and the 1990 Standard Statistical Establishment List (SSEL). Segregation is measured as the percentages black and Hispanic in an individual's industry, occupation, establishment, and job cell. The percentages in the occupation and industry are estimated from the full SDF sample, so measurement error is unlikely. However, as the authors explain, measurement error in the estimates of establishment and job segregation "could be sizable," since they are based on the matched data (see Bayard et al., 1998, for a detailed description of the matching process). Only 19.4 workers are matched to an establishment on average, so their job-level segregation estimates, in particular,

important wage determinants, such as tenure, job tasks, job-specific experience, and whether one is covered by a collective bargaining agreement.²

Utilizing slightly more extensive controls, including union membership and tenure, Hirsch and Schumacher (1992) investigate the effect of racial segregation on wage rates and the black-white wage gap using a measure of racial density within industry-occupation-region cells (measured by the ratio of black workers to the sum of white and black workers in these cells). They find that wages of both white and black workers decrease with respect to their measure of racial density, but that the black-white wage gap does not vary systematically with respect to racial density by industry-occupation-region group.

Hirsch and Macpherson (1994) illustrate the importance of including adequate controls for skill differences. Using a national estimate of the racial composition of occupations, they find that the negative effect of occupational race segregation on wages is sharply reduced when measures of occupational skill level (e.g., required years of training, job tenure, computer usage) are included. Moreover, the authors are able to virtually eliminate the negative effect of racial segregation at the occupation level by estimating wage-change equations that difference out unmeasured individual skill differences fixed over time.

The study here contributes to the current body of research in several ways. In contrast to most prior work (with the exception of Bayard et al., 1999), I use racial and

are often based on a small number of observations. The authors thus use highly aggregated occupations (based on 13 Census occupations in most specifications) to measure job-level segregation.

² Experience and tenure, in particular, are important characteristics that should be included in wage analyses investigating discrimination. Bratsberg and Terrel (1998) shed light on the persistence of black-white wage differentials by investigating the differing returns to tenure and general experience. They find that returns to general experience for black workers trail those for whites, but black workers earn equal if not higher returns to tenure than white workers earn.

ethnic composition information measured at the job level within a firm. Given the results of past studies that find firm- and job-level segregation by race and ethnicity affects earnings differentials to a much greater extent than occupational segregation, job-level segregation information is preferred over occupation-level information. Furthermore, some of the processes affecting racial and ethnic segregation are likely to occur within a firm, rather than simply at the point of sorting into an occupation (as discussed below). The second contribution of this study relates to the breadth of the data used, which were drawn from a relatively new survey of households administered between 1992 and 1994 in three major metropolitan areas. These data allow wage differentials by demographic group to be estimated using an extensive collection of productivity-related measures not available to most previous researchers who have studied this issue, such as job tasks, supervisory authority, and job-specific experience. Even after controlling for differences in personal human capital and job characteristics, I find racial and ethnic job segregation to be an important contributor to the lower wages paid to blacks and Hispanics than to similar whites.

This study also examines the potential impact of racial and ethnic segregation on the likelihood of receiving various employment benefits. The data used here contain information on whether jobs provide a retirement plan, paid sick leave, and individual or family health insurance. Job segregation is found to play a smaller role in explaining differences between minorities and whites in the number of employment benefits received than it does in explaining wage differentials. The results indicate that men working in jobs with mostly Hispanics are less likely to receive retirement benefits and

health insurance for themselves and their family, while women working in minority dominated jobs are actually more likely to receive health insurance.

Finally, this is the first study to my knowledge to explore the potential causes of racial and ethnic job segregation. The results show that while minorities who reside in more segregated neighborhoods are significantly more likely to work in segregated jobs, those who commute longer distances to work are less likely to work in a segregated job, two findings consistent with Kain's (1968) "spatial mismatch hypothesis." I also find that blacks and Hispanics who work in larger firms are less likely to be in segregated jobs, and that English fluency and citizenship status are strongly associated with the likelihood of job segregation for Hispanics.

The remainder of this paper proceeds as follows. I begin by discussing the leading explanations for wage differentials between minorities and whites, and then provide a loose theoretical framework for the empirical work that follows. Next, I describe the data drawn from the Multi-City Study of Urban Inequality and discuss the approach used to estimate the impact of job segregation on labor market outcomes and to investigate the likely sources of racial and ethnic segregation. Finally, I report the empirical results, and then conclude with some discussion of the implications of the results.

Theoretical Framework

Most labor economists agree on the existence of wage gaps by race and ethnicity, yet often disagree on the source of the gaps. While relatively little attention has been devoted to how and why wages vary with the racial and ethnic composition of jobs,

numerous studies have put forth theories in an attempt to explain the persistence of wage differentials between whites and minorities with similar measured characteristics.

Although I do not attempt to provide an exhaustive review of all these theories, several of the alternative models are relevant to this study because they imply that discrimination will lead to segregation of minorities from whites in the labor market.³ A few of these models also shed light on the relationship between wages and labor market segregation.

Perhaps the most widely recognized theories of labor market discrimination used to explain wage gaps are Becker's (1971) taste models, which identify the prejudices of employers, consumers, and employees as sources of discrimination. The *employer* discrimination model predicts that discriminatory employers will offer to pay minorities less than equally productive whites because they incur a psychic cost associated with hiring minorities. Segregation is likely to result as discriminating employers hire mostly or only whites, depending on their taste for discrimination, and nondiscriminatory employers hire otherwise identical minorities to maximize profits. Of course, an employer's taste for discrimination may differ depending on the particular job in question. A discriminating employer, for example, may have a larger distaste for hiring a minority as a manager than for hiring a minority as a blue-collar laborer. This may

³ The *employee* taste discrimination model is not discussed here because it is not supported in the data. Widespread employee discrimination implies a wage premium for white workers who work alongside minority workers. Likewise, statistical discrimination models, which emphasize the role of imperfect information about individual worker productivity, are not addressed in this paper since they differ in their implications about the effects of racial composition (see Hirsch and Macpherson, 1994). Finally, a discussion of the theory of "*occupational crowding*" is omitted here since it is thought to explain the segregation of women from men in the labor market, and past studies do not find empirical support for the model in the context of racial segregation (see Hirsch and Schumacher, 1992; Hirsch and Macpherson, 1994). Moreover, an underlying assumption of the model is that *all* employers are discriminators. Cain (1986) provides a complete discussion of these models and their implications for racial and ethnic segregation.

explain why we often see a significant amount of racial and ethnic segregation at the job level.

The resulting wage differential between minorities and whites will depend on the shape of the distribution of employers, which reflects the extent to which they discriminate, and the size of the minority group (i.e., the supply of minority labor to a particular labor market). This theory thus explains the tendency for discriminatory pay differentials to be largest where the minority group is a greater fraction of the total workforce. The basic model (which does not allow discriminatory tastes to vary by job) also predicts that the higher and wider the employer taste distribution, the greater the extent of segregation in the labor market and the larger the wage gap between minorities and whites. At the job level, however, the prediction is less clear-cut. It is likely that the stronger an individual employer's taste for discrimination, the greater the extent of job segregation and the lower minority wages within the firm. However, a discriminatory employer may prefer minorities for some jobs (perhaps those involving servile tasks), and may actually be willing to pay minorities more than otherwise identical whites for such jobs. This implies that whites may also incur wage penalties if they work in certain jobs dominated by minorities.

The employer discrimination model is often faulted for its inability to explain wage differentials between minorities and whites that persist over time if all firms are in one competitive product market. The common argument is that nondiscriminatory firms will eventually drive the discriminators out of business due to their cost advantage. Yet this obviously does not happen since persistent wage differentials and job segregation along racial and ethnic lines are well documented (e.g., Cain, 1986). This is often

reconciled by the contention that discriminating firms may choose not to maximize money profits, but to maximize utility instead, which is affected by both profits and their taste for discrimination. It is also possible that either noncompetitive forces are at work, the forces of competition work very slowly, or there are an insufficient number of nondiscriminatory firms (Hamermesh and Rees, 1993).⁴

The theory of *consumer* discrimination may also help explain the pervasive segregation seen across the U.S. labor market and is consistent with the persistent wage differentials between minorities and whites. In this model, if the price of the labor service of a white worker is p , then the tastes for discrimination of a customer are indicated by an offer price of $p - d$ for the same service of a minority, where d measures the consumer's taste for discrimination. Employers with discriminatory customers (with $d > 0$) will only be willing to hire minorities at a lower wage, since minorities will be effectively less productive. Again, customers' tastes for discrimination are likely to differ by the particular job in question – a discriminating consumer, for example, may have little objection to a minority assembling a manufactured product behind the scenes, but be less likely to buy a car from a minority salesperson than from a white. The consumer discrimination theory predicts that minorities may segregate themselves into

⁴ Whether employer discrimination is *currently* widespread may not be the relevant issue. Some argue that the adverse effects of the long history of discrimination in our country continue to impact earnings disparities and job segregation for minorities (e.g., Darity and Mason, 1998). Prior to passage of the Civil Rights Act of 1964, racial employment exclusion was blatant. Cordero-Guzman (1990) notes that “up until the early 1960s, and particularly in the south, most blacks were systematically denied equal access to opportunities [and] in many instances, individuals with adequate credentials or skills were not, legally, allowed to apply to certain positions in firms.” Although such exclusion is now illegal, some residual amount of employer discrimination may still exist. Furthermore, minorities may have learned over time that certain jobs are effectively out of their reach, and may base their human capital accumulation and job search decisions upon this expectation. Thus employer discrimination, present or past, may still play a role in perpetuating a significant amount of racial and ethnic job segregation.

jobs without direct customer contact or into firms that sell to nondiscriminatory customers only (Cain 1986).

Although the theory of customer discrimination suggests that in competitive labor markets such segregation could prevent the long-run maintenance of wage discrimination against minority groups, there are several reasons why wage gaps may persist. Chiswick (1973) argues that inequality of wages is likely to persist if some white workers have skills complementary to the skills of black workers, citing the example of white “foremen” working with black “laborers.” Holzer and Ihlanfeldt (1998) also point out that the extent to which minorities can avoid wage penalties by segregating themselves in the workplace depends on several factors, including the relative sizes of the minority and white workforces, the relative sizes of sectors in which they do and do not face discrimination, and the production technologies of each sector.

In its purest form, however, the consumer discrimination model implies that the wage gap should be negatively correlated with the percent minority in a job. In other words, all else equal, the more effective minorities are at segregating themselves into firms with nondiscriminatory customers or into jobs without direct customer contact, the closer minority wages should be to that of whites. Yet there are many reasons why this prediction may not hold. Holzer and Ihlanfeldt (1998) discuss a number of scenarios in which the job segregation resulting from customer discrimination may lead to poorer outcomes for blacks. These explanations may apply to Hispanics as well.

For example, if there is a shortage of jobs for which minorities do not face customer discrimination and the proportion minority in a job is acting as a proxy for the supply of minority labor to a particular labor market, then wages might be negatively

correlated with percent minority. Moreover, a relative “crowding” of minorities into jobs in the nondiscriminatory sector may result in lower wages for whites as well as minorities (unless of course whites require a compensating differential to work in minority dominated jobs). Holzer and Ihlanfeldt (1998) also discuss other characteristics of product and labor markets that might cause wages of minorities and whites to be lower in minority dominated jobs. For instance, they point to Bates (1993), who finds that establishments in predominately black neighborhoods are likely to have less advanced technologies and lower capital-labor ratios than firms in white neighborhoods. They note that it is also possible that firms located in minority dominated neighborhoods will pay lower wages because their customers are likely to have lower incomes, which may lead to lower prices and product market rents.

Taking all of these factors into account, customer discrimination may result in lower wages for minorities and whites working in minority dominated jobs. This seems particularly reasonable if the nondiscriminatory sector is dominated by minority-owned businesses selling to minority customers.

Another dominant explanation for estimated wage differentials is that minorities come to the labor market with productivity shortfalls (possibly due to pre-market discrimination), and that empirical studies to date do not fully control for such skill differences. Proponents of this theory argue that omitted or unobservable differences in productivity may explain residual wage differences between races, differences that most researchers characterize as labor market discrimination.⁵

⁵ Although this skills-based explanation does not directly imply segregation, the racial and ethnic composition of jobs may be correlated with unmeasured skill differences among workers, as discussed below.

In support of this skills-deficiency hypothesis, some studies find that including the Armed Forces Qualifying Test (AFQT) score in the National Longitudinal Survey of Youth will significantly reduce racial differences in wages. For example, Neal and Johnson (1996) find that AFQT scores explain nearly three-quarters of the black-white wage gap for men (reducing the differential from -24.4 percent to -7.2 percent), and the entire black-white wage gap for women in their sample. They conclude that the earnings disadvantages young black workers face in the labor market arise mostly from obstacles they faced as children in acquiring productive human capital. In contrast, AFQT scores have been found to be much less effective in explaining differences in employment between whites and minorities.⁶

The skills-deficiency hypothesis may be particularly relevant to the job segregation issues investigated here, since the racial and ethnic composition of jobs may be interpreted as a proxy for unmeasured skill differences among workers. Hirsch and Schumacher (1992) and Hirsch and Macpherson (1994), for example, present what they call a “quality sorting” model in which the racial composition of occupations serves as a skill index for labor quality. They argue that discrimination is likely to lead to a sorting equilibrium in which higher-skilled black and white workers are sorted into higher-productivity occupations with a low proportion of blacks, and lower-skilled blacks and whites are sorted into occupations with relatively lower productivity and higher concentrations of blacks. This theory does not, however, explain the mechanisms through which such segregation occurs.

⁶ Evidence based on the AFQT has been criticized by Darity and Mason (1998), who argue that AFQT scores are not easily interpreted. They note that questions remain concerning what AFQT scores are actually measuring, and that wage differentials often reappear when additional controls (such as “self-esteem”) are added.

Empirically, Hirsch and Macpherson (1994) find the negative effect of racial occupational segregation on wages is sharply reduced when controls for occupational skills (e.g., required years of training, job tenure, computer usage) are included in a cross-sectional analysis. Moreover, when panel data are used to difference out the impact of unmeasured individual skill differences, little if any relationship is found between the racial composition of occupations and wages. The authors conclude that racial occupational segregation provides an important control for what are typically unmeasured worker quality and occupational skill differences, but should not be interpreted as a causal determinant of wages.

Since prior research finds firm- and job-level segregation to be much more significant than occupational-level segregation for minorities, it remains to be seen how important unmeasured skill differences are in explaining the impact of racial and ethnic job segregation on wage outcomes. If the “quality sorting” model accurately describes the job sorting by race and ethnicity that takes place at the firm level, then any negative effect of job segregation on wages should decline with the addition of skill controls in wage regressions. Moreover, if “quality sorting” is the *only* reason for segregation in the labor market, it implies that there should be no correlation between job segregation and wages if differences in skill level are fully controlled for.

The “spatial mismatch hypothesis” is yet another leading theory advanced to explain persistent wage gaps and employment differentials between blacks and whites, and may also help explain the significant job-level segregation found in metropolitan areas. This theory suggests that the movement of employers out of the inner-city areas toward the suburbs during the 1970s and 1980s represents a spatial shift in labor demand.

Consequently, a “mismatch” is thought to result between the locations of employers and those who continue to live in the central city, particularly minorities.

There are several likely reasons for such a mismatch between jobs and workers. The decline in highway transportation costs over the past several decades has caused many employers to choose suburban locations over the central city. Manufacturing employers, who have traditionally provided relatively high-wage jobs for low-skilled workers, have relocated to the suburbs at particularly high rates since their production technology uses a relatively high ratio of land to capital. Although residential suburbanization has also occurred, proponents of the spatial mismatch hypothesis purport that some people face more barriers in choosing their residential location. For example, discrimination in the housing market perpetuates residential segregation, and may prevent minorities from following employers out to these suburban areas. Minorities residing in the highly segregated inner-city areas may also lack the necessary transportation or information networks to obtain suburban jobs.

