Employment Dynamics and Business Relocation: New Evidence from the National Establishment Time Series

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Abstract

We analyze and assess new evidence on employment dynamics from a new data source – the National Establishment Time Series (NETS). The NETS offers advantages over existing data sources for studying employment dynamics, including tracking business establishment relocations that can contribute to job creation or destruction on a regional level. We assess the reliability of the NETS data along a number of dimensions, and conclude that it is a reliable data source although not without limitations.

We then use the NETS data for the entire state of California to fully decompose employment change into its six constituent processes, including job creation and destruction stemming from business relocation. This illustrates one of the potentially important uses of the NETS data, and also contributes hard evidence to a policy debate over business relocation that has been entirely speculative and reliant upon anecdotal evidence. The analysis indicates that: (i) California has been losing business establishments and jobs due to interstate relocation, but the impact on the state economy is negligible; (ii) business establishment relocations within the state are far more common than relocations across state boundaries; (iii) employment growth is primarily driven by the births and deaths of business establishments, and by expansion and contraction of existing establishments; and (iv) business relocation across state boundaries contributes very little to overall job reallocation.

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1. Introduction

Employment growth is a major goal of economic policy at both the national and regional levels. Employment change is a dynamic process resulting from job creation and job destruction, which in turn are predominantly driven by business activities including the birth, death, growth, contraction, and relocation of business establishments. This dynamic process is represented in Figure 1, in which each arrow stands for a type of establishment-level change that causes total employment to grow or decline. When an establishment is created ("born") in the economy, or moves into it, or an existing establishment expands, then total employment will grow. Conversely, when a business establishment goes out of existence ("dies") or moves out of the economy, or an existing establishment cuts jobs, then total employment will decrease.[1]

This "demographic" depiction of business establishments and employment changes implies that employment changes in the economy can be decomposed as follows:

Employment growth

= *(job growth at expanding establishments – job decline at contracting establishments)*

+ *(jobs at new establishments – jobs at establishments that closed)*

+ *(job at establishments that moved in – jobs at establishments that moved out*).

Figure 1 and this decomposition remind us that employment change is a complex process, as it is ultimately the net effect of six influences – three that create jobs and three that destroy jobs. This complexity is important for both describing the economy and thinking about the impact of policies (and other factors) on employment growth. Ultimately, we need to understand all six of these dynamic processes to characterize employment change in an economy, both to describe employment change, and to identify the job creation and destruction processes on which it might be most productive for policymakers to focus in encouraging employment growth. Moreover, the fact that employment change is the net result of potentially large gross changes – for example, overall expansion of jobs at existing establishments and overall contraction of jobs at other existing establishments – suggests that what often

[1] Strictly speaking, whenever we refer to "employment" measured at the establishment level, we should refer to "jobs," because workers can have jobs at more than one employer. But since "job change" usually conveys a different meaning than "employment change," we often refer to employment instead.

appear as relatively moderate overall changes in employment over time may mask potentially volatile gross job flows. This implies that relatively small changes in any of the gross flows can lead to sharp changes in net job growth.

Thus, understanding the importance of each of the sources of employment change is critical both for understanding the process of employment growth, and for thinking about and studying policies that might affect any one (or more) of them. But tracking a large population of business establishments across time and space, including births, deaths, and relocations, is difficult and costly, and thus data have not been available with which to fully capture the underlying processes of employment dynamics. Primarily for this reason, although the importance of understanding the job creation-destruction process has long been widely recognized (e.g., Schumpeter, 1942, Chapter 7), systematic empirical research on this topic did not start until quite recently as researchers began to develop appropriate data sources. But this research has continued to face significant limitations imposed by the data.

In this paper we help introduce a new data source – the National Establishment Time Series (NETS) – which we believe is the first data set that permits a full decomposition of the sources of employment change in the U.S. economy, and which offers other advantages relative to existing data sources. We do two things with the NETS data. First, we assess their reliability along numerous dimensions, generally concluding that the NETS is a reliable data source although not without limitations. Second, we illustrate the usefulness of these data by using the NETS data for the entire state of California to fully decompose employment change into its six constituent processes, documenting the importance of each in contributing to employment change and its volatility. Because a principal advantage of the NETS data is the tracking of business establishment relocations, we emphasize the importance of relocation in employment dynamics. This analysis of the role of business relocation contributes hard evidence to a policy debate over business relocation that has been entirely speculative and reliant upon anecdotal evidence.

2. The NETS Database

The NETS database is a new longitudinal file based on recent D&B data. It is a long-term project of Walls & Associates in conjunction with Dun and Bradstreet (D&B). We currently have access to a version of this data set that covers all business establishments that were ever located in California between 1989 and 2002, and their respective parent headquarters (regardless of location).[2]

This version of the NETS database begins with 14 cross-sectional files of the full Data Universal Numbering System (DUNS) Marketing Information (DMI) file for each year from 1990 through 2003, each of which covers the previous year. From here on, we refer to the year covered by the data, i.e., 1989-2002 for the full sample period. The primary purpose of D&B's data collection effort is to provide information on businesses to the business community, in order to enhance their decision-making by constructing a set of "predictive indicators" (e.g., the D&B Rating and PayDex scores). The DMI file for each year is constructed from an ongoing effort to capture each business establishment in the United States in each year (including nonprofits and the public sector). The DMI file is based on a multi-layered process incorporating many data sources.

D&B strives to identify all business establishments, and to assemble information on them, through a massive data collection effort, including over 100 million telephone calls from four calling centers each year, as well as obtaining information from legal and court filings, newspapers and electronic news services, public utilities, all U.S. Secretaries of State, government registries and licensing data, payment and collections information, company filings and news reports, and the U.S. Postal Service.[3] Particular efforts are devoted to identifying the births and deaths of establishments. For every establishment identified, D&B assigns a DUNS number as a means of tracking the establishment. It should be pointed out that since around 1990, the DUNS has increasingly become the standard means of

[2] An observation in the NETS data is an "establishment." An establishment is a business or industrial unit at a single physical location that produces or distributes goods or performs services, for example, a single store or factory. Many companies own or control more than one establishment, and those establishments may be located in different geographic areas and may be engaged in different kinds of business. The NETS database is based on information collected at the establishment level, but also includes a unique Data Universal Numbering System (DUNS) to indicate the relationships among establishments in multi-establishment firms.

[3] See http://mddi.dnb.com/mddi/story.aspx (viewed April 28, 2005).

tracking business, having been adopted by many government agencies in the United States and internationally.[4]

Although the goal of D&B is not to collect and organize data for scholarly research, it does have an incentive to ensure the accuracy of its data, because inaccuracies would hurt D&B's business and might even result in lawsuits. D&B has established a sophisticated quality control system and engages in extensive quality and consistency checks.[5] Thus, the data in each cross-section should provide high quality "snapshots" of business establishments.

Walls & Associates entered into a collaboration with D&B with a very different purpose in mind – namely, to provide a dynamic view of the U.S. economy using the data from the D&B archives (Walls & Associates, 2003). Essentially, this requires linking the D&B cross-sections into a longitudinal file that tracks every establishment from its birth, through any physical moves it may make, capturing any changes of ownership, and recording the establishment's death if it occurs. This is a multi-stage process, the most important steps of which include merging the data files, imputing data when data are not reported,[6] eliminating duplicate records, merging records on establishments for which the DUNS number changes (which happens occasionally) yet which appear to cover the same establishment, and identifying establishment relocations.

