

NBER WORKING PAPER SERIES

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EVIDENCE FROM MATCHED EMPLOYER-EMPLOYEE DATA

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Working Paper 13080  
<http://www.nber.org/papers/w13080>

NATIONAL BUREAU OF ECONOMIC RESEARCH  
1050 Massachusetts Avenue  
Cambridge, MA 02138  
May 2007

This research was funded by NICHD grant R01HD042806. We also thank the Alfred P. Sloan Foundation for its generous support. We are grateful to Ron Jarmin and Julia Lane for helpful comments. The analysis and results presented in this paper are attributable to the authors and do not necessarily reflect concurrence by the Center for Economics Studies, Bureau of the Census, or by the Sloan Foundation. This paper has undergone a more limited review by the Census Bureau than its official publications. It has been screened to ensure that no confidential data are revealed. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

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Changes in Workplace Segregation in the United States between 1990 and 2000: Evidence from Matched Employer-Employee Data

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NBER Working Paper No. 13080

May 2007

JEL No. J15,J16,J71

### **ABSTRACT**

We present evidence on changes in workplace segregation by education, race, ethnicity, and sex, from 1990 to 2000. The evidence indicates that racial and ethnic segregation at the workplace level remained quite pervasive in 2000. At the same time, there was fairly substantial segregation by skill, as measured by education. Putting together the 1990 and 2000 data, we find no evidence of declines in workplace segregation by race and ethnicity; indeed, black-white segregation increased. Over this decade, segregation by education also increased. In contrast, workplace segregation by sex fell over the decade, and would have fallen by more had the services industry - a heavily female industry in which sex segregation is relatively high - not experienced rapid employment growth.

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## **I. Introduction**

In recent work we have constructed and described the 1990 Decennial Employer-Employee Dataset (DEED) based on matching records in the 1990 Decennial Census of Population to a Census Bureau list of most business establishments in the United States. We have used the 1990 DEED to estimate earnings and productivity differentials in manufacturing by demographic and skill group (Hellerstein and Neumark, 2004), to study the influence of language skills on workplace segregation and wages (Hellerstein and Neumark, 2003), to document the extent of workplace segregation by race and ethnicity and to assess the contribution of residential segregation as well as skill to this segregation (Hellerstein and Neumark, forthcoming).

We just recently completed the construction of the 2000 Beta-DEED<sup>1</sup> (based on the 2000 Census of Population). In this paper, we use the 1990 and 2000 DEEDs to measure changes in establishment-level workplace segregation over the intervening decade, an analysis for which the DEEDs are uniquely well-suited. We study segregation by education, by race and Hispanic ethnicity, and by sex. With respect to segregation by race and ethnicity, this work is complementary to a flurry of research studying changes in residential segregation from 1990 to 2000 (Glaeser and Vigdor, 2001; Iceland and Weinberg, 2002; and McConville et al., 2001). As we have suggested elsewhere (and see Estlund, 2003), however, workplace segregation may be far more salient for interactions between racial and ethnic groups than is residential segregation. The boundaries used in studying residential segregation may not capture social interactions, and are to some extent explicitly drawn to accentuate segregation among different groups; for example, Census tract boundaries are often generated in order to ensure that the tracts are “as homogeneous as possible with respect to population characteristics, economic status, and living

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<sup>1</sup> The 2000 Beta-DEED is an internal U.S. Census Bureau dataset that will ultimately become part of an integrated matched employer-employee database at the U.S. Census Bureau. The new integrated data will have characteristics of the Decennial Employer-Employee Database (DEED) and the Longitudinal Employer-Household Dynamics Program (LEHD). Hereafter, the 2000 Beta-DEED will be referred to as the 2000 DEED.

conditions.”<sup>2</sup> In contrast, workplaces – specifically establishments – are units of observation that are generated by economic forces and in which people clearly do interact in a variety of ways, including work, social activity, labor market networks, etc. Thus, while it is more difficult to study workplace segregation because of data constraints, measuring workplace segregation may be more useful than measuring residential segregation, as traditionally defined, for describing the interactions that arise in society between different groups in the population.<sup>3</sup> Of course similar arguments to those about workplaces could be made about other settings, such as schools, religious institutions, etc. (e.g., James and Taeuber, 1985), but data constraints truly prevent saying much of anything about segregation along these lines.

Segregation is potentially important for a number of reasons. Aside from general social issues regarding integration between different groups, labor market segregation by race and ethnicity accounts – at least in a statistical sense – for a sizable share of wage gaps between white males and other demographic groups (e.g., Carrington and Troske, 1998a; Bayard et al., 1999; King, 1992; Watts, 1995; Higgs, 1977), and the same is true of labor market segregation by sex (Bayard et al., 2003; Blau, 1977; and Groshen, 1991).<sup>4</sup> There has generally been less attention paid to segregation by education, but in our earlier work (Hellerstein and Neumark, forthcoming), we documented rather extensive segregation by education (as well as language, which we do not consider in the present paper) in the 1990 DEED.

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<sup>2</sup> U.S. Census Bureau, [www.census.gov/geo/www/GARM/Ch10GARM.pdf](http://www.census.gov/geo/www/GARM/Ch10GARM.pdf) (viewed April 27, 2005). Echenique and Fryer (2005) develop a segregation index that relies much less heavily on ad-hoc definitions of geographical boundaries.

<sup>3</sup> Moreover, industry code, the closest proxy in public-use data to an establishment identifier, is a very crude measure to use to examine segregation. For example, we calculate that racial and ethnic segregation at the three-digit industry level in the DEED is typically on the order of one-third as large as the establishment-level segregation we document below.

<sup>4</sup> This segregation may occur along industry and occupation lines, as well as at the more detailed level of the establishment or job cell (occupations within establishments). For example, Bayard et al. (1999) found that, for men, job cell segregation by race accounts for about half of the black-white wage gap and a larger share of the Hispanic-white wage gap.

Measuring changes in workplace segregation along these lines is of interest for a number of reasons. First, although much attention has been paid to changes in residential segregation – of which there is evidence of modest declines from 1990 to 2000 – changes in workplace segregation may be more salient to understanding changing social forces. Second, aside from the relative importance of workplace and residential segregation, in the United States there are extensive efforts to reduce labor market discrimination, and therefore measuring changes in workplace segregation is one indicator of the success of these efforts. Finally, increases in the productivity (and pay) of more-educated workers relative to less-educated workers may have led to increased segregation by skill (e.g., Kremer and Maskin, 1996).<sup>5</sup> A comparison of education segregation between 1990 and 2000 possibly can shed some light on this hypothesis, although relatively more of the run-up in wage inequality occurred prior to 1990 (Autor et al., 2005).

We measure changes in segregation using the 1990 and 2000 Decennial Employer-Employee Databases (DEEDs). For each year, the DEED is based on matching records in the Decennial Census of Population for that year to a Census Bureau list of most business establishments in the United States. The matching yields data on multiple workers matched to establishments, providing the means to measure workplace segregation (and changes therein) in the United States based on a large, fairly representative data set. In addition, the data from the Decennial Census of Population provides the necessary information on race, ethnicity, etc. Thus, data from the 1990 and 2000 DEEDs provides unparalleled opportunities to study changes in workplace segregation by skill, race, ethnicity, and sex.<sup>6</sup>

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<sup>5</sup> For example, let the production function be  $f(L_1, L_2) = L_1^c L_2^d$ , with  $d > c$ . Assume that there are two types of workers: unskilled workers ( $L_1$ ) with labor input equal to one efficiency unit, and skilled workers ( $L_2$ ) with efficiency units of  $q > 1$ . Kremer and Maskin show that for low  $q$ , it is optimal for unskilled and skilled workers to work together, but above a certain threshold of  $q$  (that is, a certain amount of skill inequality), the equilibrium will reverse, and workers will be sorted across firms according to skill. Thus, as the returns to education rise ( $q$  increases), there may be increased segregation by education.

<sup>6</sup> Carrington and Troske (1998a, 1998b) use data sets much more limited in scope than the ones we use here to examine workplace segregation by race and sex. In general, the paucity of research on workplace segregation is presumably a function of the lack of data linking workers to establishments.

## **II. The 1990 and 2000 DEEDs**

The analysis in this paper is based on the 1990 and 2000 DEEDs, which we have created at the Center for Economic Studies at the U.S. Bureau of the Census. We have described the construction of the 1990 DEED in detail elsewhere (in particular, Hellerstein and Neumark, 2003). The construction of the 2000 DEED follows the same procedures, and our detailed investigation of the 2000 data thus far has indicated that no new serious problems arise that require different methods for 2000. Thus, in this section we simply provide a quick overview of the construction of the datasets.