Yinger (1998) argues that the effects housing market discrimination are far reaching, claiming “Housing discrimination restricts the options of many black and Hispanic households and contributes to the continuing intergroup disparities in income, home ownership, wealth, education and employment (pg. 23).” Audit studies of discrimination in the housing market (both national-level studies and most smaller studies) find statistically significant levels of discrimination that are large in magnitude (Turner, et al., 1991; Turner and Mickelsons, 1992; Yinger, 1995). For example, Yinger (1995) investigates national data from the 1989 Housing Discrimination Study, and finds that black home buyers learn about 24 percent fewer houses than whites, black renters

learn about 25 percent fewer apartments, Hispanics learn about 26 percent fewer houses, and Hispanic renters learn about 11 percent fewer apartments. Overall this research finds that the incidence of housing discrimination does not appear to be abating, but that blacks and Hispanics continue to encounter discrimination in many aspects of a housing transaction: “they are told about fewer available units and must put forth considerably more effort to obtain information and to complete a transaction” (Yinger, 1998, pg. 32).⁷

Nevertheless, there are reasons why minorities may *choose* to reside in segregated neighborhoods, particularly foreign-born Hispanics. Chiswick and Miller (2001) argue that the propensity of non-English speaking immigrants to cluster in communities formed on the basis of language and ethnicity is due to the value of “ethnic goods,” which they broadly define to include conventional foods and services and social networks. They suggest that there is likely to be a compensating wage differential associated with such “ethnic goods,” and find that immigrants living in “linguistically concentrated areas” have lower earnings, even after controlling for their own language skills.⁸ Borjas (1998) explicitly studies the potential determinants of racial and ethnic residential segregation. He finds a strong negative correlation between residential segregation and both educational attainment and wages, especially among the least-skilled groups. There also seems to be a significant amount of intergenerational persistence in racial and ethnic

⁷ A hypothesis for such discrimination, which is supported in the literature, is that housing agents discriminate to protect their actual and potential business with prejudiced white households (Yinger, 1998).

⁸ Specifically, Chiswick and Miller (2001) find that earnings of foreign-born men from non-English speaking countries are lower in states with higher minority language concentrations, especially for those with greater English language fluency. They conclude that this result appears to be due to an “ethnic goods” effect, rather than to labor market crowding. That is, they argue that immigrants sort themselves across the country to equalize real incomes, and that “ethnic goods” will have a lower cost the greater the concentration of those speaking the same language. They claim that regional wage differentials may simply reflect ethnic-group specific cost-of-living differentials, rather than a “crowding effect.” However, the fact that linguistic concentrations are measured at the state level (not at the neighborhood level), and

residential segregation. Borjas (1998) concludes that “persons in the least skilled groups wish to move to neighborhoods where they can benefit from contact with highly skilled groups, while persons in the most skilled groups want to segregate themselves into wealthier enclaves” (p. 229).

Empirically, the spatial mismatch literature focuses on the effects of ‘mismatch’ on employment, wage, and earnings outcomes for blacks. Holzer (1991) provides a good overview of this literature, and concludes that blacks in inner-city areas have less access to employment than blacks or whites in the suburbs, and that unlike most other groups of workers, less-educated blacks face higher wages in the suburbs than in the central city. Providing more recent evidence of spatial mismatch, Stoll et al. (1999) find that while less-educated people tend to reside in areas with high minority populations, low-skill jobs are scarce in these areas. On the other hand, the availability of such jobs relative to less-educated people in predominantly white suburban areas is high. Cutler and Glaeser (1997) examine the impact of residential segregation (a common measure of ‘spatial mismatch’) on several outcomes for blacks, and find that blacks in more segregated cities have lower high school graduation rates, lower income, are more likely to be “idle” (neither in school or working) and more likely to become single mothers.

In terms of wage effects, the spatial mismatch hypothesis can be thought of as a variant of the “crowding” hypothesis. It suggests that since minorities tend to be residentially segregated in the central cities, and face greater challenges securing jobs in the suburbs, they are more likely to be crowded into jobs within the central city as well.⁹

several important controls (including industry and occupation) are omitted from the earnings regressions, makes these results less relevant to the job segregation and neighborhood issues addressed in this study.

⁹The job segregation effects of mismatch problems may be even more apparent if we think of spatial mismatch coupled with certain types of discrimination. For example, a discriminatory employer may

Spatial mismatch implies that minorities will receive lower wages than whites not due to employment discrimination per se (unless, of course, employers are relocating to the suburbs to gain access to a “whiter” workforce), but due to its impact on local labor supply and demand. Minorities living and working in the central city face lower labor demand as a result of firm relocation, and a relatively high supply of labor due to barriers they face in following jobs out to the suburbs. Spatial mismatch also suggests that whites competing directly against minorities for the fewer inner-city jobs should receive lower wages as well. The theory thus predicts a negative correlation between racial and ethnic job segregation and wages for both minorities and whites.

The theories I have presented thus far to explain wage differentials and job segregation are all related to some form of discrimination (labor market, housing market or pre-market discrimination). It is also possible, however, that personal choice plays a role. Blacks and Hispanics may choose to apply for jobs in which they are likely to work with others of their own race/ethnicity, perhaps under the expectation that they will have more in common with coethnics. Minorities may also choose to work in segregated jobs due to language barriers, a factor that is likely to be particularly important for Hispanics with little or no English. Moreover, minorities that self-select into segregated jobs may be willing to pay a compensating differential to do so.¹⁰ Such self-selection is likely to

prefer to hire whites, but if the firm is located in the central city, the majority of job applicants are likely to be minorities. Such an employer may be thus forced to hire more minorities than desired, but may compensate by hiring them into the most menial positions within the firm.

¹⁰ Since whites “dominate” the U.S. labor market (and society, for that matter), this idea of self selection is not thought to apply to them (i.e., they will not accept lower wages to work with mostly whites, since that is the likely outcome). I am also assuming that whites do not discriminate against minorities by demanding higher wages to work in minority dominated jobs (as the “employee discrimination” model assumes). Thus, the self-selection theory suggested here implies that whites that work with mostly minorities should earn more than the minority group in question, but not necessarily more than whites that work with mostly whites (Table 2 below supports these predictions). This is a slightly different result than that implied by the

result in the same outcomes as the discrimination models predict – racial and ethnic wage differentials and job segregation – but the policy implications of self-selection are vastly different from those implied by discrimination.

Overall, it is likely that no one dominant force is solely responsible for perpetuating wage differentials and job segregation, but that several of the forces described by the above theories play a limited, and possibly related role. For example, employer discrimination may exacerbate problems with spatial mismatch, particularly if firms are relocating to the suburbs in part to gain access to a “whiter” workforce. Holzer (1996) finds evidence that employer discrimination is greater in the suburbs than in the central city, since he finds that the ratio of new hires to job applications from blacks is lower in suburban firms than central-city firms. This is particularly troubling since he also argues that the skill needs in suburban firms are generally lower, and the relative skills of black applicants in the suburbs are likely higher. Consumer discrimination may also be related to spatial mismatch, since, as Kain (1968) argues, customer discrimination may contribute to the failure of inner-city blacks to follow jobs out to the suburbs.

Empirical Implications

Although I do not attempt to formally test any of these leading theories, they do provide a loose theoretical framework for the following empirical work that investigates the determinants of job segregation.¹¹ For example, if *employers* have a taste for discrimination against minorities as Becker’s model suggests, we may expect to find

employee discrimination model, which predicts that whites require a compensating differential to work with minorities.

¹¹ Previous studies that have attempted to test the importance of the above explanations for minority-white wage gaps have resulted in few, if any, firm conclusions. Cain (1986), who provides a review of several studies that test the discrimination hypotheses, concludes that these mixed results are due to the fact that the theories often yield ambiguous predictions. He goes on to describe several of the difficulties in testing the hypotheses of discrimination theories.

certain relationships between the likelihood of job segregation and other variables, such as region and firm size. It is well known that racial prejudice has historically been more pervasive in the South, while ethnic tensions and discrimination against Hispanics may be more pervasive in the Los Angeles area due to the large influx of Latino immigrants over the past few decades (see, for example, Ortiz, 1996). Thus a strong relationship between metropolitan area (a proxy for region) and the likelihood of job segregation for minorities may be indicative of employer discrimination (such a relationship would also support the consumer discrimination model if customers and employers share regional tastes). Similarly, we may expect to find a negative relationship between firm size and job segregation if employer discrimination is a factor. Larger firms are more likely to have a formal human resource department (or even a legal department) and be more aware of the legal consequences of employment discrimination. Moreover, Affirmative Action enforcement is likely to be stronger for larger firms.¹² Controls for firm size and metropolitan area are therefore included in the following analysis of job segregation to shed light on whether employer discrimination is a likely determinant of job segregation for minorities.

The *consumer* discrimination model suggests an empirical approach in which to explore the likelihood that buyers of labor services may help perpetuate labor market disparities between whites and minorities. I explore the impact of potential customer discrimination on wages and the likelihood that minorities work in segregated jobs using an indicator of face-to-face contact with consumers as a proxy for the potential of customer discrimination. The theory predicts that minorities who face customer

¹² Firms with fewer than fifty employees are not subject to Affirmative Action laws. However, the effect of Affirmative Action on segregation should be present whether discrimination originates from employer or

discrimination will receive lower wages than otherwise identical whites.¹³ We may also expect to see more job segregation among minority groups for whom customer discrimination is more pervasive, as they attempt to avoid the associated wage penalties. If a sufficient amount of job mobility is possible, minorities working in jobs with direct customer contact may be interpreted as those who do not face discriminatory customers and therefore we should expect to find a positive relationship between the indicator of customer contact and segregation into jobs with other minorities.

Finally, I investigate the possible impact of “spatial mismatch” on job segregation. It is well known that there is a significant amount of residential segregation between whites and minorities, with minorities often concentrated in the inner-city areas. Over the past few decades there has also been considerable movement of firms and jobs from central-city areas to the suburbs. Evidence suggests that inner-city job access has declined over time for low-skilled workers (Ihlanfeldt and Sjoquist, 1990). Past studies also find residential segregation (a proxy for spatial mismatch) to be negatively correlated with employment and income for blacks. Spatial mismatch problems further imply that minorities living in more segregated neighborhoods should be more likely to work in segregated jobs. However, if minorities can commute longer distances to follow the jobs out to the suburbs, they might be able to avoid the wage penalties associated with job segregation.

consumer prejudice.

¹³ Unfortunately, the data used here do not contain a measure of customers’ racial and ethnic composition, which may provide a more direct test of the potential for customer discrimination since one would expect less discrimination by customers of one’s same race or ethnicity. Holzer and Ihlanfeldt (1998) provide convincing evidence of customer discrimination in metropolitan areas using the employer-side counterpart to the household data used here. They find that the larger the fraction of minority customers, the higher the probability that workers from the same minority group will be hired. Their results are strongest for jobs with significant contact with customers.

I therefore examine the impact of residential segregation and average commute times on the probability that minorities work in a segregated job to assess the significance of spatial mismatch. If spatial mismatch plays a role in perpetuating racial and ethnic segregation in the labor market, residential segregation will be positively related to the likelihood of job segregation for minorities. On the other hand, holding constant residential segregation, commute time and job segregation will be negatively related. Finally, the effect of residential segregation on job segregation should decline with commute time.¹⁴

A few qualifying comments are in order here. First, it is important to note that neither residential segregation nor commute time provides a perfect measure of the degree of spatial mismatch. Residential segregation really only tells one side of the story – that the residences of minorities tend to be concentrated in certain areas within a metropolis, usually the central city. It tells us nothing about firm location (although the fact that firms have relocated to the suburbs in large numbers is well established). Furthermore, residential segregation may have negative effects on minority outcomes above and beyond the wage and employment effects of spatial mismatch. For example, Cutler and Glaeser (1997) find that residential segregation negatively affects schooling and family structure outcomes for blacks, even after controlling for average relative commute time. Commute time, on the other hand, should give an indication of the distance between residence and job location. But commute time is only observed for those who actually find jobs, and thus a measure of average commute for an area is likely

¹⁴ The theory also predicts wages to be negatively related to residential segregation (which is verified below for all demographic groups but Hispanic women), but positively correlated with commute time, since most workers require a compensating differential to commute longer distances. Although not reported, commute

to understate the true distance between workers and jobs (i.e., the degree of spatial mismatch), particularly for minorities. Despite these shortcomings, however, measures of residential segregation and commute time are reasonable proxies for spatial mismatch and are likely to be related to job segregation as discussed above if mismatch is a contributing factor to the segregation of minorities from whites in the labor market.

The remaining two explanations for wage differentials and job segregation that do not necessarily suggest labor market discrimination – the self-selection explanation and the skills-deficiency hypothesis – are difficult to test either formally or informally given the data used here. Ideally, one would use longitudinal data to difference out any unmeasured skill and taste differences among workers that may be correlated with job segregation. Difficulties associated with possible bias resulting from such unobserved heterogeneity, which may affect the following empirical results, are discussed and addressed below in the subsection of the Results section entitled “Potential Econometric Problems.”

Data and Empirical Framework

The Data Set

I explore these issues using a relatively new data set drawn from the Multi-City Study of Urban Inequality (MCSUI). The MCSUI survey was administered to adult household residents in Atlanta, Boston and Los Angeles.¹⁵ Interviewing was completed in the summer of 1993 in Atlanta, and in the summer of 1994 in Boston and Los

time was found to be largely insignificant in wage equations using these data. In equations for white women, however, commute time was verified to be positively related to wages.

¹⁵ The survey was also administered in Detroit, however the Detroit data is not analyzed here because it does not contain information on job segregation.

Angeles.¹⁶ Data from the 1990 Census at the block and group level were merged into the MCSUI data set in order to control for residential segregation.

The MCSUI survey consisted of a probability sample of households, stratified by race-ethnicity and poverty-status composition of the 1990 Census. Blacks were oversampled to yield roughly equal numbers of whites and blacks in all locations; Latinos and Asians were similarly oversampled in Los Angeles, as were Asians in Boston. In addition, concentrated poverty areas were oversampled in all metropolitan areas. The project also used a multistage sampling procedure, utilizing cluster sampling with three levels of clustering. This process generated a total of 7,373 observations – 1,528 in Atlanta, 1,820 in Boston, and 4,025 in Los Angeles. Restricting the sample to non-retired respondents reduces the sample to 6,388 observations. The sample was further restricted to include only whites, blacks and Hispanics (omitting Asians and other respondents due to limited observations on these individuals) with usable wage information.¹⁷ These additional sample restrictions reduced the full sample used in the analyses that follow to 3,895 observations – 1,260 on whites, 1,392 on blacks, and 1,243 on Hispanics (some estimates are based on fewer observations due to missing values).

One goal of the Multi-City Study of Urban Inequality was to test hypotheses concerning the status of women and minorities in urban labor markets, making this data

¹⁶ The U.S. economy was recovering from the recession of the early 1990s when the survey was administered. Monthly unemployment rates during this period averaged under six percent in Atlanta and Boston, and under 10 percent in Los Angeles. To control for differences in local labor market conditions, dummy variables for metropolitan area are included in the regression analyses that follow.

¹⁷ Please note that three observations were dropped due to missing sex information. Hourly wages below \$2 and exceeding \$200 were classified as outliers. In defense of this trimming technique, Angrist and Krueger (1998) found that "extreme wage values are likely to be mistakes," when they investigated the impact of trimming outliers using CPS data. In addition, a visual inspection of cases for which respondents reported hourly wages between \$100 and \$200 was conducted for reasonableness (e.g., verification that reported family income was consistent with these wage levels), and resulted in the classification of one additional outlier.

set particularly appropriate to address the issues discussed above. These data provide a rich source of information on labor market histories, including an extensive collection of variables to control for differences in human capital and job characteristics. For example, MCSUI provides measures of past work experience related to one's current job, specific job tasks performed, and information on the racial/ethnic composition of the job.