A central question for the use to which we put the NETS data in this paper, and more generally for decomposing employment change, is how D&B distinguishes whether an establishment at a new location previously existed elsewhere – and hence will be labeled a relocation in the longitudinal file – or instead is a new establishment. Clearly the correct classification of relocations is critically important in estimating the contributions of business relocation in job creation and destruction.

[4] See, for example, http://www.dnb.co.in/whoduns.htm (viewed May 11, 2005). This widespread adoption of the DUNS number likely stemmed from a court ruling, discussed below, that allowed the regional Bells to sell information they collected for their phonebooks to other parties, which led to a great expansion in the D&B database.
[5] See http://www.dnb.com/us/about/db_database/dnbinfoquality.html (viewed April 28, 2005).
[6] The file indicates when data are imputed, so that empirical analyses can also be done using the subset of observations with non-imputed data.

In thinking about the classification of relocations, a key point is that the DUNS number is the foundation of D&B's data system, as it is the DUNS number that allows D&B to attach information on credit histories, etc., which is what its clients value. Consequently, DUNS numbers are unique, and D&B never recycles numbers. If an establishment closes its DUNS number goes into an "out of business or inactive" file, where it remains permanently unless that business reopens. Each time D&B updates establishment information, in attempts to contact the establishment based on the previous location information on the establishment. Moves can be indicated in a number of ways. Frequently there is a forwarding address or telephone number, or continuing email contact that allows D&B to identify a new location. In many cases, business establishments notify D&B of their move. In addition, if an establishment belonging to a multi-unit firm cannot be found, D&B contacts the headquarters to determine whether a relocation has occurred. In any case in which D&B finds that the establishment previously existed elsewhere, it assigns its existing DUNS number. Finally, if a new establishment is identified whose characteristics do not match those of an existing establishment in the database, D&B contacts the establishment to verify its start date, and assigns a new DUNS number. With these procedures, the longitudinal file should correctly identify relocations of establishments and distinguish them from births of new establishments, although of course one cannot rule out the possibility of occasional errors of a move being classified as a death in one location and a birth in another, which would lead to an undercount of relocating establishments.[7]

The resulting NETS database includes the following variables that are of particular importance to this research: current business name; current establishment location (zip codes including the four-digit extension); FIPS county codes in each year; type of location (single location, headquarters, branch) in each year; employment in each year; SIC codes at the eight-digit level in each year; and, if the establishment has ever moved, the year of movement, origin zip code, origin city, origin state, destination zip code, destination city, and destination state.

[7] Most of the information in this paragraph was supplied by Don Walls and confirmed by him with D&B.

One highly desirable feature of the NETS database is that it covers essentially all establishments. This reflects the fact that it is designed to capture the universe rather than a sample of establishments. Over the sample period of 1989-2002, the database includes information each year on between 1.2 and 1.8 million establishments in California providing about 15 million to 18 million jobs. An establishment is included in our version of the NETS data only if it was ever located in California during 1989-2002, or is the parent headquarters of such an establishment. In total, more than 3.5 million establishments are covered in our extract of the NETS database.

An establishment relocation in the NETS data is identified by street address and zip code changes from one year to another. Both establishments that moved out of California and establishments that moved into California are included in the database, so we are able to track cross-state relocation. However, there are some limits to what this form of relocation can tell us about the dynamics of employment change, as other types of changes in employment might be viewed as sharing features of establishment relocation, or reflecting the same forces that drive relocation. First, if a California company sets up an establishment in another state, that establishment does not show up in our data set. That is, we can study establishments that "move out" but not those that "branch out." Although many discussions in the popular media confuse these two types of activities, the latter one should not be regarded as equivalent to the former, because branching out does not necessarily occur at the cost of creating an additional establishment within the state. Second, the NETS database only tracks physical establishment relocation. There are several other types of relocation that it does not capture by design. For instance, it does not allow us to determine when specific jobs or positions are shifted between two discrete locations of the same firm. This type of relocation, which also constitutes a relocation of jobs between establishments, will be observed in our dataset as employment expansion or contraction. Also, relocations that involve the consolidation of activities at two or more locations into a single location will often be missed, and will be reflected in one establishment growing and another closing. Despite these caveats, the NETS database enables empirical research that represents a significant step towards understanding the role of business relocation in job creation and destruction.

As the preceding discussion indicates, the data construction effort – including both the cross-sectional files and the longitudinal linking that tracks establishments over time – is a massive and complicated effort. For this reason, we have undertaken a good deal of investigation to document and examine the quality of the NETS data in order to assess their reliability, potential limitations, and how these limitations might affect results of various analyses. Before describing our research assessing the NETS, we discuss the alternative data sources that have been used in research on employment dynamics – including earlier D&B data – and some of the potential problems that they pose. This discussion serves to highlight some of the strengths as well as the potential weaknesses of the NETS, and thus paves the way for thinking about how to assess the NETS.

3. Data Sources and Past Research on Employment Dynamics

The NETS is not the first dataset with which researchers can study employment dynamics, nor is this the first project to attempt to study this question using data from D&B. However, other data sources suffer from important limitations in studying employment dynamics, and previous work using earlier D&B files has been criticized rather sharply. In this section, we review past research and alternative data sources.

3.1 Early Dun & Bradstreet data

The original work on job creation and destruction was done by Birch (1979, 1981, and 1987) and Allaman and Birch (1975). This work is of interest because it uses data collected by Dun and Bradstreet (D&B), which is also the source of the NETS database, and because some of the claims Birch makes relate to the questions we consider in using the NETS. Keep in mind, though, that we are using data from a much later period with many more sources of information available because of computer technology and other factors. Thus, conclusions regarding both the findings and the quality of the data used by Birch may be quite different from those based on the NETS database, making it particularly essential to base assessments of data derived from D&B on the current data. But a brief review of earlier work based on the data Birch used is helpful in framing the questions we consider in more detail later in this paper.

The most significant claim emanating from Birch's research was that small firms are the most important source of job creation in the U.S. economy.[8] As an example, Birch (1987) argues that during the period 1981-1985, firms with fewer than 20 employees accounted for 88.1 percent of employment growth at existing firms (Figure 1-5, p. 16), and 82 percent of employment growth via expansion and contraction of existing firms (Figure 1-3, p. 14). Birch's argument about the role of small firms in job creation caught the attention of the popular press. It also led researchers to test the hypothesis in different industries and different countries (see, for example, Kirchhoff and Phillips, 1988; Fumagalli and Mussati, 1993; Baldwin and Picot, 1995; and Broersma and Gautier, 1997). However, it also attracted much criticism.