The DEED for each year is formed by matching workers to establishments. The workers are drawn from the Sample Edited Detail File (SEDF), which contains all individual responses to the Decennial Census of Population one-in-six Long Form. The establishments are drawn from the Census Bureau's Business Register list (BR), formerly known as the Standard Statistical Establishment List or SSEL; the BR is a database containing information for all business establishments operating in the United States in each year, which is continuously updated (see Jarmin and Miranda, 2002).

Households receiving the Decennial Census Long Form were asked to report the name and address of the employer in the previous week for each employed member of the household. The file containing this employer name and address information is referred to as the "Write-In" file, which contains the information written on the questionnaires by Long-Form respondents, but not actually captured in the SEDF. The BR is a list of all business establishments with one or more employees operating in the United States. The Census Bureau uses the BR as a sampling frame for its Economic Censuses and Surveys, and continuously updates the information it contains. The BR contains the name and address of each establishment, geographic codes based on its location, its four-digit SIC code, and an identifier that allows the establishment to be linked to other establishments that are part of the same enterprise, and to other Census Bureau establishment- or firm-level data sets that contain more detailed employer characteristics. We can therefore use employer names and addresses for each worker in the Write-In file to match the Write-In file to the BR. Because the name and address information on the Write-In file is also available for virtually all employers in the BR, nearly all of the establishments in the

BR that are classified as “active” by the Census Bureau are available for matching. Finally, because both the Write-In file and the SEDF contain identical sets of unique individual identifiers, we can use these identifiers to link the Write-In file to the SEDF. Thus, this procedure yields a very large data set with workers matched to their establishments, along with all of the information on workers from the SEDF.

Matching workers and establishments is a difficult task, because we would not expect employers’ names and addresses to be recorded identically on the two files. To match workers and establishments based on the Write-In file, we use MatchWare – a specialized record linkage program. MatchWare is comprised of two parts: a name and address standardization mechanism (AutoStan); and a matching system (AutoMatch). This software has been used previously to link various Census Bureau data sets (Foster et al., 1998). Our method to link records using MatchWare involves two basic steps. The first step is to use AutoStan to standardize employer names and addresses across the Write-In file and the BR. Standardization of addresses in the establishment and worker files helps to eliminate differences in how data are reported. The standardization software considers a wide variety of different ways that common address and business terms can be written, and converts each to a single standard form.

Once the software standardizes the business names and addresses, each item is parsed into components. The value of parsing the addresses into multiple pieces is that we can match on various combinations of these components. We supplemented the AutoStan software by creating an acronym for each company name, and added this variable to the list of matching components.<sup>7</sup>

The second step of the matching process is to select and implement the matching specifications. The AutoMatch software uses a probabilistic matching algorithm that accounts for missing information, misspellings, and even inaccurate information. This software also permits users to control which matching variables to use, how heavily to weight each matching variable, and how similar two addresses must be in order to constitute a match. AutoMatch is designed to compare match criteria in a succession of “passes” through the data. Each pass is comprised of “Block” and “Match” statements. The Block

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<sup>7</sup> For 2000, we also added standard acronyms or abbreviations for cities, such as NY or NYC and LA. However, this added a negligible number of additional matches, so we did not go back and do the same for the 1990 DEED.

statements list the variables that must match exactly in that pass in order for a record pair to be linked. In each pass, a worker record from the Write-In file is a candidate for linkage only if the Block variables agree completely with the set of designated Block variables on analogous establishment records in the BR. The Match statements contain a set of additional variables from each record to be compared. These variables need not agree completely for records to be linked, but are assigned weights based on their value and reliability.

For example, we might assign “employer name” and “city name” as Block variables, and assign “street name” and “house number” as Match variables. In this case, AutoMatch compares a worker record only to those establishment records with the same employer name and city name. All employer records meeting these criteria are then weighted by whether and how closely they agree with the worker record on the street name and house number Match specifications. The algorithm applies greater weights to items that appear infrequently. The employer record with the highest weight will be linked to the worker record conditional on the weight being above some chosen minimum. Worker records that cannot be matched to employer records based on the Block and Match criteria are considered residuals and we attempt to match these records on subsequent passes using different criteria.

It is clear that different Block and Match specifications may produce different sets of matches. Matching criteria should be broad enough to cover as many potential matches as possible, but narrow enough to ensure that only matches that are correct with a high probability are linked.<sup>8</sup> Because the AutoMatch algorithm is not exact there is always a range of quality of matches, and we therefore are cautious in accepting linked record pairs. Our general strategy is to impose the most stringent criteria in the earliest passes, and to loosen the criteria in subsequent passes, while always maintaining criteria that

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<sup>8</sup> One might also consider trying to impute matches where this strategy fails, by matching based on imputed place of work instead of information in the Write-In file. However, this turns out to be problematic. Even imputing place of work at the level of the census tract is not easy. For example, there are workers in the SEDF that we are able to match to an employer in the DEED using name and address information whose place of work code actually is allocated in the SEDF. For these workers, the allocated census tract in the SEDF disagrees with the BR census tract of the matched establishment in more than half the cases.



err on the side of avoiding false matches. We choose matching algorithms based on substantial experimentation and visual inspection of many thousands of records.

The final result is an extremely large data set, for each year, of workers matched to their establishment of employment. The 1990 DEED consists of information on 3.29 million workers matched to around 972,000 establishments, accounting for 27.1 percent of workers in the SEDF and 18.6 percent of establishments in the BR. The 2000 DEED consists of information on 4.09 million workers matched to around 1.28 million establishments, accounting for 29.1 percent of workers in the SEDF and 22.6 percent of establishments in the BR.<sup>9</sup>

In Table 1 we provide descriptive statistics for the matched workers from the DEED as compared to the SEDF. Columns (1) and (4) report summary statistics for the SEDF for the sample of workers who were eligible to be matched to their establishments, for 1990 and 2000 respectively. Columns (2) and (5) report summary statistics for the full DEED sample. For both years, the means of the demographic variables in the full DEED are quite close to the means in the SEDF across most dimensions. For example, for the 1990 data, female workers comprise 46 percent of the SEDF and 47 percent of the full DEED, and the number of children (for women) is 0.75 in the SEDF and 0.73 in the DEED. Nonetheless, there are cases of somewhat larger differences. Race and ethnic differences are larger in both years; for example, in 2000 the percent white is 78 in the SEDF versus 83 in the DEED, and correspondingly the share black (and also Hispanic) is lower in the DEED. In addition, the percent female in the 2000 data is 46 in the SEDF, but 50 in the DEED; this is different than the discrepancy in 1990 where the percent female is 46 in the SEDF and only a slightly higher 47 percent in the DEED.

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<sup>9</sup> For both the DEED and SEDF we have excluded individuals as follows: with missing wages; who did not work in the year prior to the survey year or in the reference week for the Long Form of the Census; who did not report positive hourly wages; who did not work in one of the fifty states or the District of Columbia (whether or not the place of work was imputed); who were self-employed; who were not classified in a state of residence; or who were employed in an industry that was considered “out-of-scope” in the BR. (Out-of-scope industries do not fall under the purview of Census Bureau surveys. They include many agricultural industries, urban transit, the U.S. Postal Service, private households, schools and universities, labor unions, religious and membership organizations, and government/public administration. The Census Bureau does not validate the quality of BR data for businesses in out-of-scope industries.)

Part of the explanation for differences in racial and ethnic representation that result from the matching process is that there are many individuals who meet our sample inclusion criteria but for whom the quality of the business address information in the Write-In file is poor, and race and ethnic differences in reporting account for part of the differences in representation. We suspect that the differences in business address information partially reflect weaker labor market attachment among minorities, suggesting that the segregation results we obtain might best be interpreted as measuring the extent of segregation among workers who have relatively high labor force attachment and high attachment to their employers.

The last eight rows of the table report on the industry distribution of workers. We do find some overrepresentation of workers in manufacturing – more so in 1990 when manufacturing comprised a larger fraction of workers to begin with in the SEDF. The reasons for this are given below when we discuss establishment-level data.

Columns (3) and (6) report summary statistics for the workers in the DEED who comprise the sample from which we calculate segregation measures. The sample size reductions relative to columns (2) and (5) arise for two reasons. First, for reasons explained in the methods section, we exclude workers who do not live and work in the same Metropolitan Statistical Area/Primary Metropolitan Statistical Area (MSA/PMSA). Second, we exclude workers who are the only workers matched to their establishments, as there are methodological advantages to studying segregation in establishments where we observe at least two workers. The latter restriction effectively causes us to restrict the sample to workers in larger establishments, which is the main reason why some of the descriptive statistics are slightly different between the second and third columns (for example, slightly higher wages and earnings in columns (3) and (6)).

In addition to comparing worker-based means, it is useful to examine the similarities across establishments in the BR and the DEED for each year. Table 2 shows descriptive statistics for establishments in each data set. As column (1) indicates, there are 5,237,592 establishments in the 1990 BR, and of these 972,436 (18.6 percent) also appear in the full DEED for 1990, as reported in column (2).