Measures are taken in the following analyses to ensure that the data can be used to draw inferences regarding the underlying metropolitan populations. First, analysis weights for respondents are used, which were set inversely proportional to the household sampling weight. Analysis weights also reflect nonresponse (if nonresponse is not uniformly distributed) and the number of persons eligible for interview in the respective household. Second, in all analyses robust standard errors are calculated that are also adjusted for the clustering and stratification of the survey design.¹⁸

A shortcoming of these data concerns the size of the sample and whether it is representative of the U.S. labor market in general. The empirical estimates arrived at in this study probably provide good first approximations of the impact of racial job segregation on blacks in metropolitan areas. However, since estimates of the impact of ethnic job segregation are driven primarily by observations on Hispanics in the Los Angeles area, the same cannot be said for Hispanics. At best, empirical results on ethnic segregation may only be representative of metropolitan areas with relatively high concentrations of Hispanics.

Measuring Segregation

Job segregation is measured by a series of dummy variables created from a survey question asking "What (is/was) the race and ethnicity of most of the employees doing the

kind of work you (do/did) at this location?” The potential survey responses to this question are non-Hispanic white, non-Hispanic black, Hispanic, Asian, mixed racial group (in Atlanta and Boston only), or “other.” A series of five dummy variables were created from the responses to the job segregation question, combining the mixed-racial-group response with “other.” Although the survey question does not refer to a specific level of occupational disaggregation, the resulting segregation measure is likely to capture whether a fairly detailed occupation within an establishment is segregated by race or ethnicity.

Throughout this paper I refer to the segregation measure as an indication of “job segregation.” However, since these data do not allow me to explicitly control for establishment-level segregation, the segregation measure used here probably captures the impact of segregation at the establishment level as well as at the job level.¹⁹ The potential determinants of segregation at the establishment level are likely to be similar to those affecting job-level segregation, and include employer and consumer discrimination (i.e., discrimination in job hiring at the establishment due to either employer or customer prejudice), “quality sorting,” or personal choice. Spatial mismatch, on the other hand, is

¹⁸ This was accomplished using Stata survey (svy) commands (see StataCorp, 1997, pp. 305-312).

¹⁹ The inclusion of establishment-level controls for firm size and industry may reduce this possibility if these controls pick up dimensions of racial and ethnic segregation at the firm level. Furthermore, lack of data on establishment-level segregation may actually imply that the following estimates of the job segregation effect are biased towards zero. Bayard et al. (1999) find that segregation at the establishment level is associated with higher wages and thus reduces the estimated wage gap for minorities. This seems counterintuitive, but perhaps their establishment measures are picking up wage effects from minority owned and operated businesses. In contrast to the findings of Bayard et al. (1999), Carrington and Troske (1998) find little establishment-level segregation – that within metropolitan areas the interfirm distribution of black and white workers is close to what would be suggested by random assignment. Their findings imply that little bias is likely to result from the omission of establishment-level segregation data.

more likely to impact segregation at the establishment level than at the job level, unless it is coupled with customer or employer discrimination.²⁰

Empirical Methods

I first investigate the magnitude of racial and ethnic wage gaps for men and women using OLS estimates of log wage regressions of the following general form:

$$(1) \ln(W) = \beta_0 + \beta_1 BLACK + \beta_2 HISP + HC\beta_3 + PERSON\beta_4 + INSTIT\beta_5 + JOB\beta_6 + \epsilon,$$

where W is the hourly wage, $BLACK$ is a dummy variable equal to one if the individual is black, and $HISP$ is a dummy variable equal to one if the individual is Hispanic. HC is a vector of human capital controls including educational attainment, age, previous experience doing similar work and job tenure. In some specifications, HC also includes measures of English fluency and U.S. citizenship, which may be related to productivity.²¹ $PERSON$ includes personal controls for whether the individual is married and number of children under 18. $INSTIT$ contains institutional controls for firm size and whether the individual is covered under a collective bargaining agreement. JOB controls for job characteristics such as whether the individual works part time (less than 35 hours a week), whether they have the authority to supervise others and, if so, whether they have the ability to set the pay of those they supervise. JOB also contains dummy variables indicating whether specific job tasks are performed on a daily basis, including talking

²⁰ In the empirical results that follow, however, spatial mismatch should have a similar impact on the job segregation measure used here as it would on a measure of establishment segregation, if such a measure were available, since the segregation question refers to one's job at a specific location.

²¹ Trejo (1997) finds that English language deficiencies are an important source of lower earnings for Mexican-Americans. Moreover, U.S. citizenship may independently affect labor market outcomes for Hispanics, whether or not they are legally permitted to work.

with customers face to face, talking with customers on the telephone, reading paragraphs, writing paragraphs, using a computer and doing arithmetic. Finally, controls for industry and occupation are included in some specifications.

The impact of racial and ethnic segregation is then examined by adding the controls for job segregation (the categorical variable indicating whether one works with mostly whites, blacks, Hispanics, Asians or “other”). I estimate these wage equations by race/ethnicity and sex to allow the effects of job segregation, and other characteristics, to vary by demographic group.²²

In order to assess the impact of race and ethnicity on the number of job benefits received, I specify ordered logit equations for men and women of the following form:

$$(2) \quad BEN = \alpha_0 + \alpha_1 BLACK + \alpha_2 HISP + HC\alpha_3 + PERSON\alpha_4 + INSTIT\alpha_5 + JOB\alpha_6 + u,$$

where *BEN* is a variable ranging from 0 to 4 based on the total number of four employment benefits an individual receives through their job: health insurance for themselves, health insurance for their family, a retirement plan, and/or paid sick leave.

The independent variables correspond to those in equation (1). The job segregation

²² Some of the variables discussed above meant to serve as proxies for productivity, such as supervisory authority and the responsibility of specific job tasks, may be affected by discrimination. For example, if minorities do not have the same opportunity for attaining valuable control over supervisory authority and the ability to set pay as do whites with the same qualifications, then specifications that include these variables will “over control,” and underestimate discrimination. Minorities may also have unequal access to jobs associated with certain job tasks, such as jobs working with computers. In addition, like women, minorities may be segregated into lower-paying industries and occupations. Consequently, the job tasks, supervisory authority, industry and occupation variables are not included in all specifications. Two other sets of variables that are only included in later specifications are the controls for English fluency and U.S. citizenship. While these variables are likely to be related to productivity, there is also the strong possibility of discrimination based on differences in language or citizenship.

controls are then added to equation (2) to evaluate the impact of racial and ethnic job segregation on the number of benefits received.

Equation (2) is then run as a probit equation with *BEN* respecified as a dummy variable for each of the four employment benefits, indicating whether the individual receives the benefit in question. That is, separate probit equations are run to determine whether job segregation affects the probability of receiving each of the four benefits (as opposed to the number of job benefits received), since job segregation may affect the likelihood of receiving the various benefits differently.

Lastly, I explore the potential causes of job segregation. While equations (1) and (2) follow from the basic human capital model, there are many potential reasons why minorities are likely to work in highly-segregated jobs. Although I do not explicitly model the process of job segregation, the theories presented above provide some direction for choosing likely determinants. For example, customer discrimination and the “spatial mismatch hypothesis” are two theories that may help explain racial and ethnic segregation in the labor market. The consumer discrimination theory suggests that minorities who work in jobs with direct face-to-face customer contact may segregate themselves into firms that sell to nondiscriminatory customers only in order to avoid potential wage penalties associated with such discrimination. Similarly, spatial mismatch suggests that minorities who live in highly-segregated, inner-city areas, without the necessary means to follow the movement of jobs to the suburbs, may be segregated into lower-paying jobs. If these models accurately characterize the segregation process, measures of residential segregation, commute time and customer contact are likely to be correlated with job segregation.

Hirsch and Macpherson's (1994) "quality sorting" theory implies that higher-skilled workers will be sorted into occupations with a low proportion of minorities, while lower-skilled workers are sorted into occupations with higher concentrations of minorities. Thus if some sort of "quality sorting" plays a role, human capital levels are likely to be important determinants of job segregation. Finally, as discussed above, metropolitan area and firm size may capture the effects of employer discrimination on job segregation.

To more formally explore the potential determinants of racial and ethnic segregation in the labor market, I estimate probit equations for blacks and Hispanics of the following form:

$$(3) \text{ } JOBSEG = \theta_0 + \theta_1 RESSEG + \theta_2 COMM + \theta_3 CUS + HC\theta_4 + INSTIT\theta_5 + CITY\theta_6 + \theta_7 MALE + \eta,$$

where *JOBSEG* is a dummy variable equal to one if the individual is black and works with mostly blacks or Hispanic and works with mostly Hispanics (whites are omitted from this analysis). *RESSEG* measures residential segregation in one's census tract as the proportion black if the respondent is black, and as the proportion Hispanic if the respondent is Hispanic. *COMM* is a measure of average commute time in minutes and *CUS* is a dummy equal to one if the individual deals with customers or clients face to face on a daily basis. Due to sample size concerns, men and women are pooled together, and *MALE* is included to control for sex. The other variables are as defined above.

Results

Table 1 reports descriptive statistics for white, black and Hispanic male and female workers in MCSUI. The data on average log hourly wages show that racial and ethnic differences are larger for men (-0.28 for blacks and -0.58 for Hispanics) than for women (-0.20 for blacks and -0.54 for Hispanics), although the differences in wage gaps between the sexes in MCSUI are much more modest than those found in other data (see, for example, Cain, 1986 and Bayard, et al, 1999). The data on the total number of employment benefits received (of 4 possible) indicate that racial differences are slightly larger for men than for women, but the ethnic differences are virtually identical by sex. In general, however, blacks are close to parity with whites in terms of the level of employment benefits, while Hispanics receive significantly fewer employment benefits through their jobs.

Whites in the sample are slightly older, more likely to have higher educational degrees, have more tenure and previous experience than minorities, all of which are associated with higher wages and benefits (although the difference in tenure between white women and black women is not significant).²³ Minority men are more likely than white men to work part time, whereas white women are more likely than minority women to work part time. This may partly explain the larger unadjusted wage differences among men compared to women.

As indicated by Table 1, a significant proportion of Hispanics do not speak English “very well” (65.8 percent of Hispanic men and 62.1 percent of Hispanic women). Notably, a rather large proportion of Hispanics are not U.S. citizens, which may coincide

²³ Unless otherwise noted, all differences discussed in this section are (at least) significant at the 10-percent level.

with the large representation of Hispanics from the Los Angeles area (a region known for a relatively large amount of immigration from Mexico). The descriptive statistics also reveal that Hispanics work in significantly smaller establishments, while blacks tend to work in larger establishments (although not significant in these data, this latter result is explored by Holzer, 1997).

In terms of job tasks performed on a daily basis, Table 1 shows that whites are significantly more likely than both minority groups to use a computer on the job, a characteristic known to be associated with higher wages. White men are also more likely than black men to write paragraphs, while white women are more likely than black women to do arithmetic on a daily basis. Whites of both sexes are significantly more likely than Hispanics to perform virtually all of the remaining tasks on a regular basis, and to work in jobs with supervisory authority. Finally, white women are more likely than minority women to have jobs with the ability to set the pay of others.

Table 1 also reports descriptive statistics on segregation by race and ethnicity. A remarkable degree of job segregation for minorities is revealed when investigating the survey question that asks about the race/ethnicity of most employees doing similar work. Blacks are most likely to report that the majority of their coworkers are also black, with 36 percent of black men and 41 percent of black women working in predominantly black jobs. This represents a significant amount of job segregation considering the relatively small proportion of blacks in the labor markets of these metropolitan areas, with blacks accounting for only 13.2 percent of the overall sample of workers. Perhaps the most striking finding is the degree of segregation among Hispanics, with 72 percent of men and 69 percent of women working in jobs with predominantly Hispanic coworkers.

Comparatively, Hispanics comprise only 21.3 percent of the sample of workers. Finally, more than 80 percent of whites work in jobs dominated by whites, while whites make up 61.7 percent of the overall workforce in the three-city sample.

The figures reveal comparatively less racial and ethnic segregation along industry and occupation lines. There are no striking relationships in terms of the distribution of workers by the eight broad categories of industries used here, with the exception of Hispanics being less likely to work in the service industry than whites and blacks. A notable relationship does appear, however, when looking at the distribution of workers by occupation: whites are significantly more likely to be in managerial, professional or technical positions than minorities. Among men, 51 percent of whites are in these higher paying positions, compared to only 28 and 14 percent of blacks and Hispanics, respectively. Similarly, 45 percent of white women are in managerial, professional or technical positions, compared to only 18 percent of black women and 14 percent of Hispanic women. Blacks and Hispanics are more likely to be in lower-paying service and labor occupations than whites, and Hispanics are significantly more likely to be in crafts/operative positions.

Table 2 presents mean hourly wages (in panel A) and mean log wages (in panel B) for the six demographic groups, by the race and ethnicity of the majority of one's coworkers. The table also reports median wages in brackets to avoid having results driven by tails in highly skewed distributions (this is especially important given the small sample sizes for some cells). For example, comparing mean hourly wages of black males working with mostly whites (\$15.56) to that of those working with mostly blacks (\$14.17) suggests an unadjusted wage differential of just under 10 percent. However,

performing a similar comparison using median hourly wages or log wages (mean or median) indicates that black males segregated into jobs with mostly blacks earn more than 30 percent less than those working with mostly whites.²⁴ A cross-wise comparison using log wages in panel B reveals that the unadjusted wage gap between black and white men working with mostly whites is 17 percent, while it is 26 percent for those working in jobs with mostly blacks.

Overall, the comparison of wages by race/ethnicity of coworkers in Table 2 reveals an interesting relationship: Blacks and Hispanics seem to incur the largest wage penalties when they are segregated into jobs dominated with other blacks and Hispanics, respectively. In other words, blacks earn the least when they work in jobs dominated by blacks, and Hispanics earn the least when they work in jobs dominated by Hispanics. This result holds for both men and women. White men, on the other hand, earn the most when they work in jobs with predominantly white coworkers. Although it is rare for whites of either sex to work in jobs with mostly Hispanics (as seen in Table 1), those that do earn significantly less than those that work in “white jobs.” Comparatively, the small fraction of blacks that work with mostly Hispanics seem to fare quite well, as do Hispanics who work with mostly blacks.

How Job Segregation Affects Wages

Table 3 reports baseline OLS log wage regressions for men and women that describe the relationships between the variables listed in Table 1. Columns (1) and (4) include controls for demographic group; personal controls for marital status and number

²⁴ A visual inspection of certain characteristics of high wage earners (such as education, occupation and reported family income) was performed to assure the reasonableness of these observations. The inspection resulted in a decision not to omit any additional high-wage observations from the sample other than those noted in footnote #17.

of children under eighteen; basic human capital controls for age, educational attainment (categorized as the highest degree earned), prior experience doing similar work and tenure with firm; and institutional and job controls for part-time work, firm size, and collective bargaining coverage. The estimated coefficients on these variables are consistent with human capital theory, with positive returns to age (a proxy for general human capital), educational attainment, job-specific experience and tenure, while those in part-time jobs receive lower wages.²⁵ It is interesting to note that relative to high-school dropouts, women in the sample earn higher returns for all levels of educational attainment than men earn. Working in a larger firm benefits women in the sample, yet firm size is insignificant for men. For both sexes, the unadjusted black-white differential indicated in Table 1 falls by almost half after controlling for these basic characteristics. For men, the Hispanic-white differential falls by more than one-third, while the estimated Hispanic-white differential for women falls by more than one-half. Overall, the wage gaps remain larger for men.