Most notably, perhaps, Davis, et al. (1996) criticized Birch's calculations, which divide firms into size classes and then measure job growth in each class. They argued that such calculations are subject to a "regression fallacy" that leads to upward bias in the estimate contribution of small firms to job growth.[9] The bias arises because either measurement error that is uncorrelated over time or transitory fluctuations will lead to some firms being classified as small in whatever base year one chooses (because of erroneous or transitory employment declines), and then growing more sharply over the next year because of regression to the mean.[10] Davis, et al., argue that this regression fallacy fully explains the relationship between firm size and job growth. Using data from the Longitudinal Research Database (discussed below), when they use an average instead of a base-year firm size measure the data show "no systematic relationship to average plant size" (1996, p. 68).[11] Davidsson, et al. (1998) question the strength of the conclusions reached by Davis, et al., and based on evidence from Swedish data suggest that the regression

[8] The subtitle of Birch's 1987 book is "How Our Smallest Companies Put the Most People to Work."
[9] In addition, Armington and Odle (1982) present evidence that many of the small businesses generating employment growth (using Birch's measure) were smaller establishments in larger firms, and that small *firms* contributed about as much to employment growth as their overall share in employment.
[10] Leonard (1987) reports some direct evidence consistent with regression to the mean in establishment size (Table 6.7), and suggests that it is attributable to real dynamics, rather than measurement error (p. 154).
[11] They conjecture that this is even more severe in the D&B data that Birch uses, because of greater measurement error. Davis, et al., also extend their criticism to similar calculations included in annual reports to the President of the Small Business Administration (in the mid- to late-1980s).

fallacy accounts for little distortion in the relationship between firm size and net job growth.[12] Despite some remaining disagreements, the suggestion of Davis, et al., to compute job creation and destruction rates from period t to period t+1 relative to the average employment level in these two periods has been widely adopted, because this calculation captures job creation and destruction symmetrically (e.g., Pivetz, et al., 2001, pp. 14-15).

A second conclusion of Birch's work – less central to his book, but more relevant to the empirical work presented later in this paper – is that business relocation is largely irrelevant to employment change. Specifically, Birch claims that "however highly publicized they may be, relocated firms are insignificant from a job creation or loss standpoint. Many firms move each year, but the vast majority do so [over] comparatively short distances, and virtually all … within the same metropolitan area" (1987, p. 136). This conclusion is based on work reported in Allaman (1978) and Allaman and Birch (1975), using D&B data as well as data from other sources. Looking at the four Census regions, Allaman (1978) reports that in-migration of firms contributes only about 1.5 percent of employment growth over a three-year period (1970-1972), while out-migration contributes about 1 percent of employment decline. While in- and out-migration would be expected to play a larger role for more disaggregated regions (such as states), this conclusion foreshadows some of our findings on the importance of business relocation in California. But given changes in both the economy and in data collection (discussed more below), there is clearly a need for much-updated evidence on this question, especially because current policy debates (at least in California, which we have studied) place great emphasis on business relocation but rely solely on anecdotal evidence.

Aside from these two substantive issues, Birch's work is criticized by Davis, et al. (1996) because of more general criticisms of the D&B data. They note two main problems with these data, documented in earlier research. First, the D&B data appear to overstate total employment as measured by the Bureau of Labor Statistics or the Census, a problem we also discuss later in this paper. It is worth keeping in

[12] Davidsson, et al., also question their conclusions, because they are based on inferences about the behavior of small firms in the overall economy using plant data from the declining manufacturing sector in the United States. Some of the papers cited above that build on this work also explore the small firm conjecture.

mind, though, that the two main measures of overall employment in the U.S. economy – the Current Population Survey (CPS) household survey and the Current Employment Statistics (CES) payroll survey – also have a large discrepancy, which has averaged as high as seven or eight million, and often show quite different trends.[13] There are some well-understood explanations of some of the differences between these two surveys, such as the payroll survey excluding the self-employed. The point, however, is that the measurement of employment is complex, and treating one measurement as 100 percent correct, and another that differs as obviously wrong, is overly simplistic. Below, we discuss differences in alternative measures of employment levels using the CPS, the CES, and the NETS database, to try to shed additional light on these measurement issues.

Second, Davis, et al., express strong concerns about the measurement of employment at small and new establishments in the D&B data, which is relevant in light of Birch's conclusions about the contribution of small firms to job creation. They cite evidence indicating that the D&B data files used by Birch did a very poor job at tracking new businesses, failing to identify over 95 percent of new businesses identified from other sources (Birley, 1984; Aldrich, et al., 1988).[14] We should be cautious in interpreting these findings too strongly, aside from the issue raised earlier of whether these findings apply to data from the more recent past. Birley's study is based on a comparison of new firms captured in the ES-202 data, the D&B data, and telephone directories, restricted to St. Joseph County, Indiana.[15] She begins with new firms identified in the ES-202 data, and then takes the subset of these also found in the telephone directories (77 percent of the total). She finds, however, that the D&B files only capture 12 percent of the new firms identified from the ES-202 data, and fewer if other less fundamental errors (such as the founding date) are considered.[16]

[13] See, e.g., U.S. Bureau of Labor Statistics (2004).
[14] In contrast, Bednarzik (2000) claims – although he does not provide evidence – that the D&B data overstate openings and closings. Davis, et al., also refer to unpublished figures from the U.S. General Accounting Office indicating that the D&B files do not do very well at measuring mass layoffs, which is of less direct concern for our work.
[15] This identification is based on firms showing up in one of the information sources in one year but not the previous year.
[16] Both Aldrich, et al. (1989, p. 372) and Leonard (1987, p. 142) note that the ES-202 data have trouble distinguishing births from changes in ownership or in legal corporate structure, because in each case a new ES-202

Aldrich, et al. (1998) compare ES-202 data, the D&B data, and an enumeration method based on a physical census and phonebooks. They find that the enumeration method appears to work considerably better, in the sense that it captures far more new firms than either of the other methods. But in contrast to Birley's results, the D&B data and the ES-202 data have about the same overlap with the enumeration data, each capturing one-third of the cases captured by the enumeration method. But the enumeration method captured far more new firms – 77 percent of the union of sets of new firms captured by at least one method, versus 38 percent for the D&B data and 43 percent for the ES-202 data. Both papers note that, at the time, the D&B data included only businesses for which D&B had completed a credit check, or mainly those businesses that sought a credit rating. This could explain the low capture of new firms.[17] However, the findings of Aldrich, et al., indicate that the ES-202 data are roughly equally problematic, in contrast to Birley's conclusion.[18] Specifically, of those new businesses identified by the enumeration method, only 10 percent were found in the D&B data, and 14 percent in the ES-202 data. Also, 42 percent of firms identified as new in the ES-202 file were found in the D&B file, a much higher number than Birley reported (Aldrich, et al., pp. 377-378). Interestingly, Aldrich, et al., attribute this to "substantial improvements in the [D&B] coverage since Birley's research was completed," (1989, p. 378), highlighting the importance of assessing the NETS database (actually, the underlying D&B data) anew, given vast improvements in information technology since that work was done. Below, we describe efforts to check the capture of new establishments in the NETS, among our other efforts at assessing the quality of the data.

Some other work that was not specifically focused on Birch's findings also assessed the D&B data from the late 1970s and early 1980s, when the U.S. Small Business Administration contracted with the Brookings Institution to use these data to create the United States Establishment and Enterprise (USEEM) file, which linked establishments cross-sectionally (with parent firms) and over time. This

number is issued. It is not entirely clear how this affects Birley's conclusions, given the scanning she did of telephone directories, as well. The more recent work by Spletzer (2000) with the ES-202 data appears to have solved this problem (p. 3).

[17] Allaman (1978) also suggests that the D&B data from this period had lags in reporting new firms.