For 2000, the percentage in the full DEED is somewhat higher (22.6). Because only one in six workers are sent Decennial Census Long Forms, it is more likely that large establishments will be included in the DEED. One can see evidence of the bias toward larger employers by comparing the means across data sets for total employment. (This bias presumably also influences the distribution of workers and establishments across industries, where, for example, the DEEDs over-represent workers in manufacturing establishments.) On average, establishments in the BRs have 18-19 employees, while the average in the DEEDs is 49-53 workers. The distributions of establishments across industries in the DEED relative to the BR are similar to those for workers in the worker sample. In columns (3) and (6) we report descriptive statistics for establishments in the restricted DEEDs, corresponding to the sample of workers in columns (3) and (6) of Table 1. In general, the summary statistics are quite similar between columns (2) and (3) and between columns (5) and (6), with an unsurprising right shift in the size distribution of establishments. Overall, however, the DEED samples are far more representative than previous detailed matched data sets for the United States constructed using just the SEDF and the BR (see Hellerstein and Neumark, 2003).<sup>10</sup>

Because the DEED captures larger establishments and because our sample restrictions accentuate this, our analysis focuses on larger establishments. So, for example, the first quartile of the establishment size distribution for workers in our analysis is approximately 41 workers in 1990 and 36 in 2000, whereas the first quartile of the employment-weighted size distribution of all establishments in the BR for each year is 19 in 1990 and 21 in 2001.<sup>11</sup> Although we acknowledge that it would be nice to be able to measure segregation in all establishments, this is not the data set with which to do that convincingly. Nonetheless, most legislation aimed at combating discrimination is directed at larger establishments;

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<sup>10</sup> These earlier matched data sets—the Worker-Establishment Characteristics Database (WECD), which covers manufacturing only, and the New Worker-Establishment Characteristics Database (NWECD), which covers all industries—were smaller and less representative because the matching algorithm used could only be applied to establishments that were unique in a cell defined by detailed geographic information and industry classification. Thus, for example, manufacturing establishments were much more likely to occupy their own industry-location cell than were retail establishments.

<sup>11</sup> In order to adhere to U.S. Census Bureau confidentiality rules, these are “pseudo quartiles” based on averages of observations symmetrically distributed around the actual quartiles.

EEOC laws cover employers with 15 or more workers and affirmative action rules for federal contractors cover employers with 50 or more workers. Since policy has been directed at larger establishments, examining the extent of and changes in workplace segregation in larger establishments is important.

### **III. Methods**

We focus our analysis on a measure of segregation that is based on the percentages of workers in an individual's establishment, or workplace, in different demographic groups. Consider for clarity measuring segregation between white and Hispanic workers. For each white or Hispanic worker in our sample, we compute the percentage of Hispanic workers with which that worker works, excluding the worker him or herself. Because we exclude an individual's own ethnicity in this calculation, our analysis of segregation is conducted on establishments where we observe at least two workers.

We then average these percentages separately for white workers in our sample and for Hispanic workers. These averages are segregation measures commonly used in the sociology literature. The average percentage of co-workers in Hispanic workers' establishments who are Hispanic, denoted  $H_H$ , is called the "isolation index," and the average percentage of co-workers in white workers' establishments who are Hispanic, denoted  $W_H$ , is called the "exposure index." We focus more on a third measure, the difference between these, or

$$CW = H_H - W_H,$$

as a measure of "co-worker segregation."  $CW$  measures the extent to which Hispanics are more likely than are whites to work with other Hispanics. For example, if Hispanics and whites are perfectly segregated, then  $H_H$  equals 100,  $W_H$  is zero, and  $CW$  equals 100.<sup>12</sup>

We first report observed segregation, which is simply the sample mean of the segregation measure across workers. We denote this measure by appending an 'O' superscript to the co-worker segregation measure – i.e.,  $CW^O$ . One important point that is often overlooked in research on segregation,

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<sup>12</sup> We could equivalently define the percentages of white workers with which Hispanic or white workers work,  $H_W$  and  $W_W$ , which would simply be 100 minus these percentages, and  $CW' = W_W - H_W$ .

however, is that some segregation occurs even if workers are assigned randomly to establishments, and we are presumably most interested in the segregation that occurs systematically – i.e., that which is greater than would be expected to result from randomness (Carrington and Troske, 1997). Rather than considering all deviations from proportional representation across establishments as an “outcome” or “behavior” to be explained, we subtract from our measured segregation the segregation that would occur by chance if workers were distributed randomly across establishments, using Monte Carlo simulations to generate measures of randomly occurring segregation. We denote this random segregation  $CW^R$  (and similarly for the isolation and exposure indexes), and then focus on the difference  $\{CW^O - CW^R\}$ , which measures segregation above and beyond that which occurs randomly.<sup>13</sup> Although theoretically one can have  $CW^O < CW^R$  (that is, there is *less* segregation than would be generated randomly), or  $CW^O > CW^R$ , only the latter occurs in practice in our data. Again following Carrington and Troske, we scale this difference by the maximum segregation that can occur, or  $\{100 - CW^R\}$ , which we refer to as “effective segregation.” Thus, the effective segregation measure is:

$$[\{CW^O - CW^R\} / \{100 - CW^R\}] \cdot 100,$$

which measures the share of the maximum possible segregation that is actually observed.

There are two reasons that we exclude the worker’s own ethnicity when computing the fraction of Hispanics with which he or she works. First, this ensures that, in large samples of workers, if workers are randomly allocated across establishments,  $H_H$  and  $W_H$  both equal the share Hispanic in the population. That is, in the case of random allocation we expect to have  $CW^R$  equal to 0. This is a natural scaling to use, and stands in contrast to what happens when the worker is included in the calculations, where  $CW^R$  will exceed 0 because Hispanic workers are treated as working with “themselves.” Second, and perhaps more important, when the own worker is excluded our segregation measures are invariant to the sizes of establishments studied. To see this in a couple of simple examples, first consider a simple case of an

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<sup>13</sup> This distinction between comparing measured segregation to a no-segregation ideal or segregation that is generated by randomness is discussed in other work (see, e.g., Cortese et al., 1976; Winship, 1977; Boisso et al., 1994; and Carrington and Troske, 1997). Of course, to build  $CW^R$  we also compute the isolation and exposure indexes that would be generated in the case of random allocation of workers, and we report these as well.

economy with equal numbers of Hispanics and whites all working in two-person establishments.

Establishments can therefore be represented as HH (for two Hispanic workers), HW, or WW. With random allocation, 1/4 of establishments are HH, 1/2 are WH, and 1/4 are WW. Thus, excluding the own worker,  $H_H^R = (1/2) \cdot 1 + (1/2) \cdot 0 = 1/2$ ,  $W_H^R = (1/2) \cdot 1 + (1/2) \cdot 0 = 1/2$ , and  $CW^R = 0$ .<sup>14</sup> If we count the individual, then  $H_H^R = (1/2) \cdot 1 + (1/2) \cdot (1/2) = 3/4$ ,  $W_H^R = (1/2) \cdot (1/2) + (1/2) \cdot 0 = 1/4$ , and  $CW^R = 1/2$ . With three-worker establishments and random allocation, 1/8 of establishments are HHH (employing 1/4 of Hispanic workers), 1/8 are WWW (employing 1/4 of white workers), 3/8 are HWW (employing 1/4 of Hispanic and 1/2 of white workers), and 3/8 are HHW (employing 1/2 of Hispanic and 1/4 of white workers). Going through the same type of calculation as above, if we include the worker, then  $H_H^R = (1/4) \cdot 1 + (1/4) \cdot (1/3) + (1/2) \cdot (2/3) = 2/3$ ,  $W_H^R = (1/4) \cdot 0 + (1/4) \cdot (2/3) + (1/2) \cdot (1/3) = 1/3$  and  $CW^R = 1/3$ , whereas if we exclude the worker we again get  $H_H^R = 1/2$ ,  $W_H^R = 1/2$ , and  $CW^R = 0$ .

Although we just argued that in the case of random allocation Hispanics and whites should work with equal percentages of Hispanic co-workers on average (so that  $CW^R$  is zero), this result may not hold in parts of our analysis for two reasons. First, this is a large-sample result, and although the baseline sample size in our data set is large, the samples that we use to calculate some of our segregation measures are not necessarily large enough to generate this asymptotic result. Second, some of our segregation measures are calculated conditional on geography (in particular, MSA/PMSA of residence), for reasons explained below. When we condition on geography, we calculate the extent of segregation that would be expected if workers were randomly allocated across establishments within a geographic area. If Hispanics and whites are not evenly distributed across geographic borders, random allocation of workers within geographical areas still will yield the result that Hispanics are more likely to have Hispanic co-workers than are white workers, because for example, more Hispanics will come from areas where both whites and Hispanics work with a high share of Hispanic workers. For these reasons, in order to

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<sup>14</sup> For the first calculation, for example, 1/2 of Hispanic workers are in HH establishments, for which the share Hispanic is 1, and 1/2 are in WH establishments, for which the share Hispanic (excluding the worker) is 0.

determine how much segregation would occur randomly, in all cases we conduct Monte Carlo simulations of the extent of segregation that would occur with random allocation of workers.