In columns (2) and (5) I add controls for English fluency and citizenship. Although these variables may account for some productivity differences, it is important to keep in mind that there may also be discrimination based on one's ability to speak English and citizenship. The addition of these controls reduces the Hispanic-white wage differential for men from -0.353 to -0.299, and considerably reduces the adjusted Hispanic-white wage gap for women, which falls from a significant -0.223 to an

²⁵ Tests allowing the returns to a high-school diploma to differ from returns to a GED revealed that for men, returns to a GED were generally lower but that the GED dummy was consistently insignificant in the male wage models. All else equal, women with a GED appeared to earn higher wages than those with a high-school diploma, however only 2 percent of the sample reported having a GED. More importantly, including a separate dummy variable for a GED did not affect the estimated coefficients on the race/ethnicity dummies or on the job segregation dummies in any of the wage models.

insignificant -0.096. Relative to being able to speak English “very well,” a lack of English fluency seems to have a significant negative impact on wages. While the associated wage penalties are fairly consistent across other levels of fluency for men, women’s wages are not significantly penalized for speaking English less than “very well,” but are penalized to a greater extent than men’s for not being able to speak English at all. Likewise, women experience a much greater wage disadvantage than men for not being a U.S. citizen by birth (in fact, men’s wages do not appear to be penalized at all for not being a U.S. citizen by birth). The dramatic differences in these estimated returns by sex calls into question the contention that English fluency and citizenship are only picking up productivity differences and are not subject to discrimination. Perhaps Hispanic women face some sort of “double jeopardy” in the labor market, and are discriminated against for lack of citizenship and the ability to speak English to a greater extent than men. It is also possible that Hispanic women without U.S. citizenship and the ability to speak English do not have the social networks necessary to find well-paying jobs, whereas similar Hispanic men have better network systems.

Columns (3) and (6) include controls for daily job tasks, supervisory authority, and industry and occupation. Since these characteristics may themselves be affected by discrimination, these results should be interpreted with some care. For men, the inclusion of these controls reduces the racial and ethnic wage differentials considerably (from -0.144 to -0.078 for blacks and from -0.299 to -0.176 for Hispanics).²⁶ Working with a computer on a daily basis and having supervisory authority to set pay are associated with

²⁶ Controls for industry and occupation alone explain a significant amount of the wage gaps. When log wages are regressed on racial and ethnic dummies with the addition of industry and occupation controls only, the black-white wage gap is -.137 for men and -.158 for women, while the Hispanic-white wage gap is -.361 for men and -.313 for women.

higher wages for men. For women, the inclusion of these controls reduces the black-white wage gap from -0.119 to -0.086, while the Hispanic-white wage differential is reduced to an insignificant -0.025. Having supervisory authority to set pay and working with a computer on a daily basis are also associated with higher wages for women, as are the daily tasks of writing paragraphs and talking with customers on the phone. Having face-to-face customer contact on daily basis, however, is associated with lower wages for women.

The impact of job segregation on wages is now investigated to better understand the sources of racial and ethnic differences in wages. Table 4 reports results of wage regressions corresponding to the columns used in Table 3, but with the inclusion of job-level segregation controls. The results indicate that men working in jobs with predominantly black or Hispanic coworkers earn less than those working with mostly whites. Based on columns (1) and (2), men who work with mostly blacks earn about 13 percent less, and men who work with mostly Hispanics earn 17-21 percent less, compared to those who work in “white jobs.” Column (3), which includes control for daily job tasks, supervisory authority, industry and occupation, estimates the wage disadvantage associated with working with mostly blacks or Hispanics at roughly 11 percent.

While the included productivity proxies were shown to explain a significant amount of the unadjusted racial and ethnic wage gaps in Table 3, it seems that job segregation has some additional explanatory power. Columns (1) and (2) of Table 4 indicate that the black-white wage gap for men is reduced from roughly -0.14 to approximately -0.09 when I control for the effects of job segregation. The Hispanic-

white wage gap for men is reduced by more than one-quarter, but remains fairly large at roughly -0.22. When controls for daily job tasks, supervisory authority, industry and occupation are included in column (3), the black-white wage gap for men is reduced to -0.05 (a difference that is statistically insignificant). The Hispanic-white wage gap for men is also reduced considerably with the addition of these controls, to roughly -0.13.

Like men, women also experience wage disadvantages associated with working in jobs with predominantly black or Hispanic coworkers. Based on columns (4) and (5), women who work with mostly blacks earn about 10 percent less, and those who work with mostly Hispanics earn 20-23 percent less than women who work with mostly whites. Column (6) for women estimates the wage disadvantage associated with working with mostly blacks at 9 percent, and the disadvantage associated with working with mostly Hispanics at roughly 12 percent.

The inclusion of controls for job segregation reduces the black-white wage differential for women from a significant -0.12 to about -0.07 (an estimate that is not quite significant), based on columns (4) and (5). When controls for daily job tasks, supervisory authority, industry and occupation are also included in column (6), the racial wage gap for women falls further, to an insignificant -.05. The job segregation controls reduce the Hispanic-white wage gap by more than half to roughly -0.10 based on column (4). When controls are added for English fluency and citizenship in column (5), there is no significant difference in wages between Hispanic and white women.

Overall, the results of Table 4 seem to provide some evidence of “quality sorting” at the job level. The inclusion of additional personal and job characteristics (particularly the controls for daily job tasks, supervisory authority, industry and occupation) is

associated with a reduction in the estimated impact of job segregation, especially for those working with Hispanics.

Next I allow the estimated effects of job segregation on wages to differ by race and Hispanic ethnicity. Table 5 reports regression results by demographic group, with Table 5a containing the results for blacks, and the results for Hispanics and whites in Tables 5b and 5c, respectively. The controls included in the first column under each demographic-group heading correspond to those in column (1) of Table 3, while the controls included in the second column for each demographic group are the same as those in column (3) of Table 3. Note that the English fluency and citizenship controls are included only in the regressions for Hispanics.

Overall, Table 5 confirms the general result we saw in Table 2: minorities who are segregated into jobs with other minorities of the same race/ethnicity earn less than those who work in jobs with predominantly white coworkers. Table 5a shows that black men whose jobs are racially segregated seem to be penalized more than black women who are segregated. Black men who work with mostly blacks earn 14-19 percent less than otherwise identical black men who work with mostly whites, while black women who are racially segregated earn about 9 percent less than those who work with mostly whites.

Turning to Table 5b, the results for Hispanics indicate that ethnic job segregation is associated with wages that are almost 15 percent lower for Hispanic women, compared to women with the same characteristics who work in jobs with mostly whites. For Hispanic men, column (1) suggests that ethnic job segregation is associated with wages that are almost 20 percent lower. However, when controls for English fluency,

citizenship, job tasks, supervisory authority, industry and occupation are added in column (2), the effect of ethnic job segregation on wages is cut in half, indicating that Hispanic men who work with mostly Hispanics earn 10 percent less than those that work with mostly whites (a difference that is not statistically significant). Table 5b also seems to suggest that Hispanic men who work in jobs dominated by blacks are penalized considerably, earning over 30 percent less than those working in “white jobs.” However, these estimates are based on differences across small cells.²⁷

Table 5c, which contains the results for whites, indicates that once I control for differences in basic human capital and personal characteristics, the wage disadvantages that white men seemed to incur when working with minorities (based on Table 2 results) disappear. On the other hand, white women that work in jobs dominated by Hispanics appear to earn significantly less, even after including the full set of controls.

Keeping in mind the wage implications of the various models discussed in the theory section, I am now in a position to evaluate the overall findings of Table 5 (a more complete discussion of the merits of these models follows below in the subsection assessing the likely determinants of job segregation). Although no firm conclusions can be drawn since several of the models do not provide clear predictions, the finding that the considerable wage disadvantage white men experience when working in minority dominated jobs disappears once differences in basic productivity measures are eliminated lends support to the hypothesis that some sort of “quality sorting” may take place at the

²⁷ Table 5 indicates that the only demographic group for whom face-to-face customer contact seems to affect wages is black women. As mentioned in an earlier footnote, a better measure of the possibility of consumer discrimination would be an indication of the proportion of minorities living in the area where the job is located. Short of that, in regressions not shown here, residential segregation measures were interacted with the face-to-face dummy to determine if the wage effect of customer contact depends on the proportion minority in one’s neighborhood. In all specifications, this interaction variable was insignificant and had little impact on the face-to-face dummy.

job level for white men. However, even after including the full set of controls, racial and ethnic job segregation is associated with lower wages for white women and minorities. These findings are consistent with predictions made by the spatial mismatch hypothesis and the two discrimination models, but suggest that the quality sorting model alone cannot explain racial and ethnic job segregation for these groups of workers (unless the empirical specifications do not adequately control for skill differences). The results of Table 5 also provide some support for the “personal choice” model – the idea that minorities may choose to work with coethnics and accept lower wages to do so. However, this model does not explain why the wages of white women should be penalized for working with Hispanics, or why Hispanic males should experience a wage disadvantage for working with mostly blacks. Overall, Table 5 suggests that not only is it likely that several forces influence the amount of job segregation in the labor market, but that these forces may affect the different demographic groups to varying degrees.

Does Job Segregation Affect Benefits?

The results presented above provide compelling evidence that racial and ethnic segregation at the job level has a detrimental impact on the wages of minorities and may explain a portion of the persistent wage differentials found between whites and minorities. A related question is whether job-level segregation affects the level of employment benefits associated with one’s job, in addition to wages.

Table 6 reports baseline ordered logit estimates that describe the relationships between the number of benefits received (measured by an index ranging from 0 to 4) and the variables listed in Table 1, first without controlling for job segregation. Controls included in columns (1)-(6) of Table 6 correspond to those of columns (1)-(6) of Table 3.

Based upon column (1) of Table 6, which includes the basic personal and human capital controls, there is no statistical difference in the distribution of employment benefits between black men and white men. Black women, Hispanic men and Hispanic women, however, all receive fewer job benefits than their white counterparts with the same basic personal and human capital levels. When controls are added for English fluency and citizenship, the black-white benefits differential remains for women, while differences in benefit levels between whites and Hispanics are eliminated for both men and women. Again, it is important to note that while the English and citizenship controls may measure important productivity differences, it is also possible that some discrimination is based on differences in these characteristics. It is interesting to note that, as in the log wage regressions, the negative effects associated with the ability to speak English and U.S. citizenship differ a great deal by sex. Women are penalized in terms of the level of benefits received to a much greater extent than men for not being able to speak English and not being a U.S. citizen (although these results fall apart in column (6), which does not seem to fit the data for women well in general).

The impact of most human capital variables on the level of employment benefits received is as one would predict. Age, educational attainment, prior job experience and tenure are all associated with a higher level of employment benefits, while part-time jobs are associated with significantly lower benefits. Being unionized and working in larger firms are associated with higher benefits. Overall, there seems to be little relationship between job benefits and supervisory attainment and job tasks, although for men jobs that entail working with computers or reading paragraphs on a daily basis are associated with a higher level of benefits.

Table 7 investigates the impact of job segregation on the level of job benefits. Ordered logit estimates of coefficients on the job segregation measures and the race/ethnicity dummies are reported for specifications similar to those of Table 6. For men, column (1) indicates that the Hispanic-white benefits differential is completely eliminated once I control for job segregation. But there is a significant negative effect of being segregated into jobs with predominantly Hispanics on the level of employment benefits for men. This estimated effect remains significant in all three columns.

Column (4) for women indicates that the inclusion of job segregation measures does nothing to eliminate the racial and ethnic differentials in employment benefits. While the Hispanic-white differential for women is eliminated with the inclusion of controls for English fluency and citizenship in columns (5) and (6), the black-white differential for women remains significant even with the additional controls for job tasks, supervisory authority, industry and occupation in column (6). Table 7 does not seem to indicate that job segregation negatively affects the likelihood of receiving employment benefits for women. In fact, column (6) indicates that women working in minority dominated jobs are likely to receive a greater number of benefits than those working in “white jobs.”

To investigate this issue further, Table 8 contains results from estimating separate probit equations for individual job benefits, since arguably, benefits such as health insurance may be of greater value than, say, sick leave. Panel A contains the results for men, and shows that men segregated into jobs with Hispanics are significantly less likely to receive retirement insurance and health insurance for their family. The results in panel B indicate that women segregated into jobs with minorities (either blacks or Hispanics)

are more likely to receive health insurance for themselves and their family. Considering the high cost of health insurance, these benefits may somewhat offset the lower wages paid to women in minority dominated jobs. (Unfortunately, the data do not provide an indication of the level of coverage associated with benefits, so further speculation as to the economic significance of these results is not pursued here.)

The Determinants of Job Segregation

I now turn to the investigation of the most probable sources of segregation at the job level. Since policy makers are likely to be more interested in the effects of segregation on the labor market outcomes of minorities, I focus on the determinants of job segregation for blacks and Hispanics, omitting whites from the following analyses. As a first pass at attempting to understand the various potential sources of job segregation, Table 9 presents several of the characteristics discussed above by race/ethnicity and whether one works in a segregated job (being in a segregated job is defined as working with mostly blacks if the individual is black, or working with mostly Hispanics if the individual is Hispanic). Cutting the data this way reveals some interesting relationships.

Table 9 indicates that both blacks and Hispanics who work in segregated jobs also live in more segregated neighborhoods (defined at the census tract level), while those who are not segregated by job commute longer distances to work. For blacks, those who work in a segregated job are more likely to have daily face-to-face customer contact, while Hispanics are less likely to have direct customer contact if they work in a segregated job. The distribution of educational attainment is lower for both groups of minorities who are segregated at the job level. Being unionized, working in a larger firm,

and having supervisory authority are all characteristics that seem to decrease the likelihood of job segregation for minorities.

For Hispanics, job segregation is strongly related to English fluency and citizenship status. Table 9 shows that Hispanics who work in segregated jobs are much more likely to have poorer English skills and are much less likely to be a U.S. citizen. The metropolitan area dummies reveal yet another interesting relationship. Blacks in Atlanta appear to be more likely to be segregated by job. Since the south is a region with a long history of intense racial discrimination, this may indicate the existence of some residual employer and/or consumer discrimination. Likewise, Hispanics living in the Los Angeles metropolitan area, a region known for ethnic tensions, are more likely than not to be segregated by job. Finally, the majority of blacks and Hispanics living in Boston are not working in segregated jobs.

Table 10 further investigates these relationships in a multivariate setting. The results confirm the first striking relationship we saw above in the summary statistics of Table 9: Minorities who face more residential segregation are more likely to be segregated in the job market. Note also that blacks and Hispanics who commute longer distances to work are significantly less likely to be in a segregated job. These two relationships seem to support the theory regarding the impact of spatial mismatch on job segregation.²⁸

The impact of residential segregation is particularly sizable for blacks, with a one hundred percent increase in the proportion black in one's census tract implying roughly a

²⁸ In models not reported here, an interaction term between commute time and residential segregation was included and indicated that the effect of residential segregation on job segregation declines significantly with commute time. While most estimates of other included controls did not vary significantly with the addition of this interaction term, commute time itself lost its significance in several of the specifications.

40 percent increase in the likelihood of working in a segregated job. For Hispanics, columns (3)-(5) indicate that a one hundred percent increase in the proportion Hispanic in one's neighborhood implies an increase in the probability of job segregation on the order of 14-20 percent. (These estimates are calculated at the mean of the data.) Commute time, on the other hand, seems to have a much smaller impact on the likelihood of job segregation.

Once controlling for the appropriate covariates, whether one works in a job with daily face-to-face customer contact does not seem to affect the likelihood of job segregation. Although column (3) for Hispanics indicates that Hispanics in jobs with customer contact are less likely to work in a segregated job, the significance of this result falls apart in columns (4) and (5), when controls for English fluency and citizenship are included. Nevertheless, these results alone should not necessarily be taken as convincing evidence that customer discrimination does not affect the probability of job segregation. Again, a better measure of the likelihood of facing customer discrimination would be a measure of customers' racial and ethnic composition, interacted with face-to-face contact, since one would expect less discrimination by customers of one's same race or ethnicity.²⁹

Columns (4) and (5) indicate that Hispanics who cannot speak English "well" or "at all" are significantly more likely to be segregated into jobs dominated by Hispanics. The impact of these variables is quite sizable, with the inability to speak English "well" or "at all" suggesting a 14-24 percent increase in the probability of job segregation, compared to those who can speak English "very well." The citizenship status of

Hispanics also seems to play a significant role in the determination of whether one is segregated by job, with non-U.S. citizens 7-11 percent more likely to be segregated than U.S. citizens by birth. Such segregation may result from personal choice, employer discrimination or customer discrimination, or some combination of the three.