[18] "… the ES202 files are significantly more efficient in identifying new firms than the other two data sources" (Birley, 1984, p. 64).

work pointed to some coding and related errors common to most data sets, problems with coverage of single owner-operator establishments in a few sectors, and overall higher counts of employment in small establishments compared to Census data because D&B count owners/managers and partners in their employment totals. Overall, though, this research concludes that the coverage of the file is quite accurate and timely (MacDonald, 1985, p. 180).

Audretsch (1995) provides some further analysis of the quality of the USEEM, comparing employment change for the U.S. as a whole with similar measurements from the BLS (the payroll survey) and from County Business patterns, as well as employment growth by establishment size based on the BLS data. He reports a high degree of correspondence between the different data sources, but also some differences, in particular in employment growth in manufacturing. He also compares the size distribution of establishments in the USEEM and the U.S. Department of Commerce's "Enterprise Statistics" for 1982, and for this period finds that the USEEM undercounts small establishments, and undercounts employment at smaller establishments. This may mirror some of the coverage issues discussed above. It is also worth keeping in mind, as we remind the reader below, that it is not obvious that any one particular data source is the "gold standard," but rather each data source likely has its own imperfections.

Below, we provide an assessment of the more recent D&B data, including coverage of the D&B data overall and of new businesses. It is important to note that there was a dramatic change in the data collecting process at D&B in 1991. In particular, on July 25, 1991, a federal court ruling allowed regional Bells to sell information they collected (United States v. Western Elec. Co., 767 F. Supp. 308 (D.D.C., 1991)). In 1992, therefore, D&B started to use yellow pages to identify business units, which greatly expanded its database. This resulted in a significant surge in the number of establishments and jobs in the NETS data in that year, which we expect to have seriously mitigated earlier problems with coverage by the D&B data. Consistent with this, as we show below, the D&B data now detect *more* very small establishments than do other data sources.

3.2 Data from the U.S. Census of Manufactures, the Longitudinal Research Database, and the Longitudinal Business Database

With the availability of establishment-level U.S. Census data for the manufacturing sector, several studies moved beyond the data studied by Birch and his associates and broadened the inquiry to a more general understanding of employment dynamics. Using data from five Census of Manufactures (CM) – for the years 1963, 1967, 1972, 1977, and 1982 – Dunne, et al. (1989a) examine gross and net job flows caused by the opening, closing, expansion, and contraction of manufacturing plants.[19] There are some limitations of these data that are germane to the potential value of the NETS data. First, they exclude plants with five or fewer employees, for which the Census Bureau does not collect data.[20] Second, they are of course limited to manufacturing, which may not be representative of the entire economy (see, for example, Foote, 1998). Third, because the data come at five-year intervals, the dynamics that occur at higher frequency cannot be studied. And fourth, like with the other data sources described below, the data do not permit tracking physical moves of plants.

Echoing some earlier work using different data discussed below, as well as a good deal of later work building on theirs, Dunne, et al., document that gross flows are much larger than net flows, implying that relatively smooth net aggregate employment changes (in manufacturing) reflect far more tumultuous changes through the creation and destruction of jobs through births and deaths of establishments, as well as expansion and contraction of continuing establishments. For example, Dunne, et al., note that net employment fell by 3.8 percent from 1977 to 1982. But this change can be decomposed into a 17.6 percent gross increase in jobs from plant openings, an 11.7 percent increase from expansion of existing plants, a 17.7 percent decline due to plant closings, and a 15.4 percent decline due to contraction of continuing plants. Yet another way to think about these data is in terms of the contribution to net employment change of changes in the number of establishments (births minus deaths) relative to changes in the sizes of continuing establishments (expansion minus contraction). The first

[19] In related work using the same data, Dunne, et al. (1989b) study the post-entry growth and failure patterns of manufacturing plants.
[20] This is a simpler description of a slightly more complicated rule (Davis, et al., 1996, Technical Appendix).

difference is −0.1 percent, and the second −3.7 percent. More generally, over the four periods they study – connecting the five Censuses – they generally find that gross changes due to births and deaths are often quite close, and instead that net employment changes are driven more by differences between expansion and contraction of continuing plants.

Davis and Haltiwanger (1992) study job creation, destruction, and reallocation across manufacturing plants, using the Longitudinal Research Database (LRD) covering the period between 1972 to 1986. And in a comprehensive book-length study, Davis, et al. (1996) used a later version of the LRD data to paint a statistical portrait of job creation and destruction in the U.S. manufacturing sector from 1972 to 1988. The LRD is built by combining data from the CM (discussed above) and the Annual Survey of Manufactures, a probability sample of manufacturing establishments done each year.[21] Combining these yields a panel data set on establishments (which may enter or leave depending on inclusion in the sample) with which to carry out similar analyses to those described above, albeit at higher frequencies than the five-year window that can be studied with the CM data. This higher frequency is an advantage of the LRD, although it shares the other limitations of the CM data. Like the CM data, the LRD can track births and deaths, and can distinguish births from ownership changes. The findings from the LRD echo those of Dunne, et al. (1989a) and the other work summarized below in pointing to the quite high gross job flows that underlie net employment changes in manufacturing.

Finally, a newer data effort at the U.S. Census Bureau is the Longitudinal Business Database (LBD), which creates an LRD-like database covering all industries (Jarmin and Miranda, 2002).[22] The LBD is based on the Standard Statistical Establishment List (SSEL), a register of all U.S. businesses and establishments.[23] The LBD covers all in-scope establishments with employees, much like the ES-202 data. In principle, the LBD data permit the study of business relocation, since there is an establishment-

[21] Plants with approximately 250 workers are sampled with certainty in each year.
[22] It is worth noting, though, that the most of the public sector and some small parts of the private sector are considered "out-of-scope" for the Census Bureau, and for these sectors data in the LBD are not broken out by establishment.
[23] The SSEL is also known as the Business Register. An earlier, shorter longitudinal data base constructed in the same way as the LBD is the Business Information Tracking Series (BITS), also created at the Census Bureau, and sponsored by the Small Business Administration (Robb, 1999).

level identifier – the Permanent Plant Number (PPN) that stays with the establishment – and the SSEL contains information on the location of each establishment in each year. As discussed by Jarmin and Miranda, for the estimation of sources of gross job flows there are serious concerns with separating true births and deaths from spurious ones generated by broken linkages of establishments over time, a problem also noted with the LRD. They engage in a number of procedures, including matching on names and addresses, to minimize the spurious births and deaths, but some problems remain. This is also slightly problematic because matching on addresses, when linkages are broken for other reasons, implies that moves will be treated as births for some establishments, but not for others; in no case, though, has existing work classified business relocations using the LBD. [24] To the best of our knowledge, the LBD has not been used in virtually any research. The only study we have found is by Foster (2003), on establishment and employment dynamics in Appalachia.

3.3 Data from Unemployment Insurance records

Another data source that has been exploited by researchers in this area is the Unemployment Insurance (UI) data. Employers in the United States are required by law to file quarterly UI reports to state governments, and these data essentially provide a quarterly census on employers in all industries. The UI data are useful for measuring the contribution to employment change of births and deaths of establishments, and expansion and contraction of existing establishments. A principal advantage relative to the CM and LRD data is that the UI data cover employers in all sectors and all size categories, with the exception of business establishments that do not have employees – which figure in the ensuing analysis of the NETS. However, like the CM and LRD they are not useful for capturing establishment relocations, especially across state lines where – because the UI data come from separate state systems – there is no way to distinguish, say, a new establishment from one that moved from another state.