There are, of course, other possible segregation measures, such as the traditional Duncan index (Duncan and Duncan, 1955) or the Gini coefficient. We prefer the co-worker segregation measure (CW) to these other measures for two reasons. First, the Duncan and Gini measures are scale invariant, meaning that they are insensitive to the proportions of each group in the workforce. For example, if the number of Hispanics doubles, but they are allocated to establishments in the same proportion as the original distribution, the Duncan and Gini indexes are unchanged. However, except in establishments that are perfectly segregated, the doubling of Hispanics leads each Hispanic worker in the sample to work with a larger percentage of Hispanic co-workers, and also each white worker to work with more Hispanics. In general, this implies that both the isolation and exposure indexes ( $H_H$  and  $W_H$ , respectively), will increase. But the isolation index will increase by more, since establishments with more Hispanics to begin with will have larger increases in the number of Hispanic workers, and hence CW will increase.<sup>15</sup> In our view, this kind of increase in the number of Hispanic workers *should* be characterized as an increase in segregation. Second, these alternative segregation measures are also sensitive to the number of matched workers in an establishment (the same issue outlined above), and because they are measures that are calculated at only the establishment-level – unlike the co-worker segregation measure we use – there is no conceptual parallel to excluding the own worker from the calculation.<sup>16</sup>

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<sup>15</sup> More generally,  $W_H$  will also increase, but not by as much as  $H_H$ , and CW will therefore rise. For perhaps the simplest such case, start with four establishments as follows: one HHH, one HHW, one HWW, and one WWW. In this case  $H_H = 2/3$ ,  $W_H = 1/3$ , and  $CW = 1/3$ . Doubling the number of Hispanics and allocating them proportionally, we get the following four establishments: HHHHHH, HHHHW, WWHH, and WWW: In this case  $H_H$  rises to 29/36 (increasing by 5/36),  $W_H$  rises to 14/36 (increasing by 2/36), and CW rises to 15/36 (increasing by 3/36).

<sup>16</sup> We believe this explains why, in Carrington and Troske (1998a, Table 3), where there are small samples of workers within establishments, the random Gini indexes are often extremely high.

At the same time, because calculated changes in segregation between 1990 and 2000 based on our co-worker segregation index are sensitive to the overall proportions of each group in the workforce, changes over the decade in the proportions of particular demographic groups that are matched to establishments can generate changes in measured segregation. So, for example, the fact that the fraction of workers who are Hispanic grew from 1990 to 2000 should yield a small increase in measured co-worker segregation by ethnicity over the decade (even if Hispanics and whites are distributed across establishments in the same proportion in each year). We could avoid this problem by using scale-invariant segregation measures, but then we would fail to capture changes in segregation due to actual changes in workforce composition. That is, the fact that Hispanics make up a growing fraction of the workforce is an important phenomenon to capture.<sup>17</sup>

We present some “unconditional” nationwide segregation measures, as well as “conditional” measures that first condition on metropolitan area (MSA/PMSA) of residence. In the first, the simulations randomly assign workers to establishments anywhere in the country; not surprisingly, in these simulations the random segregation measures are zero or virtually indistinguishable from zero. For comparability, when we construct these unconditional segregation measures we use only the workers included in the MSA/PMSA sample used for the conditional analysis.<sup>18</sup> The unconditional estimates provide the simplest measures of the extent of integration by skill, race, ethnicity, or sex, in the workplace. However, they reflect the distribution of workers both across cities and across establishments within cities. As such, the unconditional measures may tell us less about forces operating in the labor market to create segregation, whereas the conditional measures – which can be interpreted as taking

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<sup>17</sup> Some measured changes in the sample composition of workers over time may reflect changes in the match rates of various kinds of workers to establishments rather than a change in the underlying population composition. This is obviously a limitation of matched data sets like ours, one that exists to a much smaller extent in administrative data sets that come closer to capturing fully the universe of workers.

<sup>18</sup> The results in this paper are generally robust to measuring segregation at the level of the MSA/CMSA metropolitan area (rather than the MSA/PMSA level), as well as measuring unconditional segregation by including all workers in the United States whether or not they live and work in a metropolitan area. For the within MSA/CMSA analysis, results are very similar to the within MSA/PMSA analysis, with the only difference that the increase in black-white segregation is about one-quarter smaller in the first case. For the national analysis using the full DEEDs vs. the MSA/PMSA sample, the changes in segregation are always in the same direction and qualitatively similar, although the estimated percentage changes are a bit more moderate.



residential segregation by city as given – may tell us more about these forces. Because we use the same samples for the conditional and unconditional analyses, for these analyses the observed segregation measures are identical. Only the simulations differ, but these differences of course imply differences in the effective segregation measures.

For the Monte Carlo simulations that generate measures of random segregation, we first define the unit within which we are considering workers to be randomly allocated. We use U.S. Census Bureau MSA/PMSA designations, because these are defined to some extent based on areas within which substantial commuting to work occurs.<sup>19</sup> We then calculate for each metropolitan area the numbers of workers in each category for which we are doing the simulation – for example, blacks and whites – as well as the number of establishments and the size distribution of establishments (in terms of sampled workers). Within a metropolitan area, we then randomly assign workers to establishments, ensuring that we generate the same size distribution of establishments within a metropolitan area as we have in the sample. We do this simulation 100 times, and compute the random segregation measures as the means over these 100 simulations. Not surprisingly, the random segregation measures are very precise; in all cases the standard deviations were trivially small.

#### **IV. Changes in Segregation**

With the preceding technical material out of the way, the empirical results can be presented quite concisely.

##### *Segregation by Education*

The findings for changes in segregation by education are reported in Table 3. We begin by computing segregation between those with at least some college education and those with at most a high

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<sup>19</sup> See U.S. Census Bureau, <http://www.census.gov/geo/lv4help/cengeoglos.html> (viewed April 18, 2005). This is not to say that residential segregation at a level below that of MSAs and PMSAs may not influence workplace segregation. However, an analysis of this question requires somewhat different methods. For example, in conducting the simulations it is not obvious how one should limit the set of establishments within a metropolitan area in which a worker could be employed.

school education. The observed segregation measure for 1990 indicates that on average low education workers are in workplaces in which 54.2 percent of their co-workers are low education, while high education workers are in workplaces in which only 34.5 percent are low education, for a difference of 19.7. This is also the effective segregation measure for the national sample because random allocation of workers to establishments anywhere in the country leads to a random coworker segregation measure of zero. When we look within MSAs/PMSAs, randomness generates a fairly small amount of segregation, so the effective segregation measure declines only a little, to 17.3.

In the 2000 data, observed segregation is 1.4 percentage points higher (21.1), while random segregation is lower. In combination, then, looking within MSAs/PMSAs, effective segregation by education rises two percentage points, or by 11.3 percent, from 1990 to 2000. In the national data, the increase is smaller, from 19.7 to 21.1 percent, or 7.0 percent.<sup>20</sup> The next two panels of Table 3 report results for two alternative education cutoffs: high school dropouts vs. at least a high school degree; and less than a bachelor's degree vs. at least a bachelor's degree. For the high school dropouts vs. at least a high school degree breakdown, the overall national figures indicate an increase in segregation similar to that seen in the first panel of the table; educational segregation increased by 1.7 percentage points (11.1 percent), and by 1.9 percentage points (13.6 percent) within MSAs/PMSAs. When we instead classify workers by whether or not they have a bachelor's degree, the increases in segregation are somewhat larger, between 2.8 and 3.3 percentage points, or 16 to 16.2 percent.<sup>21</sup>

These figures strike us as modest but measurable increases in segregation by education. The direction of change is consistent with the conjecture of Kremer and Maskin (1996), and it is possible that the decade of the 1980s might have experienced even a greater increase in segregation by education, given the sharper increase in schooling-related earnings differentials in that period, although the workforce adjustments may occur relatively slowly. Nonetheless, we may want to be cautious in

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<sup>20</sup> We remind that reader that when we say “national,” we refer to the MSA/PMSA sample.

<sup>21</sup> In Hellerstein and Neumark (forthcoming), we report bootstrapped standard errors for differences in estimates of effective segregation. Differences considerably smaller than the types of increases we find in this paper were strongly significant.

inferring that the increase in segregation by education is attributable to increased returns to skill. One of the mechanisms for this increase in segregation by education is the decline over the decade in the fraction of workers in the sample with low levels of education – for example, the fraction with at most a high school degree drops from 42.9 percent in 1990 to 35.8 percent in 2000. It is also possible, then, that segregation by skill (rather than measured education) is actually unchanged but more workers with high unobserved skills have higher education in the 2000 data.