While part-time work status does not significantly affect the likelihood of job segregation once I control for other covariates, being unionized seems to significantly reduce job segregation for blacks and Hispanics. (Perhaps unions serve to provide an important source of labor market information, especially for newly immigrated Hispanics). Supervisory authority and the ability to set the pay of coworkers, in general, does not appear to be strongly associated with the likelihood of being in a segregated job, although column (2) for blacks indicates that those in supervisory positions are less likely to be segregated.

Table 10 also investigates the impact of firm size and region on whether minorities are likely to be racially or ethnically segregated by job. Both of these variables may be indicative of employer discrimination. Since larger firms are subject to Affirmative Action laws and more likely to have employees that specialize in labor law, we might expect to see a lesser degree of racial and ethnic segregation along job lines among larger firms. This is essentially what is seen in Table 10, which shows that an increase in firm size is associated with a significant decrease in the likelihood of job segregation for both Blacks and Hispanics. Table 10 also confirms that region of the country (for which metropolitan area serves as a proxy) is likely to play a significant role in the likelihood of a minority being segregated. Blacks in Atlanta are more likely to be

²⁹ To test whether the effect of meeting customers face to face depends on the proportion minority in one's neighborhood, measures of residential segregation were interacted with the customer contact dummy. This

segregated by job than blacks in other regions, and Hispanics are more likely to be segregated by job if they work in Los Angeles. The metropolitan dummy variables may be picking up both employer and customer discriminatory tastes, which are likely to be correlated by region. However, Hispanics in Los Angeles may be more likely to work in segregated jobs than those in other regions simply due to the relatively high proportion of Hispanics living in the area.

Potential Econometric Problems

Several econometric problems need to be addressed before we can draw any firm conclusions from the above empirical results. The first set of potential problems stems from limitations of the data used in this study. As discussed above, job segregation is measured from a survey question asking individuals the race/ethnicity of most of their fellow coworkers doing similar work. The survey question does not refer to a specific level of occupational disaggregation (although most respondents likely refer to a fairly detailed occupation in their establishment). Measurement error in the binary indicators of job segregation therefore may be an issue. If the measurement error is classical (i.e., mean-zero “white noise”), however, the estimated effects of job segregation presented in the above tables are biased upwards towards zero.³⁰

The lack of segregation data by industry and occupation is unlikely to introduce a significant amount of bias to the results of this paper. The findings of Bayard et al. (1999) show that while segregation at the establishment and job level is severe and has a

interaction term was generally positive, but insignificant in all specifications.

³⁰ Another potential problem with this measure of job segregation is that one can not determine how many other people do the job in question. This is only a concern for cases in which there are very few coworkers doing similar work, in which case the measure will not be very meaningful. As an extreme example, suppose there are only two people performing a particular job within a firm, both of whom are Hispanic. The respondent in question will be classified as working in a segregated job.

significant impact on wages, there is relatively little racial or ethnic segregation along occupation or industry lines. Overall, they find that the negative segregation effect for minorities stems primarily from job-level segregation. Moreover, any such bias is likely reduced by the inclusion of individual controls for industry and occupation.

As with most empirical studies, the possibility of omitted variables is another potential econometric problem that needs to be addressed. Individuals who are in segregated jobs may be different than those who are not in segregated jobs in ways not captured by the included controls. Such unobserved heterogeneity will bias the above estimates of the effect of job segregation on earnings and benefits if such differences are related to labor market outcomes. For example, those in segregated jobs may be less skilled than those in non-segregated jobs (as predicted by the “quality sorting” hypothesis). Differences in job segregation will therefore reflect not only forms of discriminatory behavior, but also unmeasured quality differences. Omitted differences in skill will be captured by the error term and cause the above estimates of the segregation effect to be downward biased (away from zero).

I deal with this omitted variable problem by using family background characteristics as proxy variables for unmeasured skill. As noted above, Neal and Johnson (1996) conclude that the disadvantages young black workers face in the labor market arise mostly from obstacles they faced as children in acquiring productive human capital. It is likely that personal human capital levels are correlated with both parental human capital and family structure during one’s upbringing. As such, controls for living with both parents until age 16, parental education in years, and whether parents worked when the individual was age 16 are included to control for unobserved heterogeneity.

The inclusion of family background variables should also reduce any omitted variable bias in the estimates of the coefficients on the black and Hispanic dummy variables.³¹

Table 11 lists coefficient estimates on the black and Hispanic dummy variables and on the job segregation controls, replicating the models shown in Table 4 with the addition of family background proxies for quality. The results provide little evidence of omitted variable bias affecting the job segregation coefficient estimates. In fact, when controlling for family background, the estimates of penalties associated with working in jobs dominated by Hispanics actually increase slightly. On the other hand, omitted variable bias may have caused the wage gaps to be overestimated in Table 4. When family background variables are included, the black-white wage gap for men is no longer significant, the Hispanic-white wage gap for men is reduced by about one-third, and the Hispanic-white wage gap for women also loses its significance.³²

Table 12 investigates this issue by demographic group, reporting coefficient estimates on the job segregation controls from models similar to those in Table 5, but with family background controls. Again, there is little change in the job segregation coefficient estimates and little evidence of omitted variable bias.³³ If the family background variables are indeed controlling for skill differences not captured by the other

³¹ Neal and Johnson (1996) find such family background variables to be strongly correlated with AFQT scores, and to explain a portion of the black-white gaps in AFQT.

³² The family background variables themselves were consistently insignificant all wage equations (with the exception of mother's education implying a significant increase in wages for men only). For this reason, coupled with the fact that coefficients on the job segregation variables of interest did not change significantly and the family background variables are missing for a significant fraction of the sample, these background controls were left out of the wage equations in previous tables.

³³ It is important to note that the estimates presented in Tables 10 and 11 are based on significantly smaller samples than those reported in Tables 4 and 5 due to missing information on family background. For example, roughly 25 percent of the full sample reported that they did not know the number of years their father was in school. This causes some of the estimates (in Table 12 in particular) to be estimated from very small cells, which is likely to explain most of the significant variations between Table 5 results and Table 12 results.

controls, a significant reduction in the job segregation effects would be expected if quality sorting at the job level were the only force behind segregation.

Although the results presented in Table 10 suggest that spatial mismatch (as proxied by residential segregation) contributes to job segregation, it is worth investigating this relationship a bit further before drawing any firm conclusions. First of all, it is important to note that even in the absence of discrimination and spatial mismatch, workforces will still resemble neighborhoods in terms of racial and ethnic composition. Jobs in more segregated neighborhoods will be more segregated, since the probability that an employer hires a minority should rise with the proportion of minority applicants.³⁴ If residential segregation is merely picking up such a neighborhood effect and not spatial mismatch, however, there are no obvious wage implications. Therefore, the fact that working in minority dominated jobs was found to significantly affect wages calls into question the possibility that job segregation is simply due to the fact that neighborhoods tend to be racially and ethnically segregated.

As a further probe, Table 13 provides estimates of coefficients on measures of job segregation and residential segregation for wage models similar to those depicted in Table 5. If job segregation is due solely to the ethnic makeup of one's neighborhood, and not to labor market discrimination or spatial mismatch, then holding constant residential segregation, it should not affect wages. Table 13 shows that job segregation still negatively affects wages even after controlling for residential segregation. For male

³⁴ This logic also predicts a negative relationship between firm size and job segregation if larger firms are likely to hire more people into any given job. Note also that if there is residential segregation but no spatial mismatch problems (i.e., firms and workers are similarly distributed across metropolitan areas), commute time will still be negatively related to job segregation. However, since the relocation of firms to the suburbs is well documented, it is unlikely that such findings between commute, residential segregation and job segregation should be characterized as arising due to neighborhood composition effects only.

minorities, measures of the proportion minority in one's neighborhood generally has a negative but insignificant effect on wages, and does not have an appreciable effect on either the magnitude or the significance of the job segregation variables. Likewise, residential segregation measures do not have a significant impact on wages for Hispanic women, nor do they significantly alter the effect of job segregation. The only minority group for whom residential segregation seems to substantially reduce wages, and reduce the negative impact of job segregation, is black women. Interestingly, (with the exception of black women) Table 13 indicates that living in minority dominated neighborhoods has the biggest negative wage effect for whites.

Since not everyone works where they live, Table 14 takes this analysis a step further by controlling for not only whether one *lives* in a minority dominated neighborhood (now measured by a dummy variable for a neighborhood that is at least 50 percent black or Hispanic), but also whether they are likely to *work* in a minority dominated neighborhood. This is accomplished by adding an indication of a short commute (less than or equal to 10 minutes), and a measure of short commute interacted with minority dominated residence.³⁵ The results support the contention that job segregation is not due solely to the makeup of one's neighborhood. Comparing the results of Table 5 with those of Table 14, the negative effects of job segregation on wages remain even after controlling for the racial and ethnic makeup of where one lives and works, leaving spatial mismatch, discrimination, and/or other factors as potential causes.

³⁵ Several different specifications of these variables were explored, with a short commute defined as less than or equal to 5, 10, 15, or 20 minutes, and a minority dominated neighborhood defined as having at least 30, 50, or 75 percent blacks or Hispanics. The job segregation effects did not change significantly under these different scenarios; however, the estimates on the dummy variables themselves (and the interaction term) were somewhat sensitive to how they were defined (although they were generally insignificant). For this reason, I do not put much emphasis on the dummy variable estimates.

There is one more implication of this simple test. If spatial mismatch (i.e., relatively low labor demand in minority dominated areas) were the *only* reason for job segregation, then otherwise-identical workers working in equally segregated areas should earn the same amounts, whether or not their jobs are segregated. Thus, Table 14 also suggests that job segregation is likely due to more than just spatial mismatch, since job segregation still matters even after controlling for neighborhood segregation. In addition, if the interaction term accurately picks up whether one works in a minority dominated neighborhood, the results seem to indicate that spatial mismatch is only a significant problem for black women in terms of its wage effects.³⁶ However, it is important to keep in mind that past studies (which tend to focus on blacks) find that spatial mismatch has a stronger effect on employment rates than on wages.³⁷

Another econometric issue concerns the estimated effects of commute time on job segregation. Underlying the results presented in Table 10 is the assumption that minorities take spatial mismatch as a given constraint, and then decide whether or not they can commute longer distances to overcome its negative effects on crowding. As predicted by the discussion outlined in the theory section, I find that commute time is negatively related to the likelihood of job segregation. However, Ihlanfeldt and Sjoquist (1990) interpret high average commute times in a neighborhood as indicative of more mismatch and poorer job access under the assumption that individuals facing more spatial mismatch (particularly inner-city minorities) will have to commute more. Since more spatial mismatch would imply more job segregation, this interpretation suggests that a

³⁶ It is interesting to note that living in a segregated neighborhood actually seems to be related to higher wages for Hispanic females. This may be indicative of the importance of social contacts for new immigrants.

measure of average commute time may be positively correlated with job segregation. Therefore, if higher commute times reflect more spatial mismatch, in addition to the efforts to overcome its negative effects, the estimated coefficients on commute time in the above job segregation probit equations are muddled by these opposing relationships and biased downward towards zero.

It is also possible that commute time may be simply capturing the empirical finding that more-educated and higher-wage individuals generally commute longer distances. However, the correlation between commute time and income is strongest for whites, who are much less likely to be constrained by the discrimination and zoning problems that often restrict the residential choices of minorities (Holzer, 1991). Thus estimating separate equations for blacks and Hispanics likely mitigates the severity of this problem.³⁸ (While instrumental variables provides another potential way to deal with these potential econometric problems associated with commute time, variables that influence commute time but that do not directly influence the likelihood of being in a segregated job would be required. Unfortunately, there are no compelling choices for such instruments available in MCSUI.³⁹)

Lastly, I turn to the issue of whether the negative effect of job segregation on wages merely represents a compensating differential that minorities are willing to pay to

³⁷ Another possibility is that, since the sample sizes are rather small, there is simply not enough independent variation between residential segregation and job segregation.

³⁸ Ihlanfeldt and Sjoquist (1990) find substantially larger effects of travel time on employment rates for blacks than for whites, supporting the notion that the bias (downward in their case) resulting from endogeneity is reduced by running separate equations. The inclusion of controls for educational attainment and other wage determinants in the logit segregation equations should also reduce the likelihood of the commute time measure capturing the fact that more-educated and higher-wage people generally commute over further distances.

³⁹ Cutler and Glaeser (1997) use this IV approach to deal with the possibility of residential segregation being endogenous with respect to their outcome measures of education, income and single motherhood. The two instruments they use for residential segregation are the number of municipal and township

work with coethnics. That is, how much of the job segregation we see is a product of personal choice, as opposed to a real constraint due to spatial mismatch or some form of discrimination? This is an important question because self-selection into a segregated job will lead to the same outcomes as discrimination, but with drastically different policy implications. For instance, while some may view the significance of English fluency controls in the job segregation models for Hispanics as indicative of employer or customer discrimination, it is also possible that Hispanics with weak English skills may *choose* to work along side those with whom they can more easily communicate.

To further gauge the importance of personal choice or self-selection in the job segregation process, I use a series of variables that assesses racial attitudes as a proxy for the preference to work with coethnics. Survey respondents were asked to use a scale from 1 to 7 to express their views concerning the ease of “getting along with” whites, blacks and Hispanics, where 1 means “tends to be hard to get along with” and 7 means “tends to be easy to get along with.” A rating of 4 is neutral, meaning “the group is not towards one end or the other.” For instance, blacks that give a high rating for blacks, and relatively low ratings for whites and Hispanics, may be interpreted as individuals who have a taste for working with coethnics.

An indicator of the preference to work with coethnics was created from these variables to assess the relative importance of personal choice, or self-selection, in the job segregation process. The self-selection dummy equals one if an individual gave their own demographic group a higher rating than other demographic groups, meaning they

governments in the metropolitan area and the share of local revenue that comes from intergovernmental sources.

view their own race/ethnicity as being relatively easier to get along with.⁴⁰ This is admittedly a subjective measure, but given the lack of panel data necessary to difference out such preferences, it should at least provide some indication of whether self-selection into segregated jobs is a significant concern.

In Table 15, I present the results of including the self-selection dummy in the five models depicted in Table 10. As an additional probe, I also include the individual scale variables from which the self-selection dummy was created in separate specifications. Overall, I found little evidence that job segregation is likely to be due *solely* to self-selection as measured by racial attitudes. Panel A of Table 15 shows that the self-selection dummy variable was generally insignificant for blacks and Hispanics.

When the scale variables were included separately, however, there is at least some evidence that racial preferences may play a role in the job segregation process. Panel B of Table 15 indicates that blacks who find Hispanics relatively easy to get along with are less likely to be segregated into jobs with other blacks, while their racial attitudes towards whites and other blacks are not significantly related to the likelihood of job segregation. Perhaps counter-intuitively, it seems that Hispanics who find those of similar ethnic background relatively easy to get along with are *less* likely to be segregated into jobs with other Hispanics. Column (3) of Table 15 seems to indicate that Hispanics who find blacks relatively easy to get along with are less likely to be in a segregated job, however this result falls apart in the models indicated by columns (4) and (5). Racial attitudes of Hispanics towards whites are not significantly related to the likelihood of job segregation.

⁴⁰ An alternative self-selection measure was created to equal one if an individual gave their own race/ethnicity a rating greater than 4, meaning they viewed their own demographic group as being relatively easy to get along with, but gave other demographic groups a rating less than 4, indicating they

Overall, this somewhat crude test of the impact of racial attitudes implies that further investigation of the importance of self-selection in job segregation is merited.⁴¹ It is important to note, however, that the majority of the other covariates were not changed significantly with the addition of the various measures of racial attitudes. This suggests that spatial mismatch and discrimination, as indicated by the other determinants of job segregation previously discussed, are likely to play a role in the job segregation process above and beyond personal choice.