[24] Jarmin and Miranda (2002) also point to some other problems with the LBD. Two are relevant to comparisons with the NETS data. Some problems stem from missing data in earlier years that make it harder to match establishments and thus overstates births and deaths. Another problem is that "data quality may be poor for establishments with few observations in the LBD" (p. 18), in part because newer establishments are less likely to have been surveyed by the Census Bureau. Below, we discuss similar problems with measuring employment in the NETS in the early years of firms' existence.

Research by Leonard (1987), using Unemployment Insurance (UI) records for establishments in the state of Wisconsin from 1977 to 1982,[25] is probably accurately described as the study that opened up more general questions of the dynamics of employment, in contrast to the earlier work stemming from Birch's conjecture about the contribution of small firms to employment growth. Leonard focused to some extent on the structural versus frictional unemployment debate, but also provides more general information on which we focus here. He documents the relative magnitudes of gross versus net job creation and destruction, reporting that, on an annual basis, each year gross job creation adds about 13.8 percent to the job base, and gross job destruction eliminates about 11 percent of jobs, leaving a net change of only 2.8 percent.[26] In other words, relatively small changes in employment may mask substantial "reallocation" in the labor market, which he defines as gross turnover, or the sum of job creation and destruction rates. Leonard also finds that most gross job flows – 82 percent of the gains and 89 percent of the losses – are attributable to expansion and contraction of existing establishments, with births and deaths contributing much less.

More recently, Spletzer (2000) used UI data to examine the contribution of business establishment births and deaths to employment growth in West Virginia, covering 1990-1995. Besides presenting new evidence, this paper introduces a new longitudinal database developed by the BLS – referred to as the BLS longitudinal database, or LDB – which makes use of the ES-202 data to study employment dynamics.[27] A significant advance in the LDB is the ability to capture data at the establishment level via what are called "Multiple Worksite Reports" for reporting UI data.[28] This had been a problem in earlier work with UI data, and had led to criticism of using this data source from Davis, et al. (1996).

[25] He states that most of the data come from single-establishment firms, having eliminated at least some reports that cover multiple establishments in the same company.
[26] Note that these flows do not refer to worker-level separations and accretions to establishments, but to changes in employment levels at the establishment level, as of March of each year.
[27] The ES-202 data collection program is also known as the Quarterly Census of Employment and Wages (QCEW) program.
[28] See, also, Pivetz, et al. (2001).

Spletzer devotes considerable attention to measurement issues, including both the measurement of births and deaths, and the sensitivity to the time horizon used of the contributions of births and deaths to employment change. It is easy to see why the latter issue is significant. In the extreme, with a long enough time horizon all employment change must come from births and deaths, and none from expansion and contraction of existing establishments. Conversely, if establishment growth and decline are gradual, then a short time horizon means that employment growth due to births and employment decline due to deaths will be downplayed, although there is also an influence in the other direction, as only a short time horizon will detect changes from short-lived establishments. Spletzer decomposes employment change into the components due to births and deaths, and expansion and contraction of existing establishments, using data at different frequencies. Reflecting the expected sensitivity to the window used, Spletzer finds that about 20 percent of job creation is due to births using a quarterly frequency, rising to 40 percent using an annual frequency and 56 percent using a triennial frequency. The figures for the contribution of establishment deaths behave similarly, with corresponding numbers of 19, 41, and 60 percent.

The U.S. Bureau of Labor Statistics (BLS) has used the linked ES-202 records to create a data series called Business Employment Dynamics (BED), which reports on findings from the LDB. The BED tracks gross job flows (creation and destruction) at the national level as well as disaggregated by industry. These can also be decomposed into openings and expansions (for job creation), and closings and contractions (for job destruction).[29] These data can be used to study gross flows at the regional level as well,[30] although currently the BLS does not make such disaggregated data available. To date, to the best of our knowledge, only BLS researchers have had access to the raw LDB data underlying the BED. Researchers in the state agencies that are responsible for assembling the ES-202 data (the states' Labor Market Information Divisions) have also done some work on employment dynamics in their state.[31]

[29] Details as well as sample data releases are available at http://www.bls.gov/bdm/home.htm#news.
[30] See, e.g., Faberman (2002).
[31] See Hardiman and Holden (2003), and the discussion of some related work therein.

3.4 The Longitudinal Employer-Household Dynamics program

The newest longitudinal data set on establishments that permits the measurement of gross job flows is the Longitudinal Employer-Household Dynamics (LEHD) program, housed at the U.S. Census Bureau (U.S. Census Bureau, 2002). The LEHD is also based on the ES-202 data, but goes beyond the existing data sources in tracking the individual workers in these establishments, which it does by accessing their social security numbers. This permits a great many important questions about worker mobility, careers, wage variation within and across firms, etc., to be addressed with a richness that has not previously been possible, although one limitation is that the worker-level data are matched by firm, not establishment. However, with regard to studying business establishment dynamics that do not focus on individual workers, the LEHD offers essentially the same thing as other data sets based on the ES-202 data.[32]

3.5 Advantages of the NETS

Based on the discussion to this point, we see a few important advantages to the NETS data. Perhaps most important, from the perspective of fully characterizing employment change – as depicted in Figure 1 – the NETS is critical, because it capture business relocation. Unlike the other data sources described above, the NETS database tracks business address changes and identifies business moves over time within the entire country. As discussed below, this is important because business retention and attraction issues are often at the center of policy debates at the state (and local) level.

In addition, though, the NETS offers a few important advantages in actually carrying out research on employment dynamics. Access to the alternative data sources described in this section is highly restricted because of confidentiality reasons, and hence requires a long and complex process of application and approval. As a practical matter, this has deterred many researchers from pursuing research with these data, and has clearly made it difficult to do research in a timely manner. In addition, again because of confidentiality, researchers working with these data sources are restricted in the geographic detail to which they can disaggregate in describing results. And this confidentiality

[32] Benedetto, et al. (2004) explore exploiting the LEHD to improve tracking of entry and exit of firms as well as administrative changes by following clusters of matched workers.

sometimes extends to studying and certainly extends to identifying particular companies. With the NETS data, in contrast, none of these problems arise. There are not confidentiality restrictions imposed on the users of the data.[33]

4. Assessment of NETS Database

We have used three strategies to assess the reliability of the NETS data. First, we compare the NETS data with alternative data sources that are publicly available to assess the accuracy of measurements of employment levels and changes. Second, we search business relocation cases reported in the media and check whether they are captured by the NETS data, which is particularly important given our focus below on business relocation.[34] And third, we use phonebooks to try to identify business establishment births and assess the accuracy with which the NETS tracks such births.

In all cases, the reader is reminded that there are complexities involved in each of these measurement exercises, and it not clear that any one particular data source is the "gold standard." Thus, our analysis does not focus solely on whether the NETS "measures up" to these other data sources, but instead discusses the strengths and weaknesses of each and the degree of correspondence between them. There is a lot more to learn about these measurement differences, and we suspect that the potential advantages of the NETS will spur further assessments that build on those we carry out here.