### *Segregation by Race*

Evidence on changes in segregation by race is reported in Table 4. In 1990, the observed segregation measures indicate that blacks on average worked with workforces that were 23.7 percent black, whereas the comparable figure for whites was only 5.8 percent, for an observed segregation measure of 17.8. This rose between 1990 and 2000 to 21.8, driven mainly by an increase in the average share black in workplaces where blacks were employed. Nationally, black-white segregation rose four percentage points, from 17.8 to 21.8, or an increase of 22.3 percent. Within MSAs/PMSAs, the increase is slightly smaller, at 2.8 percentage points, or 20.3 percent. We interpret these magnitudes as indicating a relatively large increase in workplace segregation by race from 1990 to 2000.

### *Hispanic-White Segregation*

Next, Table 5 reports results for Hispanic-white segregation.<sup>22</sup> Observed Hispanic-white segregation is pronounced. In 1990, Hispanic workers on average worked in establishments with workforces that were 39.4 percent Hispanic, compared with a 4.5 percent figure for whites. Both of these numbers increased slightly as of 2000, to 40.7 percent and 6 percent, respectively, so that the observed segregation measure remained roughly constant – 34.9 percent in 1990 and 34.7 percent in 2000.

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<sup>22</sup> Using the 1990 data only, Hellerstein and Neumark (forthcoming) go into considerable detail regarding Hispanic-white segregation, finding that differences in English language skills account for about one-third of this segregation.

Because of relatively sharp differences in the Hispanic composition of urban areas across the United States, randomness generates a considerable amount of Hispanic-white segregation. This is indicated in the table, where random segregation equals 18.8 in 1990 and 18.0 in 2000. However, again the changes are small, so that the change in effective Hispanic-white segregation appears to be relatively minor. Segregation declines in the national data by 0.2 percentage points, or by less than one percent. And within urban areas, segregation increases slightly, from 19.8 to 20.4, or by only three percent. Overall, then, both the small magnitudes and the differences in results across and within urban areas lead us to conclude that little changed with respect to Hispanic-white workplace segregation between 1990 and 2000.

### *Sex Segregation*

Finally, we turn to segregation by sex. A priori, we might expect to find substantial declines in this form of segregation, because of the declining differences in the types of jobs done by men and women (Wells, 1998). As Table 6 reports, in 1990 women on average worked in establishments with workforces that were 59.9 percent female, as compared with establishments in which men worked, which were 36.2 percent female. Thus, observed segregation was 23.6. As of 2000, the increase in the share female with which men work increased relatively sharply, from 36.2 to 40.2, and as a result observed segregation fell to 20.4. Random segregation by sex is relatively trivial, because neither men nor women constitute a very small share of the workforce. As a result, the change in effective segregation is close to the change in observed segregation. In particular, effective segregation by sex declined from 23.6 to 20.4, or 13.7 percent, on a national basis. And virtually the same decline, 3.2 percentage points or 13.6 percent, is estimated within urban areas because, of course, the distributions of men and women across cities are similar. We view the magnitude of these changes in sex segregation as suggesting a substantive decline over the decade.

One possible explanation for the overall decline in sex segregation is convergence in the occupational distributions of men and women, rather than a reduction in segregation across workplaces

even for men and women in the same occupation. To address this possibility, following the methods in Hellerstein and Neumark (forthcoming), we construct “conditional” random segregation measures, where we simulate segregation holding the distribution of workers by occupation fixed across workplaces. So, for example, if an establishment in our sample is observed to have three workers in occupation A, then three workers in occupation A will be randomly allocated to that establishment. As before, we compute the average (across the simulations) simulated fraction of co-workers who are female for females, denoting this  $F_F^C$ , and the average (across the simulations) simulated fraction of co-workers who are female for males, denoting this  $M_F^C$ . The difference between these two is denoted  $CW^C$ , and we define the extent of “effective conditional segregation” to be:

$$[\{CW^0 - CW^C\} / \{100 - CW^R\}] \times 100,$$

where  $CW^R$  is the measure of random segregation obtained when not conditioning on occupation. A conditional effective segregation measure of zero would imply that all of the effective segregation between women and men can be attributed to differences in the occupations employed by various establishments (“occupational segregation”), coupled with differences in the occupational distributions of women and men. Conversely, a conditional effective segregation measure equal to that of the (unconditional) effective segregation measure would imply that none of the effective segregation between women and men can be attributed to occupational segregation across workplaces.

Columns (5) and (6) of Table 6 report the results of doing this calculation based on a consistent occupation classification across 1990 and 2000, as developed in Meyer and Osborne (2005), which is approximately at the 3-digit level.<sup>23</sup> We do this only for the within MSA/PMSA sample, because central to this analysis is the ability to randomly distribute workers to different establishments, and it only makes sense to do this within the urban areas in which workers commute. The estimates for 1990, in column (5), indicate that a substantial fraction (nearly 50 percent) of the effective segregation of women from men is attributable to differences in the occupational distribution; conditional on occupation, effective

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<sup>23</sup> There are non-trivial differences in occupation codes at the 3-digit level between 1990 and 2000. The structure of occupation codes at the 1-digit level changed even more dramatically between 1990 and 2000, so we do not attempt a concordance at this higher level of aggregation.

segregation by sex falls from 23.3 (column (2)) to 10.4. In the 2000 data, reported in column (6), the effect of occupation is a little bit more pronounced, accounting for 61.2 percent of effective segregation. Finally, conditional on occupation, sex segregation within MSAs/PMSAs declines over time by 2.6 percentage points (from 10.4 to 7.8); in absolute terms, this is similar to the decline in unconditional segregation, but because effective segregation conditional on occupation (in 1990) was only about 45 percent as large as the unconditional effective segregation measure, the decline in conditional segregation between columns (5) and (6) represents a much larger percentage decline – 24.8 percent. Altogether, these results suggest that the decline in sex segregation over the decade is not being driven by the increased propensity of women to work in the same occupations as men.

## **V. The Impact of Changing Establishment and Industry Composition**

Changes in segregation can arise due to a multitude of factors, some of them compositional, such as the changing occupational distribution of women as discussed in the previous section. In this section, we explore the robustness of our full-sample results to two other types of potentially important compositional changes. First, we explore whether the changes in segregation are due to the changing composition of establishments by recalculating our segregation indexes for only the sample of establishments that exist in both the 1990 and 2000 Restricted DEED samples (corresponding to columns (3) and (6) of Table 1).<sup>24</sup> Ideally, we would like to isolate the separate roles of establishment entry and exit – i.e., births of new establishments and deaths of existing ones. However, given that we only match some establishments, we cannot necessarily distinguish births and deaths from matches and non-matches. But assuming that matching is random with respect to segregation, focusing on the set of establishments that are in both samples is informative about the combined roles of establishment entry and exit.

Second, we explore the robustness of our changes in segregation to changes in the industry mix of employment over the decade by reweighting the segregation indexes for 2000 to reflect the industrial

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<sup>24</sup> By restricting the sample to establishments that exist in the Restricted DEED samples in both years we drop some very small MSAs from some of the samples we used to calculate segregation indexes in earlier tables, in cases where there are no matched workers for whom to calculate indexes across the two years.

composition of employment at the 1-digit level that exists in our 1990 data. This is a little more complicated. First, because we are interested in calculating within-MSA indexes, it is actually the within-MSA industry composition that we need to hold fixed at 1990 levels. As a result, we include in the sample only MSAs that exist in both years. Second, we exclude mining, because mining makes up such a trivial proportion of employment that there are some MSAs that have matched workers in mining in 1990 but not in 2000.