Conclusions and Policy Implications

The results of this paper can be summarized into three main findings. First, job segregation is an important contributor to the lower wages paid to minorities than to whites with similar individual characteristics. Job segregation explains a significant portion of the black-white wage gap that remains after controlling for differences in human capital and job characteristics for both men and women. On the other hand, while job segregation explains a fraction of the Hispanic-white adjusted wage differential for men, it still remains sizable at around 13 percent. The Hispanic-white wage gap for women is virtually eliminated with the addition of controls for English fluency and citizenship, before controlling for job-level segregation. However, job segregation has a significant negative impact on the earnings of blacks and Hispanic women even after controlling for differences in a variety of skill measures. For example, Hispanic women

viewed other races/ethnicities as being relatively difficult to get along with. The results of using this alternative measure were very similar to those reported in Table 15.

⁴¹ A preliminary analysis of the effect of racial attitudes on wages revealed that the self-selection dummy was insignificant with respect to wages, and the negative effect of job segregation on wages actually increased when this variable was included. Given the substantial proportion of the sample that chose not to respond to the racial attitudes questions, however, these results should be viewed with caution due to potential sample selection issues. A complete analysis of the self-selection issue awaits future research.

who are segregated into jobs with mostly Hispanics earn roughly 15 percent less than those who work with mostly whites. These findings suggest that while “equal pay” laws may offer some hope for reducing the Hispanic-white differential for men, policies targeted at alleviating segregation into lower-paying jobs may be more effective at reducing pay gaps for minorities overall.

Second, job segregation plays a much smaller role in explaining differences in the number of employment benefits received between minorities and whites than is does in explaining wage differentials. Starting out with significantly less disparity in unadjusted benefit differentials between whites and minorities to begin with, in general, job segregation seems to have little negative impact on the level of benefits received (although being segregated into jobs with mostly Hispanic men is associated with lower benefit levels). However, it is important to note that the data used here tell us nothing about the quality of the benefits packages received, so the above analysis should be viewed only as an important first step in understanding the impact of job segregation on employment benefits.

Finally, this paper sheds a considerable amount of light on the likely sources of racial and ethnic job segregation. I find that minorities facing more residential segregation are significantly more likely to work in a segregated job, while that those who commute longer distances to work are less likely to be segregated by job. Since these findings are consistent with the notion of spatial mismatch, this research suggests that policies aimed at improving the accessibility of public transportation, reducing housing market discrimination, or encouraging employers to locate in inner-city areas may help reduce job segregation.

This paper also finds that minorities who work in larger firms are less likely to work in segregated jobs, perhaps because larger employers are less likely to engage in employment discrimination due to a greater awareness of the legal consequences or to stronger enforcement of Affirmative Action laws. In addition, English fluency and citizenship status are found to be strongly associated with the likelihood of job segregation for Hispanics. This suggests that improving language skills and access to labor market information for Hispanics may help reduce the considerable earnings disparities we find between Hispanics and whites.

While I view the findings of this study as an important first step in understanding these issues, more definitive policy conclusions should await further analysis of other data that improve on some of the shortcomings discussed above. In particular, a more rigorous examination of the role of personal choice, or self-selecting into segregated jobs, is clearly needed before any firm conclusions can be drawn.

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Table 1
Sample Descriptive Statistics by Demographic Group

	Men			Women		
	White	Black	Hispanic	White	Black	Hispanic
Log hourly wages	2.76 (.040)	2.48 (.053)	2.18 (.034)	2.49 (.034)	2.29 (.040)	1.96 (.037)
# of employment benefits	2.96 (.116)	2.74 (.138)	1.87 (.133)	2.54 (.117)	2.43 (.127)	1.62 (.128)
Age	39.42 (.767)	35.76 (1.11)	34.39 (.597)	39.70 (.861)	37.04 (.749)	35.60 (.775)
Married	.604	.426	.590	.601	.306	.457
Number of children < 18	.604 (.063)	.656 (.109)	.998 (.092)	.738 (.062)	.736 (.067)	1.218 (.083)
No high school degree	.024	.065	.443	.049	.068	.443
High school/GED	.360	.524	.369	.400	.467	.315
Some college	.147	.135	.076	.204	.275	.148
Bachelor's degree	.293	.192	.090	.251	.134	.076
Advanced degree	.176	.084	.022	.096	.056	.018
Tenure (in years)	7.41 (.510)	5.79 (.541)	4.93 (.305)	6.71 (.531)	6.20 (.413)	4.70 (.428)
Prior experience in job (in years)	4.64 (.472)	3.53 (.583)	2.61 (.349)	4.24 (.486)	2.85 (.245)	2.12 (.333)
Part-time job	.104	.189	.156	.331	.190	.235
Collective bargaining	.169	.260	.181	.139	.229	.132
<u>Speaks English:</u>						
Very well	.918	.786	.342	.909	.835	.379
Well	.077	.184	.243	.077	.118	.167
Not well	.007	.029	.370	.014	.047	.366
Not at all	.000	.000	.045	.000	.000	.088
<u>Citizenship status:</u>						
By birth in U.S.	.929	.760	.298	.900	.932	.298
By naturalization	.036	.047	.077	.064	.025	.132
Not a citizen	.035	.193	.625	.037	.042	.618
Atlanta	.137	.284	.012	.168	.327	.013
Boston	.404	.134	.048	.408	.126	.049
Los Angeles	.459	.582	.940	.423	.548	.938

Table 1 (cont'd)

	Men			Women		
	White	Black	Hispanic	White	Black	Hispanic
Firm size	453 (72.7)	473 (71.0)	219 (80.6)	481 (78.06)	562 (70.8)	203 (39.2)
<u>Job tasks performed daily:</u>						
Customer contact	.594	.549	.445	.627	.665	.469
Telephone	.554	.549	.245	.619	.616	.322
Reading	.590	.649	.485	.558	.561	.355
Writing	.429	.376	.285	.459	.412	.260
Use computer	.556	.440	.181	.575	.516	.261
Mathematics	.700	.674	.395	.662	.566	.366
Supervise others	.365	.320	.260	.287	.253	.171
Set the pay of others	.115	.100	.096	.070	.041	.033
<u>Coworkers are predominantly:</u>						
White	.801	.338	.185	.838	.389	.215
Black	.034	.355	.028	.023	.414	.021
Hispanic	.059	.119	.723	.032	.072	.688
Asian	.010	.004	.033	.027	.042	.023
Other (or mixed)	.097	.184	.032	.080	.082	.053
<u>Industry:</u>						
Construction	.066	.063	.142	.013	.008	.004
Durable manufacturing	.102	.040	.137	.077	.047	.105
Non-durable mfg.	.063	.124	.106	.043	.023	.157
Transportation	.098	.123	.090	.034	.083	.037
Wholesale/retail trade	.122	.117	.237	.196	.142	.180
Finance/insurance/ real estate	.075	.086	.035	.089	.106	.046
Service	.385	.368	.209	.505	.527	.432
Public/self employed/ other	.152	.125	.184	.052	.070	.031
<u>Occupation:</u>						
Managerial	.196	.078	.069	.131	.079	.053
Professional/technical	.310	.201	.071	.317	.198	.089
Sales	.105	.057	.042	.126	.138	.101
Clerical/administration	.083	.169	.076	.261	.296	.163
Service/labor	.101	.284	.270	.110	.223	.303
Crafts/operative	.188	.176	.444	.044	.059	.283
Self employed/other	.016	.019	.024	.009	.001	.008

Note: Standard errors are reported in parentheses for continuous variables. Standard errors for binary variables with mean p can be calculated by taking the square root of $p(1-p)/n$. Sample sizes are 616, 476 and 610 for white males, black males and Hispanic males, respectively, and 644, 916, and 633 for white females, black females and Hispanic females, respectively. Some estimates are based on slightly smaller samples due to missing values.

Table 2
Hourly Wages and Log Wages
by Demographic Group and Job Segregation

A. Hourly Wages Coworkers are predominantly:	Men			Women		
	White	Black	Hispanic	White	Black	Hispanic
White	20.41 (1.519) [16.92]	15.56 (.914) [14.80]	12.39 (.857) [11.52]	14.45 (.839) [12.00]	11.92 (.637) [11.00]	10.83 (.627) [10.00]
Black	15.75 (2.497) [14.00]	14.17 (2.943) [10.00]	11.57 (3.066) [11.00]	13.91 (2.306) [12.90]	10.59 (.545) [8.50]	11.61 (1.886) [14.50]
Hispanic	12.61 (1.287) [10.50]	15.72 (4.168) [11.54]	9.33 (.439) [7.50]	7.26 (1.200) [6.00]	16.21 (6.86) [7.50]	6.95 (.362) [5.60]
B. Log Wages Coworkers are predominantly:	Men			Women		
	White	Black	Hispanic	White	Black	Hispanic
White	2.81 (.046) [2.83]	2.64 (.061) [2.69]	2.40 (.072) [2.44]	2.50 (.038) [2.48]	2.37 (.055) [2.40]	2.28 (.059) [2.30]
Black	2.59 (.134) [2.64]	2.33 (.093) [2.30]	2.25 (.266) [2.40]	2.53 (.157) [2.56]	2.20 (.047) [2.14]	2.37 (.179) [2.67]
Hispanic	2.44 (.094) [2.36]	2.46 (.263) [2.45]	2.10 (.036) [2.01]	1.85 (.149) [1.79]	2.32 (.254) [2.01]	1.82 (.040) [1.72]

Note: Standard errors of estimates are reported in parentheses. Median wages of distributions reported in brackets. Wages of individuals working with mostly Asians or other/mixed available upon request.

Table 3
Log Hourly Wage Regressions
without Controlling for Job Segregation

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-.149 (.054)	-.144 (.055)	-.078 (.043)	-.118 (.041)	-.119 (.042)	-.086 (.035)
Hispanic	-.353 (.052)	-.299 (.064)	-.176 (.050)	-.223 (.050)	-.096 (.065)	-.025 (.043)
Age	.016 (.013)	.017 (.013)	.022 (.012)	.027 (.014)	.029 (.014)	.020 (.009)
Age ²	-.0002 (.0002)	-.0002 (.0002)	-.0003 (.0001)	-.0003 (.0002)	-.0004 (.0002)	-.0002 (.0001)
High school/GED	.190 (.044)	.129 (.048)	.114 (.043)	.362 (.058)	.274 (.068)	.139 (.056)
Associates/ Vocational/Trade	.229 (.061)	.164 (.065)	.119 (.056)	.476 (.056)	.393 (.067)	.238 (.061)
Bachelor's degree	.504 (.062)	.427 (.064)	.265 (.055)	.679 (.065)	.591 (.077)	.363 (.069)
Advanced degree	.624 (.091)	.518 (.087)	.204 (.083)	.700 (.103)	.629 (.106)	.369 (.098)
Part-time job	-.067 (.077)	-.046 (.075)	.005 (.052)	-.062 (.049)	-.071 (.048)	.032 (.044)
Married	.047 (.050)	.054 (.049)	.083 (.037)	.007 (.039)	.020 (.038)	-.005 (.029)
# children <18	.033 (.022)	.035 (.022)	.034 (.018)	-.007 (.018)	-.006 (.017)	-.005 (.015)
Prior experience	.015 (.004)	.015 (.004)	.015 (.003)	.013 (.004)	.012 (.004)	.010 (.003)
Tenure	.054 (.008)	.051 (.008)	.038 (.007)	.020 (.013)	.019 (.013)	.007 (.011)
Tenure ²	-.001 (.0003)	-.001 (.0003)	-.0008 (.0002)	.0003 (.001)	.0003 (.0006)	.0006 (.0005)
Unionized	.069 (.049)	.073 (.048)	.164 (.044)	-.025 (.048)	-.042 (.046)	.023 (.040)
Ln (firm size)	.007 (.012)	.008 (.011)	.012 (.010)	.022 (.009)	.019 (.008)	.018 (.008)

Table 3 (cont'd)

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Speaks English well	--	-.194 (.048)	-.125 (.041)	--	-.020 (.062)	-.033 (.043)
Does not speak English well	--	-.180 (.071)	-.129 (.061)	--	-.047 (.099)	-.091 (.060)
Does not speak English	--	-.268 (.092)	-.081 (.109)	--	-.275 (.109)	-.320 (.085)
U.S. citizen by nationalization	--	.202 (.124)	.003 (.066)	--	-.161 (.090)	-.181 (.062)
Not a U.S. citizen	--	.012 (.060)	-.029 (.056)	--	-.209 (.080)	-.173 (.058)
Supervise others	--	--	.034 (.038)	--	--	-.078 (.047)
Supervise and set the pay of others	--	--	.135 (.068)	--	--	.323 (.067)
<u>Daily job tasks:</u>						
Face-to-face customer contact	--	--	-.050 (.035)	--	--	-.093 (.036)
Talk w/ customers on telephone	--	--	.054 (.042)	--	--	.098 (.039)
Reading paragraphs	--	--	.029 (.032)	--	--	-.007 (.032)
Writing paragraphs	--	--	.029 (.039)	--	--	.130 (.036)
Work w/ computer	--	--	.113 (.042)	--	--	.096 (.041)
Arithmetic	--	--	.011 (.039)	--	--	-.017 (.034)
Occupation controls:	No	No	Yes	No	No	Yes
Industry controls:	No	No	Yes	No	No	Yes
N	1,640	1,638	1,438	2,059	2,059	1,913
R ²	.464	.479	.578	.458	.474	.623

Note: Robust standard errors of regression estimates reported in parentheses. Omitted categories are "high school dropout" for educational attainment, "speaks English very well" for English fluency, and "US citizen by birth" for citizenship. All regressions include a constant, a missing value dummy for part-time job, and control for metropolitan area and whether reported job information is for most recent job. Sample sizes differ between regressions due to missing values.

Table 4
Log Hourly Wage Regressions:
The Impact of Job Segregation

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-.094 (.053)	-.091 (.054)	-.051 (.048)	-.069 (.046)	-.071 (.046)	-.053 (.041)
Hispanic	-.251 (.052)	-.219 (.059)	-.131 (.048)	-.099 (.054)	-.001 (.065)	.022 (.048)
<u>Coworkers are</u> <u>Predominantly:</u>						
Black	-.130 (.071)	-.131 (.070)	-.106 (.055)	-.089 (.048)	-.096 (.048)	-.085 (.052)
Hispanic	-.211 (.058)	-.167 (.060)	-.108 (.049)	-.233 (.064)	-.204 (.067)	-.118 (.061)
Asian	-.022 (.161)	-.010 (.138)	-.051 (.101)	-.016 (.058)	-.002 (.064)	-.068 (.054)
Other (or mixed)	-.092 (.066)	-.099 (.064)	-.040 (.065)	.063 (.053)	.066 (.052)	.039 (.050)
N	1,587	1,585	1,428	1,995	1,995	1,899
R ²	.485	.499	.581	.477	.491	.627

Note: Included controls correspond to those listed in Table 3. Robust standard errors of regression estimates are reported in parentheses. Sample sizes differ due to missing values.