4.1. Measurement of employment levels

One approach to assessing the NETS database is to compare its estimates of employment levels and changes with similar estimates from other sources. We can do this at a point in time, looking across detailed geographic regions and industries, and can also compare changes over time in alternative data sources. The data products that can be used for these comparisons are: the Quarterly Census of Employment and Wages (QCEW); the Current Employment Statistics (payroll) survey (CES); the Size of Business data (SOB); and the Business Employment Dynamics data (BED). With the exception of the

[33] The NETS data are not freely available, and are not inexpensive. For example, a two-year license for the California file we use in this paper costs $15,000. But the time plus money costs may actually be quite favorable, compared to the alternative data sources.

[34] Below, after reporting on decompositions of the sources of employment change using the NETS, we also report corresponding evidence from other data sources.

payroll survey, all of these are constructed from the ES-202 data. Details on these alternative data sources are provided in Table 1.

What is not available from these alternative data sources is the detailed establishment level information such as address, employment, start date, close date, industry code, etc. These data sets only provide aggregate statistics at various geographic, industry, or establishment size levels, and thus it is only at these levels that we can compare the data sources.[35]

We begin by comparing employment level measurements in the NETS to those in the Quarterly Census of Employment and Wages (QCEW). We do this at the most disaggregated level at which QCEW data are publicly available – by county and one-digit SIC industry. Figure 2 plots the data for the alternative measurements of employment by county and industry from the two sources. If the measurement agreed exactly, then they would all lie on a 45-degree line, which is drawn in the figure. It is clear from visual examination of the figure, as well as the extremely high computed correlation of 0.994, that employment levels between these two data sources correspond very closely. On the other hand, the points actually lie on a line that is flatter than the 45-degree line, implying higher employment levels in the NETS.[36] We return to this issue below. Several other figures and correlations comparing employment at higher levels of aggregation reveal similar relationships to those in Figure 2. We use 1997-2000 for the comparison with QCEW data because earlier years were not readily available, and subsequent years use the North American Industrial Classification System (NAICS), and hence cannot be directly compared.

We constructed a similar figure for comparing employment measurements in the NETS with those in the Current Employment Statistics survey (CES). Figure 3 reveals a similar pattern, and a correlation of 0.948; again, the line traced out by the points is below the 45-degree line, indicating that measured employment is higher in the NETS.

[35] The publication of such employment/establishment data is sometimes withheld in order to protect the identity of cooperating employers. For example, QCEW is suppressed if there are fewer than three establishments, or if a single employer makes up more than 80 percent of the employment in that cell.

[36] The points that are farthest off the line, at high employment levels, are for service-related industries in Los Angeles. However, these points actually lie quite close to a regression line through the data.

To assess the quality of employment measurements in the NETS by establishment size, we also examined the correspondence between employment as measured by the NETS and by Size of Business (SOB) report; for this latter source employment can be measured by industry and size of establishment (as well as county, of course) as can also be done in the NETS database. As Figure 4 reveals, here the data correspond less well, and the correlation falls to 0.817. Looking at employment by establishment size, we believe that the greater discrepancies reflect the fact that the NETS database has much higher coverage of small establishments than does SOB. This is documented in Figure 5, in which we compare the NETS to SOB employment data for 2002. In 2002, the NETS database reports 184% more employment in establishments with 1-4 employees than does SOB, and 29% greater employment in establishments with 5-9 employees. But for every other business size category the NETS and SOB employment levels are within 10% of one another. This apparently greater coverage of employment in small establishments (1-9 employees) also helps to explain why comparisons with QCEW or CES do not reveal a one-to-one correspondence in employment. We cannot address this issue as directly as we can with the SOB data, because the QCEW and CES data are not available by establishment size. However, we can easily see that there are many fewer QCEW establishments than NETS establishments.[37] At the same time, whereas NETS total employment is 15-30% greater than QCEW total employment, establishments covered in NETS are 35-60% more numerous than establishments covered in QCEW, implying that small establishments are under-represented in the QCEW data.

Part of the disparity in employment and the number of establishments indicated by the SOB and NETS datasets for small establishments might be driven by the fact that business owners are typically excluded from coverage under the ES-202 unemployment insurance system (although they are permitted to pay UI taxes and be covered).[38] This could be quite important for the smaller establishments in which a single owner can represent a sizable share of total employment. Figure 6 shows the correspondence between the two datasets if we remove one employee from each establishment covered in the NETS

[37] Unfortunately, CES does not include a series for the number of establishments. Therefore, a similar comparison of average establishment size is impossible. Because CES is periodically benchmarked to UI universe counts, we would not expect that results would be much different.

[38] This same point was noted earlier in work by MacDonald (1985).

database, while keeping the assignment to each establishment size category fixed. What we see is that the discrepancies for the smaller establishments (0-4 and 5-9 employees) shrink considerably, suggesting that it is, indeed, the inclusion of business owners in the NETS data that accounts for a sizable share of the difference in employment at small establishments in the NETS and the SOB data. However, it might also be necessary to reassign NETS establishments to size categories based on the adjusted employment level, after subtracting one employee from each establishment. The results are shown in Figure 7, and indicate that the employment discrepancy at the smallest establishments grows somewhat, although it is still nowhere as large as in Figure 5.

With respect to comparisons of employment counts for the other establishment size categories, there is a complication introduced because, as documented in some detail below, there is a good deal of rounding of self-reported establishment size, and as it turns out there is much rounding to the employment levels that are the left-hand "edges" of the size categories into which the SOB data are classified (10, 20, 50, 100, 250, 500, and 1000 – but not 5). Consequently, if we eliminate one employee and reassign establishments to size categories based on the adjusted employment level, we get excessive shifting of establishments to smaller size categories. This is reflected in the results.

At the upper end, in the largest size category (1000+), Figure 6 shows more employees in the NETS data than in the SOB data, whereas once we reassign size categories, in Figure 7, this is reversed. This is exactly what we would expect if establishments with somewhat fewer than 1000 employees are rounded to 1000 in the NETS data, and hence included in the 1000+ category in Figure 6 but not in Figure 7. As further confirmation, as shown in Figure 8, establishment counts were initially greater in NETS for all establishment size categories larger than 9 employees, but after reassignment to new size categories the establishment counts are fewer in NETS for the same categories. This is consistent with the effects of rounding, which, interacted with the boundaries of the SOB categories, results in assignment of establishments to too high a size category prior to the reassignment to size categories, and the opposite result following the reassignment.

Similarly, Figure 9 shows that, whereas in the NETS data the mean number of employees at each establishment was generally less than the average number of employees per establishment indicated in SOB data at all establishments in each size category with more than 4 employees, when we adjust the size classification after subtracting one employee the average number of employees at each NETS establishment is greater than that of SOB establishments for each respective size category, reflecting the same rounding phenomenon that results in many establishments in each category in the NETS data lying on the edge of the size categories into which the SOB data are classified. Moreover, the fact that – whether we look at establishment size or employees per establishment – the numbers from the NETS with and without reassignment to size categories generally bracket the SOB numbers indicates that the NETS data are relatively accurate, aside from the rounding. However, as discussed in the next section, the rounding does have implications for the measurement of employment change.

Finally, in Table 2, we attempt to account for differences between the NETS database and the SOB data described above by examining employment data for 1994-2002. The first two rows of the table indicate the total employment levels reported in the NETS and the SOB report, respectively, for each year. Since SOB data only include individuals earning wages that are covered by unemployment insurance, several categories of workers that are reported in the NETS, most notably the self-employed and independent contractors, are excluded from the SOB by statute.