To understand how we construct changes in segregation over the decade while holding the distribution of employment across industries within MSAs fixed at 1990 levels, consider again the example of ethnic segregation we discussed in Section III above. Obviously, we compute  $H_H$  (the isolation index) and  $W_H$  (the exposure index) for 1990 in the same way we did previously, since no adjustment needs to be made when accounting for the 1990 industry composition. In order to compute  $H_H$  for 2000 with industry composition fixed as of 1990, we compute the isolation index separately for each industry/MSA pair in 2000.<sup>25</sup> We then take a weighted average across industries of these isolation indexes, where the weight is product of two components: the fraction of total Hispanic employment (in this example) that works in that industry/MSA pair in 2000, and the ratio of the employment share in the industry/MSA pair in 1990 relative to 2000. The fraction of Hispanic employment serves to aggregate up the industry/MSA-specific isolation indexes to the full-sample isolation index (and if used alone to weight up the industry/MSA-specific indexes would yield the 2000 unadjusted isolation index), while the ratio of the employment shares adjusts the data appropriately to reflect the composition of employment in 1990 across industries. For the exposure index,  $W_H$ , we do the same thing, calculating a separate exposure index for each industry/MSA pair and then weighting by the product of the industry employment share ratio times the fraction of white employment in that pair in 2000. Because the fraction Hispanic in an industry MSA/pair may differ from the fraction white in that same industry/MSA pair, the reweighting may have differential effects on the exposure and isolation indexes. As a consequence, adjusting for industry employment changes over the decade will have the largest impact on measured changes in

segregation when there has been differential employment growth in industries with a large share Hispanic coupled with a large difference between the share of Hispanic and the share of white employment in the industry (or if there is a large difference between the isolation and exposure indexes).<sup>26</sup>

The results of these alternative computations are presented in condensed form in Table 7, where we report only the within-MSA effective segregation measures in each year and the changes over the decade. In the first panel of Table 7, we report results for co-worker segregation by high school degree status. In column (1), we first report the within-MSA effective segregation measure in 1990 of 17.3 (from Table 3). Below that number, we report the corresponding figure for the sample of establishments that existed both in 1990 and 2000, finding that co-worker segregation by high school degree status in 1990 is somewhat lower, at 15.7. The fixed-industry-composition co-worker segregation measure for 1990 is 17.3, identical to that for the full sample.<sup>27</sup> In column (2) we report the co-worker segregation measures for 2000. For the fixed-establishment sample, co-worker segregation by high school degree status is 17.0, 2.2 percentage points lower than for the full sample, and for the results holding industry composition fixed, the co-worker measure is slightly higher, at 20.3. Overall, the change over the decade of 2 percentage points for the full sample is close to the 1.4 percentage point increase for the fixed establishment sample, and the increase holding industry composition fixed is a bit larger, at 3.1 percentage points. In general, though, the observed increase in co-worker segregation for the full sample over the decade is robust to the changing mix of establishments and industries.

In the second and third panels of Table 7 we report the results for the alternative education cut-offs. The results again reflect some small differences across the sample of establishments and mix of industries, and the overall qualitative results again point to increases in segregation by education over the decade.

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<sup>25</sup> For the random segregation indexes, the industry used is the random industry to which the worker is assigned.

<sup>26</sup> This turns out to be quite significant in our calculations for changes in sex segregation holding the industry composition of employment fixed, where the services industry grew rapidly, and is also heavily female.

<sup>27</sup> Because we exclude workers in mining and workers in MSAs that were not defined as such in 1990 and 2000, the results for 1990 can be slightly different than we report in the full sample in Table 3.



Racial segregation increased over the decade for the full sample by 2.8 percentage points (20.3 percent), but increased by only about half that much for the sample of establishments that exist in both years. This means that new establishments in 2000 are characterized by more racial segregation than establishments that existed in 1990. Moreover, holding the industry composition of employment fixed at 1990 levels, racial segregation increased by a much smaller amount over the decade – 0.6 percentage points (4.6 percent). The fact that newer establishments and the industries that are gaining in employment over the decade are also more segregated by race in 2000 than older establishments and declining industries could portend continuing increases in racial segregation.

Interestingly, the results are somewhat different for Hispanic-white segregation, as we report in the fifth panel of Table 7. In the overall sample co-worker segregation increased relatively little over the decade, by only 0.6 percentage points (three percent). For the sample of establishments that exist in both years, co-worker segregation actually fell a little, from 16.5 to 15.6, whereas for the employment-constant industry results, the co-worker segregation measure rose over the decade from 19.1 to 22.0. While none of these results point to major differences, it appears that the changing industry mix served to decrease Hispanic-white segregation, while the entry and exit of establishments seems to have worked in the opposite direction.

As reported in the last panel of Table 7, the results are most notably different for sex segregation – in particular, with respect to the role of industry composition. First, for the full sample, co-worker segregation fell by 3.2 percentage points over the decade, whereas for the sample of continuing establishments it fell by 2.3 percentage points. Since the baseline co-worker segregation measure in 1990 for the continuing establishments sample is slightly higher (25.2) than for the full sample (23.3), on a percentage basis segregation actually declined somewhat more for the full sample, but the difference is small. However, a much sharper difference arises when comparing the change over the decade to that obtained holding the distribution of employment across industries fixed at 1990 levels. In particular, co-worker segregation in 2000 is 20.1 in the full sample, but only 14.4 in the fixed-industry-composition

results. As a result, co-worker segregation for the fixed-industry-composition calculation falls over the decade by a full nine percentage points, or 38.3 percent.

Industry composition has such a strong influence on changes in measured sex segregation because there was very sharp employment growth in services, which is a highly sex segregated industry with a high share of female employment. Thus, absent the growth in services (which is what we mimic by holding the industry composition of employment fixed), sex segregation would have declined by considerably more. To see this, Table 8 presents detailed information on isolation and exposure indexes and observed segregation by industry, as well as the distribution of employment of men and women across industries, and industry employment growth over the decade. The table shows, first, that with the exception of transportation, which is a relatively small industry, services is the most segregated industry in both years. The percentage point decline in observed segregation is relatively similar across industries, with the exception of wholesale. In addition, the services industry was the largest employer of women in both years, accounting for 35 percent of female employment in 1990 and 44 percent of female employment in 2000. So, for example, services alone accounts for half of the isolation index in 2000.<sup>28</sup> Services was also the second largest employer of men in both years, but well behind manufacturing. Employment of both men and women in services grew sharply over the decade. This is reflected in the distribution of men and women across industries by year (columns (4) and (5)), as well as in the ratio of overall employment in 1990 relative to 2000, as reported in column (6); services has the lowest ratio (0.76), corresponding to the sharpest growth.

As a result of the fact that services is a relatively highly sex segregated industry, coupled with the fact that it is a heavily female industry that grew tremendously over the decade, the services industry plays a large role in overall changes in sex segregation. To reinforce the importance of the growth in services employment in mitigating the decline in sex segregation, Table 9 shows our calculations of effective sex segregation and how it changed over the decade, with and without holding the industry

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<sup>28</sup> This can be seen by multiplying the isolation index for services of 72.7 percent by the employment share of services in female employment of 44 percent, as reported in Table 8, accounting for 53 percent of the overall isolation index in 2000 of 60.7 corresponding to the sample used in that table.

composition of employment fixed, and with and without including services. The top panel shows calculations for all industries, echoing the earlier results.<sup>29</sup> In the bottom panel, however, services is simply dropped from the calculation, and the difference in the change in segregation from holding industry composition fixed is only about half as large.<sup>30</sup>

## **VI. Conclusions**

We present evidence on changes in workplace segregation by education, race, ethnicity, and sex. For this analysis, we use the newly-constructed 2000 Decennial Employer-Employee Dataset (DEED). The 2000 DEED, like the 1990 DEED, provides new opportunities to study workplace segregation at the establishment level. More significantly, by pairing the two we are able to present what we believe are the first estimates of changes in workplace segregation based on 2000 Census data. These estimates provide evidence that is complementary to that on changes in residential segregation in the decade between the Censuses. Moreover, we believe that evidence on workplace segregation and how it has changed is likely to be more informative about social interactions between groups (with reference to race, ethnicity, and sex), and directly informative about hypotheses regarding changes in workplace segregation by skill.

The evidence indicates that racial and ethnic segregation at the workplace level remains quite pervasive. For example, if we compare black and white workers, the difference in the share black among the workforce at the establishments where they work is around 22 percentage points. If we compare Hispanics and whites, the difference is about 50 percent larger. At the same time, there is fairly substantial segregation by skill, as measured by education. In other work (Hellerstein and Neumark (forthcoming), using only the 1990 DEED) we explore the extent to which racial and ethnic segregation are attributable to skill differences between blacks and whites or Hispanics and whites; in the latter case

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<sup>29</sup> As noted earlier, the estimates allowing the industry composition to change are slightly different from in Table 6 because of changes in the sample to do the calculation holding industry composition fixed.

<sup>30</sup> Note that there was also strong growth in retail, another industry that is relatively sex segregated.

we focus on language skills. Only for the latter is there evidence that skill differences play a substantial role, explaining about one-third of Hispanic-white segregation.

More significantly, putting together the 1990 and 2000 data, we find *no* evidence of declines in workplace segregation by race and ethnicity. Hispanic-white segregation was largely unchanged, while black-white segregation increased by about three to four percentage points, or about 20 percent. This increase in racial segregation was reinforced by the entry and exit of establishments and by the changing industry composition of employment, suggesting that there may be forces at work that will lead to the persistence of or even increases in racial segregation. Over this decade, segregation by education also increased, by about two to three percentage points, or 11 to 16 percent. This increase is consistent with conjectures that rising returns to skill might generate more segregation by skill, although it could also be attributable to rising education levels among workers with more unobserved skills but with an unchanging pattern of segregation based on these skills.<sup>31</sup>

To the extent that declines in segregation are positive developments, the one bright spot is the decline in workplace segregation by sex, which fell about three percentage points, or 14 percent, from 1990 to 2000. Changes in the occupational distribution of men and women did not play a major role in this decline in segregation. If we hold the distribution of men and women across 3-digit occupations fixed, the absolute decline in segregation is roughly the same, although it is larger in percentage terms. On the other hand, shifts in the industry composition of employment worked *against* the decline in sex segregation, as the fastest-growing industry was services, which is also one of the most sex-segregated and most heavily female industries. What this implies for future changes in sex segregation depends on whether the shifts in industrial composition continue as in the recent past or change course, on changes in the distribution of women across industries, and on changes in sex segregation across establishments within industries.