Table 5a
Log Hourly Wage Regressions:
The Impact of Segregation by Demographic Group

	Black			
	Males		Females	
	(1)	(2)	(1)	(2)
<u>Coworkers are Predominantly:</u>				
Black	-.192 (.084)	-.142 (.059)	-.088 (.047)	-.086 (.047)
Hispanic	-.198 (.127)	-.078 (.090)	.072 (.195)	.049 (.099)
Asian	-.135 (.174)	-.163 (.245)	-.026 (.061)	-.031 (.074)
Other (or mixed)	.066 (.115)	.056 (.080)	-.004 (.063)	-.005 (.053)
High school/GED	-.003 (.133)	-.039 (.121)	.193 (.066)	.083 (.065)
Associates/ Vocational/Trade	.038 (.163)	-.040 (.143)	.320 (.079)	.153 (.076)
Bachelor's degree	.472 (.168)	.176 (.127)	.664 (.085)	.410 (.095)
Advanced degree	.481 (.206)	.343 (.162)	.762 (.091)	.563 (.103)
Part-time job	-.082 (.104)	-.020 (.113)	-.102 (.055)	.029 (.055)
Married	-.073 (.094)	.042 (.065)	.055 (.058)	.003 (.043)
# children <18	.019 (.036)	-.008 (.024)	.003 (.019)	-.006 (.014)
Prior experience	.021 (.010)	.005 (.006)	.011 (.005)	.011 (.004)
Tenure	.057 (.015)	.049 (.012)	.043 (.011)	.028 (.008)
Tenure ²	-.001 (.0005)	-.001 (.0004)	-.0008 (.0003)	-.0005 (.0003)
Unionized	.065 (.073)	.126 (.050)	.071 (.052)	.148 (.047)
Ln (firm size)	-.002 (.019)	.008 (.014)	.018 (.011)	.021 (.009)

Table 5a (cont'd)

	Black			
	Males		Females	
	(1)	(2)	(3)	(4)
Supervise others	--	-.018 (.055)	--	.049 (.043)
Supervise and set the pay of others	--	.144 (.117)	--	.383 (.156)
<u>Daily job tasks:</u>				
Face-to-face customer contact	--	-.031 (.061)	--	-.125 (.046)
Talk w/ customers on telephone	--	.028 (.054)	--	.075 (.057)
Reading paragraphs	--	.159 (.064)	--	-.033 (.043)
Writing paragraphs	--	-.017 (.058)	--	.073 (.042)
Work w/ computer	--	.141 (.064)	--	.115 (.043)
Arithmetic	--	-.012 (.050)	--	.034 (.038)
Occupation controls:	No	Yes	No	Yes
Industry controls:	No	Yes	No	Yes
N	448	414	843	821
R ²	.464	.608	.505	.635

Note: Robust standard errors of regression estimates are reported in parentheses. The omitted categories are "high school dropout" for educational attainment, "speaks English very well" for English fluency, and "US citizen by birth" for citizenship. All regressions include a constant, a missing value dummy for part-time job, and control for age, metropolitan area and whether reported job information is for most recent job. Sample sizes differ between regressions due to missing values.

Table 5b
Log Hourly Wage Regressions:
The Impact of Segregation by Demographic Group

	Hispanic			
	Males		Females	
	(1)	(2)	(1)	(2)
Coworkers are				
<u>Predominantly:</u>				
Black	-.380 (.162)	-.347 (.097)	-.031 (.084)	-.234 (.093)
Hispanic	-.196 (.071)	-.097 (.073)	-.148 (.081)	-.144 (.055)
Asian	-.010 (.191)	-.018 (.111)	-.042 (.097)	-.049 (.124)
Other (or mixed)	-.002 (.096)	.063 (.122)	.091 (.096)	-.140 (.108)
High school/GED	.200 (.063)	.098 (.051)	.321 (.082)	.127 (.078)
Associates/ Vocational/Trade	.201 (.090)	.088 (.079)	.330 (.101)	-.019 (.070)
Bachelor's degree	.199 (.109)	.089 (.076)	.678 (.118)	.167 (.084)
Advanced degree	.649 (.199)	.299 (.146)	.170 (.299)	-.123 (.163)
Part-time job	.169 (.077)	.103 (.064)	.034 (.066)	-.041 (.059)
Married	.094 (.065)	.121 (.046)	-.032 (.061)	.095 (.050)
# children <18	-.002 (.018)	-.0004 (.019)	-.004 (.025)	-.026 (.018)
Prior experience	.023 (.007)	.014 (.006)	-.007 (.007)	-.008 (.006)
Tenure	.071 (.016)	.067 (.011)	.035 (.016)	.012 (.013)
Tenure²	-.002 (.0009)	-.002 (.0007)	-.0006 (.0005)	-.0002 (.0005)
Unionized	.210 (.074)	.198 (.063)	.036 (.076)	-.035 (.061)
Ln (firm size)	.028 (.019)	.048 (.017)	.029 (.015)	.024 (.014)

Table 5b (cont'd)

	Hispanic Males		Hispanic Females	
	(1)	(2)	(3)	(4)
Speaks English well	--	-.021 (.059)	--	-.005 (.065)
Does not speak English well	--	-.005 (.074)	--	-.164 (.063)
Does not speak English	--	.034 (.104)	--	-.314 (.066)
U.S. citizen by nationalization	--	-.160 (.110)	--	-.214 (.080)
Not a U.S. citizen	--	-.092 (.072)	--	-.120 (.070)
Supervise others	--	-.123 (.066)	--	-.061 (.059)
Supervise and set the pay of others	--	.125 (.097)	--	.063 (.138)
<u>Daily job tasks:</u>				
Face-to-face customer contact	--	.050 (.046)	--	-.013 (.066)
Talk w/ customers on telephone	--	.119 (.061)	--	.113 (.064)
Reading paragraphs	--	-.038 (.047)	--	.041 (.050)
Writing paragraphs	--	.063 (.059)	--	.161 (.055)
Work w/ computer	--	.165 (.063)	--	-.066 (.066)
Arithmetic	--	.060 (.043)	--	.033 (.046)
Occupation controls:	No	Yes	No	Yes
Industry controls:	No	Yes	No	Yes
N	580	537	577	545
R ²	.428	.580	.470	.676

Note: Robust standard errors of regression estimates are reported in parentheses. The omitted categories are "high school dropout" for educational attainment, "speaks English very well" for English fluency, and "US citizen by birth" for citizenship. All regressions include a constant, a missing value dummy for part-time job, and control for age, metropolitan area and whether reported job information is for most recent job. Sample sizes differ between regressions due to missing values.

Table 5c
Log Hourly Wage Regressions:
The Impact of Segregation by Demographic Group

	White			
	Males		Females	
	(1)	(2)	(1)	(2)
Coworkers are				
<u>Predominantly:</u>				
Black	.030 (.113)	.045 (.119)	.029 (.131)	.083 (.117)
Hispanic	-.137 (.117)	-.039 (.083)	-.503 (.129)	-.332 (.104)
Asian	.094 (.100)	-.230 (.086)	-.035 (.098)	-.060 (.086)
Other (or mixed)	-.125 (.084)	-.096 (.095)	.100 (.071)	.113 (.067)
High school/GED	.074 (.145)	.163 (.134)	.434 (.120)	.262 (.094)
Associates/ Vocational/Trade	.105 (.141)	.164 (.134)	.519 (.119)	.382 (.093)
Bachelor's degree	.384 (.161)	.359 (.149)	.681 (.123)	.496 (.108)
Advanced degree	.451 (.165)	.273 (.142)	.742 (.145)	.457 (.135)
Part-time job	-.232 (.140)	-.106 (.100)	-.108 (.071)	.033 (.063)
Married	.080 (.074)	.097 (.060)	.022 (.059)	-.033 (.041)
# children <18	.066 (.041)	.089 (.033)	-.021 (.029)	-.013 (.025)
Prior experience	.011 (.007)	.014 (.005)	.017 (.005)	.011 (.003)
Tenure	.055 (.012)	.032 (.009)	.005 (.016)	-.002 (.013)
Tenure²	-.001 (.0004)	-.001 (.0003)	.0009 (.0007)	.001 (.001)
Unionized	-.030 (.065)	.151 (.072)	-.034 (.068)	.063 (.061)

Table 5c (cont'd)

	White			
	Males		Females	
	(1)	(2)	(3)	(4)
Ln (firm size)	.0004 (.016)	.004 (.014)	.021 (.013)	.006 (.012)
Supervise others	--	.067 (.055)	--	-.119 (.068)
Supervise and set the pay of others	--	.159 (.091)	--	.381 (.084)
<u>Daily job tasks:</u>				
Face-to-face customer contact	--	-.081 (.058)	--	-.083 (.060)
Talk w/ customers on telephone	--	.041 (.063)	--	.100 (.055)
Reading paragraphs	--	.011 (.053)	--	-.039 (.048)
Writing paragraphs	--	.050 (.059)	--	.141 (.056)
Work w/ computer	--	.061 (.061)	--	.114 (.058)
Arithmetic	--	-.071 (.065)	--	-.010 (.052)
Occupation controls:	N	Y	N	Y
Industry controls:	N	Y	N	Y
N	559	479	575	533
R ²	.406	.511	.395	.592

Note: Robust standard errors of regression estimates are reported in parentheses. The omitted categories are "high school dropout" for educational attainment, "speaks English very well" for English fluency, and "US citizen by birth" for citizenship. All regressions include a constant, a missing value dummy for part-time job, and control for age, metropolitan area and whether reported job information is for most recent job. Sample sizes differ between regressions due to missing values.

Table 6
Ordered Logit Estimates from Job Benefits Models
without Controlling for Job Segregation

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-.036 (-.185)	.075 (.373)	2.57 (1.20)	-.276 (-1.74)	-.271 (-1.71)	-.234 (-1.39)
Hispanic	-.430 (-2.68)	-.046 (-.245)	.025 (.123)	-.461 (-3.22)	.107 (.640)	.137 (.773)
Age	.101 (-.473)	.095 (2.33)	.073 (1.66)	.074 (2.34)	.085 (2.67)	.086 (2.57)
Age ²	-.002 (-3.34)	-.002 (-3.17)	-.001 (-2.44)	-.001 (-2.72)	-.001 (-3.05)	-.001 (-2.89)
High school/GED	.984 (5.68)	.842 (4.42)	.631 (3.14)	.965 (6.08)	.537 (3.09)	.064 (.344)
Associates/ Vocational/Trade	1.41 (5.94)	1.24 (4.86)	.920 (3.41)	1.04 (5.81)	.609 (3.10)	-.069 (-.325)
Bachelor's degree	1.96 (9.23)	1.76 (7.74)	1.11 (4.27)	1.59 (8.47)	1.16 (5.75)	.445 (1.89)
Advanced degree	1.78 (6.79)	1.57 (5.73)	.989 (3.21)	1.40 (5.28)	1.07 (3.85)	.122 (.381)
Part-time job	-1.30 (-7.40)	-1.34 (-7.51)	-1.11 (-5.51)	-1.90 (-15.35)	-1.98 (-15.76)	-1.73 (-12.65)
Married	.237 (1.71)	.268 (1.92)	.233 (1.56)	.016 (.138)	.069 (.598)	-.023 (-.189)
# children <18	-.060 (-.968)	-.034 (-.539)	-.015 (-.224)	-.046 (-.930)	-.042 (-.830)	-.036 (-.688)
Prior experience	.010 (.887)	.008 (.707)	.004 (.277)	.022 (2.20)	.022 (2.17)	.022 (2.04)
Tenure	.212 (8.62)	.206 (8.33)	.219 (8.18)	.205 (9.43)	.207 (9.40)	.220 (9.18)
Tenure ²	-.005 (-4.82)	-.004 (-4.75)	-.005 (-5.08)	-.005 (-6.91)	-.005 (-6.88)	-.006 (-7.14)
Unionized	1.24 (7.89)	1.29 (7.98)	1.54 (8.87)	1.01 (6.82)	.969 (6.51)	1.20 (7.33)
Ln (firm size)	.425 (11.77)	.421 (11.54)	.353 (8.88)	.387 (14.10)	.394 (14.16)	.427 (13.70)

Table 6 (cont'd)

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Speaks English well	--	-.691 (-3.92)	-.577 (-3.11)	--	.089 (.528)	.133 (.176)
Does not speak English well	--	-.789 (-3.48)	-.658 (-2.76)	--	-.706 (-2.97)	-.246 (.252)
Does not speak English	--	.387 (.860)	.989 (1.86)	--	-1.04 (-2.82)	-.700 (.403)
U.S. citizen by nationalization	--	-.216 (-.740)	-.430 (-1.38)	--	-.550 (-2.97)	-.542 (.194)
Not a U.S. citizen	--	-.177 (-.940)	-.145 (-.723)	--	-.749 (-3.59)	-.692 (.219)
Supervise others	--	--	-.104 (-.640)	--	--	.294 (.137)
Supervise and set the pay of others	--	--	-.210 (-.921)	--	--	-.299 (.257)
<u>Daily job tasks:</u>						
Face-to-face customer contact	--	--	-.070 (-.500)	--	--	-.098 (.137)
Talk w/ customers on telephone	--	--	.063 (.363)	--	--	.122 (.135)
Reading paragraphs	--	--	.680 (4.61)	--	--	.265 (.130)
Writing paragraphs	--	--	.177 (1.12)	--	--	.150 (.136)
Work w/ computer	--	--	.904 (5.66)	--	--	.478 (.137)
Arithmetic	--	--	-.299 (-2.06)	--	--	-.256 (.120)
Occupation controls:	No	No	Yes	No	No	Yes
Industry controls:	No	No	Yes	No	No	Yes
N	1,369	1,367	1,354	1,815	1,815	1,805
Pseudo R ²	.202	.210	.252	.217	.227	.250

Note: Z-statistics of regression estimates are reported in parentheses. The omitted categories are "high school dropout" for educational attainment, "speaks English very well" for English fluency, and "US citizen by birth" for citizenship. All regressions include a constant and control for metropolitan area. Sample sizes differ between regressions due to missing values.

Table 7
Ordered Logit Estimates from Job Benefits Models:
The Impact of Job Segregation on the Number of Benefits

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-.018 (-.086)	.118 (.542)	.272 (1.19)	-.370 (-2.04)	-.382 (-2.10)	-.439 (-2.28)
Hispanic	-.046 (-.252)	.291 (1.43)	.277 (1.28)	-.406 (-2.42)	.069 (.371)	-.041 (-.209)
Coworkers are Predominantly:						
Black	.116 (.443)	.057 (.215)	.316 (1.13)	.299 (1.35)	.310 (1.38)	.494 (2.07)
Hispanic	-.735 (-4.15)	-.621 (-3.39)	-.317 (-1.64)	-.111 (-.640)	.142 (.786)	.420 (2.19)
Asian	-1.61 (-3.52)	-1.68 (-3.86)	-1.88 (-4.46)	.919 (2.86)	1.00 (3.07)	1.07 (3.17)
Other (or mixed)	-.215 (-1.01)	-.245 (-1.13)	-.477 (-1.99)	1.22 (5.67)	1.23 (5.66)	1.10 (4.93)
N	1,360	1,358	1,346	1,802	1,802	1,792
Pseudo R ²	.208	.215	.259	.227	.237	.259

Note: Included controls correspond to those listed in Table 6. Z-statistics of regression estimates are reported in parentheses. Sample sizes differ between regressions due to missing values.