To estimate the number of individuals who are either self-employed or independent contractors, we use data reported in the Contingent Work Supplement (CWS) to the Current Population Survey (CPS) in years 1995, 1997, 1999 and 2001. In 1995, for example, we see that 13,047,314 individuals were reported on payrolls in California, and that 2,093,767 Californians were self-employed or working as independent contractors. In the third row of the table, we combine these two figures to arrive at an approximate level of household employment in California. Thus, in 1995, there were 15,141,081 individuals reported on payrolls, self-employed, or working as independent contractors.

As we see in the fifth row of the table, our approximation of household employment in California falls short of the level of employment reported in the NETS database for each of the years we examined.

It is instructive to note that while the NETS over-reports household employment in comparison to our approximation in the third row of the table, this approximation itself overstates household employment when compared to the CPS employment figures for each year, as shown in the last row of the table. For example, in 1995 the NETS overestimates our approximation of household employment by 1,100,075, while at the same time, this approximation overstates CPS employment by 1,101,233. The differences between the NETS and the SOB+SE/IC series may be partly explained by some self-employed or independent contractors having multiple businesses – all of which should show up in the NETS, but not in the SE/IC series, where an individual is counted only once. On the other hand, this cannot account for the CPS versus SOB+SE/IC difference. Overall, while the NETS does use a more comprehensive approach in estimating employment in each year, our comparisons indicate that measurement error could account for some of the remaining discrepancy between the NETS and SOB employment data. However, as revealed by the table, there are discrepancies across any pair of data sets one chooses to compare.

4.2 Measurement of employment change

Next, we turn to measurements of employment change. We first begin by documenting, in Figures 10 and 11, the rounding of employment in the NETS data. These figures show that for both smaller and larger establishments – as reported in the data – the distribution of the number of employees is disproportionately concentrated on numbers that are divisible by 5, 10, 100, and so on. For example, in 2000, 63.4% of establishments with 10 to 100 employees reported employment that was a multiple of 5 or 10, and 40.3% of establishments with 100 or more employees reported employment that was a multiple of 50 or 100. On the other hand, on an establishment (rather than employment) basis, this is somewhat less of a concern; rounding is less perceptible at establishments with fewer than 10 employees, which comprise 81.2% of all establishments in 2000.

While employment rounding may bias some of our estimations, it is not a particularly serious problem for the measurement of employment levels if we believe that employment numbers are rounded to the closest "salient numbers." In that case, our aggregate levels are unbiased estimates because some people round their numbers up and others round them down, and the establishment-level measurements

may contain measurement error that is largely random (although non-classical). It does, however, mean that employment change is "sticky," and that our estimates likely underreport the frequency with which establishments change their levels of employment, thereby underestimating the degree of employment reallocation caused by establishment expansion and contraction.

Another potential source of stickiness in the measurement of employment change in the NETS is imputed data. Table 3 shows that, during 1993-2002, more than 50% of each year's employment figures are coded as "actual figures." The share of "actual" employment figures has increased steadily from 52% in 1993 to approximately 64% in 2002. Of the remaining establishment records, 1-2% are recorded as "bottom of range" estimates, 7-29% as D&B estimates, and 6-34% as Walls estimates.[39,40]

Some of the differences in imputation are attributable to age of establishment. We are considerably more likely to have actual employment figures for older establishments, as shown in Table 4. In contrast, D&B estimates are much more common for young firms (1-3 years old) than older firms. Older establishments are also much less likely to have employment estimated by Walls, though this decline with respect to age is much more gradual than that for the D&B estimates.[41]

Turning to the issue of the persistence of employment, Table 5 shows that there is a fairly high degree of persistence in employment imputation. That is, there is a relatively high likelihood that the same imputation procedure was used for the current year as the previous year, as evidenced by the large percentages on the diagonal cutting from the upper-left to the lower-right corner. However, notice that the percentages below the diagonal in the first column are also large, implying a high rate of transition

[39] D&B imputes based on cross-sectional information, whereas Walls & Associates also use time-series information and should therefore obtain better imputations (and does so when the D&B imputations appear suspect). The share of D&B estimates fluctuated substantially over the period. The sharp fluctuation of the share of Walls & Associates imputations is less mysterious – as the last step in employment estimation, it is simply determined by the fluctuation of the other sources of employment information. There are many cases where suspect employment figures imputed by D&B are re-imputed estimated by Walls & Associates.

[40] The share of employees that are covered by these different means of imputing employment data diverges from the share of establishments, as different types of imputation are more or less common for establishments of different sizes. Overall, the average number of employees per establishment during 1993-2002 is between 9 and 12. Establishments with actual employment figures or bottom of range estimates are slightly larger, with 12-15 and 14-19 employees per establishment, respectively, while the average number of employees per establishment with D&B employment estimates is only 2-3. Employees per establishment with Walls employment estimates ranges from 7-9 between 1989 and 2001, but unexpectedly jump to 16 in 2002.

[41] Some of the low average employment per establishment we found for D&B estimated establishments might be explained by the fact that such estimates primarily cover young firms.

from imputed to actual data, especially for the bottom of range and Walls estimates; this reflects the same point noted above, that imputation is a feature of establishments' earliest appearances in the database.

We also examined the pattern of employment imputation for establishments over time in more detail than is afforded by Table 5. In particular, Figure 12 plots the distributions, for each number of possible years in the data set, of the number of years with actual data. The larger the circle used in the plots, the greater the share of observations at that point (the area is proportional to the share at each point on the horizontal axis). To fix ideas, if there were no imputed data – and all data were actual – then all of the mass would be along the 45-degree line between years with actual data and years in dataset, with large circles along this line. What we tend to see, instead, is that establishments that are tracked for a relatively short period of time exhibit a bimodal distribution, with either no years with actual data or all years with actual data. But establishments that are tracked in the dataset for a longer period are much less likely to have no years without actual employment data, and conversely have relatively more years with actual data; and the mode is to have actual data for all years.

Together, rounding and imputation of employment data results in infrequent year-to-year changes in employment. Among existing establishments, 7.6% and 16.3% reported a change in the number of employees in 1993 and 2002, respectively, and 19.6% and 14.1% of workers were at establishments that reported a change in the number employed in those years. While the fraction of establishments that report a change in employment level varies substantially from year to year, it usually falls somewhere between 5-15%. And at establishments for which D&B imputed employment, there is a much lower frequency of establishment-specific employment changes, with the exception to 2002 when 42% of D&B estimated establishments report a change in employment levels. Table 6 shows the incidence of employment change by type of employment data.

Finally, the implication of these measurement problems is that the NETS data compare less favorably with other data sources when we look at employment changes, rather than employment levels, especially for high-frequency (short-term) changes. As shown in Figure 13, the correspondence between NETS and QCEW yearly first-difference employment changes by industry and county is not very strong,

with a correlation of only .528. However, if we look at employment changes over periods of at least a few years, this problem is substantially mitigated, as the correlation rises to .864 (Figure 14).