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<sup>31</sup> Coupled with the earlier findings suggesting that black-white segregation is largely unrelated to education differences, this likely has little if anything to do with the increase in workplace segregation by race.

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Table 1: Means for Workers

	1990 SEDF	1990 Full DEED	1990 Restricted DEED	2000 SEDF	2000 Full DEED	2000 Restricted DEED
	(1)	(2)	(3)	(4)	(5)	(6)
Age	37.08 (12.78)	37.51 (12.23)	37.53 (12.13)	39.15 (13.03)	39.57 (12.51)	39.53 (12.33)
Female	0.46	0.47	0.47	0.46	0.50	0.51
Married	0.60	0.65	0.63	0.58	0.62	0.60
White	0.82	0.86	0.84	0.78	0.83	0.79
Hispanic	0.07	0.05	0.06	0.09	0.07	0.08
Black	0.08	0.05	0.06	0.09	0.06	0.08
Fulltime	0.77	0.83	0.84	0.78	0.82	0.83
Number of Kids (if female)	0.75 (1.04)	0.73 (1.01)	0.69 (0.99)	0.78 (1.07)	0.76 (1.04)	0.74 (1.03)
High School Diploma	0.34	0.33	0.30	0.31	0.29	0.25
Some College	0.30	0.32	0.33	0.33	0.35	0.35
BA	0.13	0.16	0.18	0.15	0.18	0.20
Advanced Degree	0.05	0.05	0.06	0.06	0.08	0.09
Ln(Hourly Wage)	2.21 (0.70)	2.30 (0.65)	2.37 (0.65)	2.55 (0.73)	2.63 (0.70)	2.70 (0.70)
Hourly Wage	12.10 (82.19)	12.89 (37.07)	13.68 (27.41)	17.91 (137.20)	18.83 (63.61)	20.19 (64.05)
Hours Worked in previous year	39.51 (11.44)	40.42 (10.37)	40.55 (10.10)	40.22 (11.74)	40.72 (11.09)	40.90 (10.85)
Weeks Worked in previous year	46.67 (11.05)	48.21 (9.34)	48.46 (9.05)	47.23 (10.58)	48.38 (9.27)	48.56 (9.05)
Earnings in previous year	22,575 (26,760)	25,581 (29,475)	27,478 (30,887)	33,521 (42,977)	37,244 (47,237)	40,272 (50,406)
Industry:						
Mining	0.01	0.01	0.01	0.01	0.00	0.00
Construction	0.07	0.04	0.03	0.08	0.05	0.04
Manufacturing	0.25	0.34	0.35	0.21	0.26	0.26
Transportation	0.08	0.05	0.05	0.07	0.05	0.05
Wholesale	0.05	0.07	0.08	0.05	0.05	0.05
Retail	0.20	0.17	0.15	0.21	0.21	0.20
FIRE	0.08	0.08	0.09	0.07	0.07	0.07
Services	0.26	0.24	0.24	0.31	0.31	0.32
N	12,143,183	3,291,213	1,828,020	14,057,121	4,089,098	2,209,908



Table 2: Means for Establishments

	1990 BR	1990 Full DEED	1990 Restricted DEED	2000 BR	2000 Full DEED	2000 Restricted DEED
	(1)	(2)	(3)	(4)	(5)	(6)
Total Employment	17.57 (253.75)	52.68 (577.39)	104.67 (996.52)	18.77 (138.11)	48.74 (232.05)	95.54 (371.18)
Establishment Size:						
1-25	0.88	0.65	0.39	0.87	0.66	0.41
26-50	0.06	0.15	0.22	0.06	0.15	0.21
51-100	0.03	0.10	0.19	0.03	0.09	0.17
101+	0.03	0.10	0.21	0.03	0.09	0.20
Industry:						
Mining	0.00	0.01	0.01	0.00	0.00	0.00
Construction	0.09	0.07	0.06	0.11	0.08	0.07
Manufacturing	0.06	0.13	0.23	0.06	0.13	0.18
Transportation	0.04	0.05	0.05	0.04	0.05	0.05
Wholesale	0.08	0.11	0.10	0.07	0.07	0.07
Retail	0.25	0.24	0.23	0.25	0.29	0.27
FIRE	0.09	0.10	0.11	0.09	0.08	0.07
Services	0.28	0.26	0.21	0.35	0.30	0.27
In MSA	0.81	0.82	1	0.81	0.79	1
Census Region:						
North East	0.06	0.06	0.05	0.06	0.05	0.04
Mid Atlantic	0.16	0.15	0.16	0.15	0.14	0.14
East North Central	0.16	0.20	0.21	0.16	0.20	0.21
West North Central	0.07	0.08	0.07	0.08	0.09	0.08
South Atlantic	0.18	0.16	0.15	0.18	0.16	0.16
East South Central	0.05	0.05	0.04	0.06	0.05	0.04
West South Central	0.10	0.10	0.09	0.10	0.10	0.10
Mountain	0.06	0.05	0.05	0.07	0.06	0.06
Pacific	0.16	0.15	0.17	0.16	0.15	0.17
Payroll (\$1000)	397 (5,064)	1,358 (10,329)	2,910 (16,601)	694.44 (69,383)	1,993 (115,076)	4,421 (198,414)
Payroll/Total Employment	21.02 (1,385.12)	24.24 (111.79)	26.70 (181.48)	33.74 (70,72.29)	35.91 (1,834.40)	42.27 (1,877.29)
Share of Employees Matched	--	0.17	0.16	--	0.16	0.14
Multi-Unit Establishment	0.23	0.42	0.53	0.26	0.40	0.50
N	5,237,592	972,436	317,112	5,651,680	1,279,999	411,300

Table 3: Segregation by Education

	1990 U.S. MSA/PMSA Sample	1990 Within MSA/PMSA Sample	2000 U.S. MSA/PMSA Sample	2000 Within MSA/PMSA Sample
	% Low Ed	% Low Ed	% Low Ed	% Low Ed
	(1)	(2)	(3)	(4)
Co-Worker Segregation				
<b>High school degree or less vs. more than high school</b>				
Observed Segregation				
Low Education Workers	54.2	54.2	49.3	49.3
High Education Workers	34.5	34.5	28.2	28.2
Difference	19.7	19.7	21.1	21.1
Random Segregation				
Low Education Workers	42.9	44.6	35.8	37.3
High Education Workers	42.9	41.7	35.8	35.0
Difference	0	2.9	0	2.3
Effective Segregation	19.7	17.3	21.1	19.2
<b>Percentage Point (Percent) Change, 1990-2000</b>			<b>1.4 (7.0)</b>	<b>2.0 (11.3)</b>
<b>Less than high school vs. high school degree or more</b>				
Observed Segregation				
Low Education Workers	26.0	26.0	25.5	25.5
High Education Workers	10.8	10.8	8.6	8.6
Difference	15.2	15.2	16.9	16.9
Random Segregation				
Low Education Workers	12.7	13.8	10.4	11.3
High Education Workers	12.7	12.6	10.4	10.3
Difference	0	1.3	0	1.0
Effective Segregation	15.2	14.1	16.9	16.0
<b>Percentage Point (Percent) Change, 1990-2000</b>			<b>1.7 (11.1)</b>	<b>1.9 (13.6)</b>
<b>Less than bachelor's degree vs. bachelors' degree or more</b>				
Observed Segregation				
Low Education Workers	80.7	80.7	77.7	77.7
High Education Workers	60.6	60.6	54.3	54.3
Difference	20.2	20.2	23.4	23.4
Random Segregation				
Low Education Workers	75.9	76.6	70.8	71.9
High Education Workers	75.9	73.5	70.8	68.2
Difference	0	3.1	0	3.8
Effective Segregation	20.2	17.6	23.4	20.4
<b>Percentage Point (Percent) Change, 1990-2000</b>			<b>3.3 (16.2)</b>	<b>2.8 (16.0)</b>
Number of Workers	1,828,020	1,828,020	2,209,908	2,209,908
Number of Establishments	317,112	317,112	411,300	411,300