Table 8
Probit Estimates of the Effect of Job Segregation on Benefits

	<u>Retirement</u>		<u>Sick Leave</u>		<u>Personal Health</u>		<u>Family Health</u>	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
A. Males								
Black	.106 (.050)	.133 (.050)	.055 (.045)	.101 (.042)	-.098 (.047)	-.057 (.044)	-.134 (.055)	-.034 (.058)
Hispanic	.065 (.047)	.086 (.056)	.015 (.042)	.167 (.045)	-.070 (.037)	-.016 (.037)	-.080 (.048)	.001 (.058)
<u>Coworkers are Predominantly:</u>								
Black	-.069 (.070)	-.036 (.075)	-.018 (.061)	.041 (.056)	.003 (.045)	.034 (.037)	.034 (.064)	.055 (.067)
Hispanic	-.128 (.049)	-.102 (.053)	-.082 (.044)	.039 (.043)	-.089 (.037)	-.015 (.033)	-.219 (.048)	-.164 (.053)
Asian	-.314 (.118)	-.287 (.135)	-.418 (.101)	-.479 (.108)	-.297 (.113)	-.306 (.132)	-.402 (.095)	-.499 (.088)
Other (or mixed)	-.018 (.062)	-.065 (.070)	.055 (.046)	.069 (.047)	.022 (.039)	-.012 (.043)	-.041 (.058)	-.055 (.066)
N	1423	1407	1436	1420	1436	1420	1375	1361
Pseudo R ²	.332	.397	.247	.327	.300	.373	.288	.373
B. Females								
	<u>Retirement</u>	<u>Sick Leave</u>	<u>Personal Health</u>	<u>Family Health</u>				
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Black	-.008 (.049)	-.025 (.052)	-.074 (.046)	-.079 (.049)	-.099 (.049)	-.146 (.052)	-.063 (.049)	-.095 (.051)
Hispanic	-.056 (.048)	-.017 (.056)	-.100 (.044)	-.028 (.050)	-.124 (.046)	-.047 (.053)	-.119 (.047)	.0001 (.055)
<u>Coworkers are Predominantly:</u>								
Black	.017 (.061)	.036 (.064)	.007 (.054)	.040 (.055)	.100 (.048)	.115 (.048)	.107 (.059)	.152 (.063)
Hispanic	-.021 (.049)	.055 (.052)	-.090 (.045)	-.013 (.048)	.054 (.042)	.140 (.041)	.058 (.049)	.211 (.052)
Asian	.173 (.078)	.222 (.075)	.119 (.070)	.112 (.073)	.246 (.045)	.270 (.033)	.409 (.052)	.453 (.049)
Other (or mixed)	.091 (.056)	.043 (.061)	.124 (.044)	.105 (.046)	.270 (.026)	.262 (.026)	.317 (.044)	.303 (.050)
N	1884	1874	1897	1887	1899	1889	1818	1802
Pseudo R ²	.375	.413	.277	.333	.340	.397	.328	.400

Note: Table reports dF/dx for a discrete change of dummy variables from 0 to 1, with standard errors in parentheses. Included controls for model (1) correspond to those listed in Table 6, column (1); while those for model (2) above correspond to those listed in Table 6, column (3). Sample sizes differ between regression due to missing values.

Table 9
Characteristics of Minorities by Job Segregation

	Blacks		Hispanics	
	<u>Segregated</u>	<u>Not Segregated</u>	<u>Segregated</u>	<u>Not Segregated</u>
Residential Segregation (% own race/ethnicity)	.596	.415	.606	.451
Commute time (minutes)	29.15	32.51	25.80	27.26
Face-to-face customer contact	.669	.625	.413	.550
<u>Education:</u>				
No high school degree	.092	.049	.542	.215
High school/GED	.505	.481	.338	.367
Assoc./Voc./Trade	.210	.212	.058	.224
Bachelor's degree	.156	.167	.053	.157
Advanced degree	.038	.091	.009	.037
Male	.428	.490	.572	.531
Part-time job	.171	.202	.188	.202
Unionized	.182	.290	.120	.252
Ln (firm size)	3.46	4.80	3.06	3.96
Atlanta	.376	.260	.002	.038
Boston	.078	.151	.027	.097
Los Angeles	.545	.589	.971	.865
<u>Industry:</u>				
Construction	.049	.022	.092	.061
Durable manufacturing	.039	.091	.160	.059
Non-durable mfg.	.029	.055	.138	.091
Transportation	.090	.112	.050	.103
Wholesale/retail trade	.146	.124	.227	.182
Finance/Insurance/ real estate	.089	.099	.038	.047
Service	.503	.413	.261	.413
Public/self employed/ Other	.090	.099	.119	.101

Table 9 (cont'd)

	Blacks		Hispanics	
	<u>Segregated</u>	<u>Not Segregated</u>	<u>Segregated</u>	<u>Not Segregated</u>
<u>Occupation:</u>				
Managerial	.099	.067	.042	.108
Prof./technical	.050	.234	.039	.160
Sales	.086	.110	.060	.087
Clerical/admin.	.203	.264	.092	.172
Service/labor	.309	.205	.330	.184
Crafts/operative	.130	.103	.423	.270
Self employed/other	.009	.010	.015	.013
<u>Speaks English:</u>				
Very well	--	--	.258	.577
Well	--	--	.195	.255
Not well	--	--	.457	.165
Not at all	--	--	.089	.003
<u>Citizenship status:</u>	--	--		
By birth in U.S.	--	--	.198	.465
By naturalization	--	--	.064	.171
Not a citizen	--	--	.738	.365
Supervise others	.233	.314	.196	.250
Supervise and set the pay of others	.057	.074	.064	.063

Table 10
Determinants of Job Segregation:
Probit Estimates of Marginal Effects

	Blacks		Hispanics		
	(1)	(2)	(3)	(4)	(5)
Residential Segregation	.391 (.046)	.406 (.047)	.212 (.053)	.135 (.054)	.143 (.059)
Commute Time	-.001 (.0006)	-.002 (.0006)	-.002 (.0007)	-.002 (.0007)	-.002 (.0008)
Face-to-face customer contact	.017 (.031)	.010 (.033)	-.071 (.030)	-.029 (.030)	.028 (.034)
Male	-.024 (.030)	-.019 (.033)	.032 (.029)	.044 (.029)	.023 (.034)
High school/GED	-.098 (.061)	-.112 (.060)	-.147 (.035)	-.028 (.038)	-.016 (.041)
Associates/ Vocational/Trade	-.089 (.063)	-.086 (.063)	-.445 (.050)	-.301 (.060)	-.251 (.066)
Bachelor's degree	-.090 (.065)	-.107 (.066)	-.378 (.059)	-.245 (.065)	-.190 (.073)
Advanced degree	-.204 (.061)	-.172 (.068)	-.435 (.109)	-.328 (.127)	-.178 (.136)
Part-time job	-.014 (.041)	.040 (.045)	-.071 (.040)	-.085 (.040)	-.053 (.043)
Unionized	-.066 (.034)	-.056 (.034)	-.100 (.042)	-.084 (.042)	-.064 (.043)
Ln (firm size)	-.072 (.007)	-.051 (.008)	-.054 (.008)	-.048 (.008)	-.044 (.010)
Atlanta	.024 (.035)	.072 (.036)	-.520 (.156)	-.530 (.171)	-.548 (.173)
Boston	-.185 (.039)	-.162 (.039)	-.251 (.077)	-.216 (.083)	-.265 (.088)
Speaks English well	--	--	--	-.006 (.038)	-.025 (.041)
Does not speak English well	--	--	--	.150 (.038)	.139 (.042)
Does not speak English	--	--	--	.244 (.030)	.237 (.035)

Table 10 (cont'd)

	Blacks		Hispanics		
	(1)	(2)	(3)	(4)	(5)
U.S. citizen by nationalization	--	--	--	-.004 (.055)	-.030 (.060)
Not a U.S. citizen	--	--	--	.114 (.042)	.072 (.043)
Supervise others	--	-.105 (.034)	--	--	-.045 (.043)
Supervise and set the pay of others	--	.0002 (.069)	--	--	.081 (.060)
Industry controls included:	No	Yes	No	No	Yes
Occupation controls included:	No	Yes	No	No	Yes
N	1280	1250	1144	1144	1087
Pseudo R ²	.150	.167	.202	.246	.279

Note: Dependant variable is a dummy equal to one if the individual is black and works with mostly blacks or Hispanic and works with mostly Hispanics. The omitted categories are "high school dropout" for educational attainment, "speaks English very well" for English fluency, and "US citizen by birth" for citizenship. Standard errors of regression estimates are reported in parentheses. For dummy variables, dF/dx is for a discrete change from 0 to 1.

Table 11
Log Hourly Wage Regressions:
The Impact of Job Segregation Controlling for Family Background

	Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-.070 (.061)	-.080 (.065)	-.047 (.054)	-.079 (.050)	-.088 (.050)	-.069 (.045)
Hispanic	-.162 (.060)	-.159 (.066)	-.124 (.051)	-.068 (.076)	-.002 (.082)	-.075 (.063)
Coworkers are Predominantly:						
Black	-.131 (.082)	-.132 (.084)	-.146 (.064)	-.085 (.061)	-.085 (.061)	-.078 (.063)
Hispanic	-.222 (.072)	-.186 (.073)	-.134 (.057)	-.289 (.071)	-.276 (.073)	-.185 (.061)
N	1172	1170	1046	1401	1401	1330
R ²	.492	.501	.583	.480	.491	.635

Note: Included controls correspond to those listed in Table 4, along with father's education, mother's education (in years), and indicators of whether father usually worked and mother usually worked (when respondent was age sixteen), and whether respondent lived with both parents (most of the time until age sixteen). Robust standard errors of regression estimates are reported in parentheses. Sample sizes differ due to missing values. Coefficient estimates on dummy variables for working with mostly Asians and "other (or mixed)" available upon request.

Table 12
Log Hourly Wage Regressions:
The Impact of Job Segregation by Demographic Group
Controlling for Family Background

A. Blacks	Males		Females	
	(1)	(2)	(1)	(2)
Coworkers are				
<u>Predominantly:</u>				
Black	-.175 (.088)	-.166 (.064)	-.088 (.049)	-.100 (.048)
Hispanic	-.202 (.143)	-.037 (.091)	-.035 (.075)	.009 (.072)
N	292	271	543	526
R ²	.503	.658	.537	.646
B. Hispanics	Males		Females	
	(1)	(2)	(1)	(2)
Coworkers are				
<u>Predominantly:</u>				
Black	-.394 (.193)	-.333 (.111)	-.108 (.128)	-.015 (.152)
Hispanic	-.191 (.081)	.115 (.078)	-.128 (.094)	-.131 (.071)
N	384	354	379	360
R ²	.437	.637	.522	.725
C. Whites	Males		Females	
	(1)	(2)	(1)	(2)
Coworkers are				
<u>Predominantly:</u>				
Black	-.007 (.122)	-.047 (.122)	-.003 (.174)	.011 (.156)
Hispanic	-.134 (.124)	-.056 (.090)	-.635 (.125)	-.386 (.101)
N	496	423	479	444
R ²	.453	.548	.415	.611

Note: Included controls correspond to those listed in Table 3, along with father's education, mother's education (in years), and indicators of whether father usually worked and mother usually worked (when respondent was age sixteen), and whether respondent lived with both parents (most of the time until age sixteen). Robust standard errors of regression estimates are reported in parentheses. Sample sizes differ due to missing values. Coefficient estimates on dummy variables for working with mostly Asians and "other (or mixed)" available upon request.

Table 13
The Impact of Job Segregation and Residential Segregation on Wages
by Demographic Group

A. Males	Black		Hispanic		White	
	(1)	(2)	(1)	(2)	(1)	(2)
Coworkers are						
<u>Predominantly:</u>						
Black	-.199 (.081)	-.149 (.057)	-.345 (.158)	-.330 (.097)	.042 (.112)	.055 (.117)
Hispanic	-.178 (.129)	-.068 (.092)	-.180 (.075)	-.095 (.073)	-.087 (.118)	-.018 (.084)
<u>Proportion of</u>						
<u>Neighborhood:</u>						
Black	-.106 (.125)	-.001 (.095)	-.122 (.125)	-.053 (.105)	-.093 (.217)	-.030 (.182)
Hispanic	-.392 (.244)	-.077 (.170)	-.168 (.100)	-.107 (.083)	-.672 (.254)	-.333 (.175)
N	447	413	580	537	559	479
R ²	.478	.609	.435	.583	.421	.515
B. Females	Black		Hispanic		White	
	(1)	(2)	(1)	(2)	(1)	(2)
Coworkers are						
<u>Predominantly:</u>						
Black	-.062 (.047)	-.064 (.047)	-.052 (.083)	-.245 (.097)	.036 (.132)	.089 (.117)
Hispanic	.117 (.179)	.086 (.090)	-.161 (.084)	-.148 (.058)	-.466 (.128)	-.296 (.104)
<u>Proportion of</u>						
<u>Neighborhood:</u>						
Black	-.315 (.132)	-.248 (.084)	.114 (.107)	.080 (.102)	-.112 (.225)	.012 (.154)
Hispanic	-.726 (.224)	-.523 (.155)	.048 (.116)	.068 (.089)	-.451 (.224)	-.463 (.171)
N	842	820	576	544	574	532
R ²	.536	.649	.470	.675	.400	.598

Note: Included controls correspond to those included in the models depicted in Table 5. Robust standard errors reported in parentheses. Sample sizes differ between regressions due to missing values.

Table 14
The Impact of Job Segregation on Wages by Demographic Group
Controlling for Neighborhood Effects

A. Males	Black		Hispanic		White	
	(1)	(2)	(1)	(2)	(1)	(2)
Coworkers are						
<u>Predominantly:</u>						
Black	-.202 (.082)	-.146 (.058)	-.382 (.158)	-.341 (.098)	.046 (.115)	.041 (.122)
Hispanic	-.164 (.134)	-.064 (.092)	-.200 (.075)	-.097 (.076)	-.120 (.118)	-.020 (.083)
Neighborhood at least 50% minority	-.056 (.081)	-.042 (.059)	-.035 (.056)	-.031 (.050)	-.228 (.086)	-.093 (.077)
Short commute (≤ 10 minutes)	-.076 (.133)	-.045 (.096)	.126 (.104)	.035 (.073)	.008 (.082)	-.069 (.080)
Minority neighborhood x short commute	.137 (.167)	.071 (.125)	-.125 (.119)	-.042 (.099)	-.064 (.149)	-.133 (.169)
N	447	413	580	537	559	479
R ²	.474	.610	.437	.582	.410	.514
B. Females	Black		Hispanic		White	
	(1)	(2)	(1)	(2)	(1)	(2)
Coworkers are						
<u>Predominantly:</u>						
Black	-.074 (.047)	-.074 (.044)	-.066 (.082)	-.270 (.097)	.003 (.130)	.075 (.114)
Hispanic	.039 (.145)	.030 (.080)	-.156 (.089)	-.153 (.058)	-.488 (.125)	-.320 (.105)
Neighborhood at least 50% minority	-.012 (.056)	-.016 (.050)	.124 (.064)	.103 (.049)	-.146 (.097)	-.139 (.090)
Short commute (≤ 10 minutes)	.434 (.182)	.335 (.012)	.113 (.095)	.067 (.066)	.287 (.120)	.201 (.118)
Minority neighborhood x short commute	-.434 (.181)	-.341 (.112)	-.108 (.138)	-.053 (.090)	-.093 (.052)	-.029 (.046)
N	842	820	576	544	574	532
R ²	.541	.655	.478	.680	.400	.593

Note: Included controls correspond to those included in the models depicted in Table 5. Robust standard errors reported in parentheses. Sample sizes differ between regressions due to missing values.

Table 15
Measures of Racial Attitudes in Job Segregation Models:
Probit Estimates of Marginal Effects

Panel A	Blacks		Hispanics		
	(1)	(2)	(3)	(4)	(5)
Self Selection	.073 (.054)	.082 (.057)	.053 (.040)	.010 (.042)	.047 (.047)
N	569	555	665	665	631
Pseudo R ²	.184	.220	.233	.287	.369
 Panel B					
	Blacks		Hispanics		
	(1)	(2)	(3)	(4)	(5)
Ease of Getting Along with (scale 1-7):					
Blacks	-.005 (.017)	.001 (.017)	-.026 (.011)	-.009 (.011)	-.017 (.012)
Hispanics	-.026 (.015)	-.031 (.016)	-.030 (.012)	-.036 (.013)	-.023 (.014)
Whites	-.012 (.011)	-.006 (.011)	-.005 (.009)	-.003 (.009)	-.001 (.010)
N	569	555	665	665	631
Pseudo R ²	.194	.227	.250	.300	.375

Note: Dependant variable is a dummy equal to one if individual is black and works with mostly blacks or Hispanic and works with mostly Hispanics. Additional controls correspond to those listed in Table 10. Standard errors of regression estimates are reported in parentheses. For dummy variables, dF/dx is for a discrete change from 0 to 1.