Table 4: Black-White Segregation

	1990 U.S. MSA/PMSA Sample	1990 Within MSA/PMSA Sample	2000 U.S. MSA/PMSA Sample	2000 Within MSA/PMSA Sample
	% Black	% Black	% Black	% Black
	(1)	(2)	(3)	(4)
Co-Worker Segregation				
Observed Segregation				
Black Workers	23.7	23.7	28.7	28.7
White Workers	5.8	5.8	6.9	6.9
Difference	17.8	17.8	21.8	21.8
Random Segregation				
Black Workers	7.1	11.2	8.8	14.2
White Workers	7.1	6.8	8.8	8.3
Difference	0	4.4	0	5.9
Effective Segregation	17.8	14.0	21.8	16.8
<b>Percentage Point (Percent) Change, 1990-2000</b>			<b>4.0 (22.3)</b>	<b>2.8 (20.3)</b>
Number of Workers	1,618,876	1,618,876	1,893,034	1,893,034
Number of Establishments	285,988	285,988	360,072	360,072

Table 5: Hispanic-White Segregation

	1990 U.S. MSA/PMSA Sample	1990 Within MSA/PMSA Sample	2000 U.S. MSA/PMSA Sample	2000 Within MSA/PMSA Sample
	% Hispanic	% Hispanic	% Hispanic	% Hispanic
	(1)	(2)	(3)	(4)
Co-Worker Segregation				
Observed Segregation				
Hispanic Workers	39.4	39.4	40.7	40.7
White Workers	4.5	4.5	6	6
Difference	34.9	34.9	34.7	34.7
Random Segregation				
Hispanic Workers	6.9	24.4	9.2	25.5
White Workers	6.9	5.6	9.2	7.5
Difference	0	18.8	0	18.0
Effective Segregation	34.9	19.8	34.7	20.4
<b>Percentage Point (Percent) Change, 1990-2000</b>			<b>-0.2 (-0.4)</b>	<b>0.6 (3.0)</b>
Number of Workers	1,625,953	1,625,953	1,906,878	1,906,878
Number of Establishments	293,989	293,989	373,006	373,006

Table 6: Segregation by Sex

	Unconditional				Conditional on 3-Digit Occupation	
	1990 U.S. MSA/PMSA Sample	1990 Within MSA/PMSA Sample	2000 U.S. MSA/PMSA Sample	2000 Within MSA/PMSA Sample	1990 Within MSA/PMSA Sample	2000 Within MSA/PMSA Sample
	% Female	% Female	% Female	% Female	% Female	% Female
	(1)	(2)	(3)	(4)	(5)	(6)
Co-Worker Segregation						
Observed Segregation						
Female Workers	59.9	59.9	60.6	60.6	59.9	60.6
Male Workers	36.2	36.2	40.2	40.2	36.2	40.2
Difference	23.6	23.6	20.4	20.4	23.6	20.4
Random Segregation						
Female Workers	47.4	47.7	50.5	50.7	54.4	56.8
Male Workers	47.4	47.2	50.5	50.3	41.1	44.1
Difference	0	0.5	0	0.4	13.3	12.6
Effective Segregation	23.6	23.3	20.4	20.1	10.4	7.8
<b>Percentage Point (Percent) Change, 1990-2000</b>			<b>-3.2 (-13.7)</b>	<b>-3.2 (-13.6)</b>		<b>-2.6 (-24.8)</b>
Fraction of sex segregation accounted for by occupation					55.4	61.2
Number of Workers	1,828,020	1,828,020	2,209,908	2,209,908	1,828,020	2,209,908
Number of Establishments	317,112	317,112	411,300	411,300	317,112	411,300

Table 7: Alternative Co-Worker Segregation Calculations

	1990 Within MSA/PMSA Sample, Effective Segregation	2000 Within MSA/PMSA Sample, Effective Segregation	Percentage Point (Percent) Change, 1990-2000
	(1)	(2)	(3)
<b><i>Segregation by Education</i></b>			
<b>High school degree or less vs. more than high school</b>			
Full sample, Table 3	17.3	19.2	<b>2.0 (11.3)</b>
Establishments present in 1990 and 2000	15.7	17.0	<b>1.4 (8.9)</b>
Fixed industry composition	17.3	20.3	<b>3.1 (17.8)</b>
<b>Less than high school vs. high school degree or more</b>			
Full sample, Table 3	14.1	16.0	<b>1.9 (13.6)</b>
Establishments present in 1990 and 2000	11.4	12.7	<b>1.2 (10.7)</b>
Fixed industry composition	13.8	15.8	<b>2.0 (14.3)</b>
<b>Less than bachelor's degree vs. bachelor's degree or more</b>			
Full sample, Table 3	17.6	20.4	<b>2.8 (16.0)</b>
Establishments present in 1990 and 2000	15.4	17.4	<b>2.0 (12.8)</b>
Fixed industry composition	17.6	21.8	<b>4.2 (24.0)</b>
<b><i>Black-White Segregation</i></b>			
Full sample, Table 4	14.0	16.8	<b>2.8 (20.3)</b>
Establishments present in 1990 and 2000	11.2	12.6	<b>1.4 (12.7)</b>
Fixed industry composition	14.1	14.7	<b>0.6 (4.6)</b>
<b><i>Hispanic-White Segregation</i></b>			
Full sample, Table 5	19.8	20.4	<b>0.6 (3.0)</b>
Establishments present in 1990 and 2000	16.5	15.6	<b>-0.9 (-5.6)</b>
Fixed industry composition	19.1	22.0	<b>2.9 (15.3)</b>
<b><i>Segregation by Sex</i></b>			
<b>Unconditional</b>			
Full sample, Table 6	23.3	20.1	<b>-3.2 (-13.6)</b>
Establishments present in 1990 and 2000	25.2	23.0	<b>-2.3 (-8.9)</b>
Fixed industry composition	23.4	14.4	<b>-9.0 (-38.3)</b>

Mining is excluded for "Full sample, fixed industry composition."

Table 8: Observed Sex Segregation, by Industry, Within MSA/PMSA

	1990 Observed Segregation	2000 Observed Segregation	Percentage Point (Percent) Change, 1990-2000	1990 Share of Female or Male Employment in Industry	2000 Share of Female or Male Employment in Industry	Ratio of 1990 to 2000 Industry Employment
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Construction</b>						
Female	23.7	21.6		0.02	0.02	0.92
Male	21.1	23.9		0.05	0.06	
Difference	2.5	-2.3	-4.9 (-191.4)			
<b>Manufacturing</b>						
Female	43.7	41.2		0.24	0.17	1.29
Male	28.2	29.8		0.44	0.35	
Difference	15.5	11.4	-4.1 (-26.3)			
<b>Transportation</b>						
Female	49.0	48.8		0.04	0.04	1.05
Male	29.4	32.2		0.07	0.07	
Difference	19.6	16.7	-3.0 (-15.1)			
<b>Wholesale</b>						
Female	40.2	38.6		0.06	0.03	1.50
Male	31.9	31.6		0.10	0.07	
Difference	8.3	6.9	-1.4 (-16.3)			
<b>Retail</b>						
Female	59.3	57.1		0.17	0.20	0.80
Male	42.9	44.7		0.14	0.20	
Difference	16.4	12.4	-4.0 (-24.3)			
<b>FIRE</b>						
Female	69.3	69.1		0.12	0.09	1.24
Male	61.3	63.7		0.05	0.05	
Difference	8.0	5.4	-2.6 (-32.7)			
<b>Services</b>						
Female	74.2	72.7		0.35	0.44	0.76
Male	55.1	58.4		0.15	0.21	
Difference	19.2	14.3	-4.8 (-25.1)			

Table 9: Effective Sex Segregation, with Fixed Industry Composition of Employment, Including and Excluding Services

	<b>1990 Observed Segregation</b>	<b>2000 Observed Segregation</b>	<b>2000 Observed, Fixed Industry Composition</b>
<b><i>Including Services</i></b>			
Observed Segregation			
Female	59.9	60.7	55.1
Male	36.2	40.2	40.3
Difference	23.7	20.5	14.7
Random Segregation			
Female	47.7	50.7	50.7
Male	47.2	50.4	50.4
Difference	0.5	0.4	0.4
Effective Segregation	23.4	20.2	14.4
<b>Percentage Point (Percent) Change, 1990-2000</b>		<b>-3.2 (-13.5)</b>	<b>-9.0 (-38.3)</b>
Number of Workers	1,739,063	2,151,566	2,151,566
Number of Establishments	301,029	398,958	398,958
<b><i>Excluding Services</i></b>			
Observed Segregation			
Female	52.1	51.2	48.9
Male	32.9	35.4	35.1
Difference	19.2	15.9	13.8
Random Segregation			
Female	41.0	42.2	42.2
Male	40.5	41.9	41.9
Difference	0.5	0.3	0.3
Effective Segregation	18.8	15.7	13.6
<b>Percentage Point (Percent) Change, 1990-2000</b>		<b>-3.1 (-16.5)</b>	<b>-5.2 (-27.7)</b>
Number of Workers	1,310,125	1,450,311	1,450,311
Number of Establishments	236,412	289,206	289,206

Mining is excluded.