This greater correspondence of employment changes over longer intervals is consistent with what we would expect based on the findings noted above regarding rounding and imputation. With rounding, the data will likely more accurately measure employment changes over a longer period, because rounding results in small changes being ignored but larger changes being measured. Similarly, we saw that imputation tends to be a feature of establishments' first appearance in the data set, whereas over time actual data are more likely to be reported and hence employment changes better measured. The implication of these findings is that the NETS database should not be used for measuring very short-term employment changes, but is more useful for measuring employment changes over periods of a few years or more. This does present a tradeoff, however, as an inability to focus on short-term changes inhibits our ability to observe high-frequency changes in job creation and destruction, such as over the business cycle. We also note that making the unit of analysis for employment change longer affects what proportion of employment change we attribute to job creation and destruction versus establishment expansion and contraction, and to a much lesser extent relocation, a point to which we return below.

4.3 Tracking business relocations

A unique feature of the NETS dataset is its ability to track establishment relocation and births. Our next task, then, is to assess how well D&B's methods track relocating establishments. There are no other comprehensive longitudinal datasets with which to compare measurements of geographic movement of establishments over time. In lieu of such comparisons, we used Lexis-Nexis to conduct newspaper searches of business relocations involving California establishments, and conducted a detailed comparison of evidence on relocation in the NETS database to evidence found in theses searches. Our search was not meant to be exhaustive; it is only intended to obtain a replicable sample of press coverage of specific business relocations.

We limited our search to the Los Angeles Times and the Kiplinger California letter. The L.A. Times has the largest circulation of any California newspaper and the Kiplinger California Letter is a

concise bi-monthly business newsletter that has a section specifically reserved for business relocation reports. As expected, the Los Angeles Times has a regional bias in that it almost exclusively focuses on business moves in Southern California, especially the Los Angeles region. Business relocations in other regions are reported only if they are high-profile or reflect a move between the Los Angeles region and the rest of the state. The Kiplinger California Letter shows a much more balanced coverage of business moves in different regions in the state.

Using a carefully-designed search algorithm,[42] we focused on 1,067 newspaper articles from the L.A. Times (1996-2000), from which we were able to identify 576 references to specific instances of business relocation. In many cases, multiple articles referred to the same occurrence of relocation. As such, 124 duplicate references were excluded from our sample.

Of these 452 unique observations, 237 business relocations were confirmed as valid moves by the NETS database. An additional 47 were confirmed as "invalid" moves: 5 moves turned out to be consolidation of businesses because the establishment at the destination already existed before the move; 17 cases were planned moves but did not occur later; 12 of the establishments at "destination" were new branches instead of relocated businesses; and 13 moves involved establishments such as schools and nonprofits that are not the focus of our research.

In total, 58.5% (237/{452 − 47}) of the valid business relocations that we identified from the L.A. Times could be found in our NETS dataset. This rate of confirmation varies dramatically depending on the distance over which the relocation occurred. We are able to confirm only 27% (21/77) of within-city moves, whereas we are able to confirm 70% (177/252) of between-city, within-state moves, and 74% (37/50) of cross-state moves.[43] It is neither surprising nor worrisome that NETS detects only a relatively small share of within-city moves because short-distance moves are much less significant for the scope of research for which this database is most useful. In fact, many of such moves occur over such short

[42] We experimented with several search terms. Our final choice of search term – "ATL2(RELOCAT!) AND BUSINESS AND (MOVE OR MOVING OR MOVED) AND (SECTION("BUSINESS") OR SECTION("METRO"))" – was guided by a desire to exclude irrelevant articles. We elaborate the particulars of this process in Appendix A.

[43] Not all of the 405 valid moves can be classified in terms of origin and destination.

distances that they could not possibly be identified within the NETS database. For instance, several contacted establishments said that the moves had occurred, as indicated in the newspaper article, but the new location was adjacent to or "across the street" from the previous location. The NETS is designed to only report "significant moves," which are defined as moves where both the street address and zip code information change. This criterion was chosen to avoid mistaking the changing boundaries of zip codes for actual moves.

For the reported relocations not confirmed in NETS, we undertook thorough efforts to independently verify whether there was in fact a relocation. It turns out to be very difficult to use other information sources to locate the establishments whose relocations are reported in the media but for which there is not an obvious match in the NETS. Ideally we would contact the establishment directly and confirm that the reported relocation occurred. However, this becomes very difficult when establishments (or often, businesses) can be acquired by other firms or for other reasons currently do business under a different name, or no longer exist. Naturally, these problems are more severe in trying to verify reports of relocation that are relatively old. Nonetheless, when possible we contacted the establishments directly. We also searched for company information using Hoovers.com[44] and Lexis-Nexis Company Information Search – web-based resources that track company addresses and would reveal new addresses for companies that changed location. Of 168 reports of relocation that we could not locate in the NETS database, we were able to independently verify that 18 relocations indeed occurred. And not one of the 18 was a cross-state move. Despite our best efforts using the methods described above, we were unable to confirm the remaining 150 reports of relocation from the Lexis-Nexis. And at least 91% of these companies (136 out of 150) are captured by the NETS database with no relocation indicated. Furthermore, 92 (68%) of these establishments were still in existence through 2002, although we were only tracking relocations that were reported between 1996 and 2000. If these establishments had relocated, but not been tracked properly as relocations by NETS, then these establishments would have

[44] Hoovers.com utilizes the same raw data provided by the DMI file as the NETS database. However, the search mechanism is very flexible, sometimes making it easier to locate establishments that could not be found through company keyword searches in the NETS database.

reported closing years close to the date of the relocation. Thus a large proportion of the remaining moves reported in the media may not have happened at all, suggesting that our confirmation rates should be thought of as lower-bound estimates.

Our search for reports of business relocation in the Kiplinger California Letter (1996-2001) revealed similar results. Of the 79 incidents of relocation we identified in this search, 12 were found to be misreports of establishment relocation. Of the remaining 67 media observations of relocation, 35 (55%) were confirmed in the NETS database. In addition, 3 cases were confirmed in Hoovers.com, but occurred too recently to be found in NETS, and 29 cases could neither be confirmed nor denied.

We certainly do not expect every relocation to appear in Lexis-Nexis, but we do expect all real relocations that are covered in the media to also appear in the NETS. Given the difficulty of checking whether reported cases actually occurred, it is impossible to quantify exactly what share of real relocation is captured in the NETS. However, for moves crossing city boundaries, we estimate that the share is well over three-quarters, based on the fact that most of the cases not captured by NETS cannot be independently confirmed as real relocations. Thus, we conclude that the NETS database appears to do quite a good job of tracking these business relocations. However, in contemplating the empirical results on establishment relocation discussed later in this paper, one might want to adjust upward modestly the job creation and destruction attributed to relocation.

4.4 Capture of new business establishments

Given the concern expressed in earlier research regarding the ability of the D&B data to track new establishments, and the importance of establishment births in job creation that has been documented in other work, it is also important to assess how well the NETS tracks new business establishments. We do not have access to ES-202 data on new establishments, but we can compare the NETS data to new establishments identified from phonebooks, following the earlier work by Birley (1984). Specifically, we selected a random sample of establishments from the 1999-2001 San Francisco Pacific Bell Business White Pages. We were primarily interested in identifying businesses that are initially *not* in the phonebook, but then show up in a later year, as a means of identifying an alternative list of new

establishments. We randomly chose to start with listings beginning with "B." There should be no relation between the name of the business and the likelihood of its inclusion in the NETS database, so simply beginning at this point and working forward seemed as valid as randomly sampling from different beginning letters. Preliminary investigation suggested that business establishments that use initials in their name (such as "B & G auto rental") may change names from year to year, so we instead began drawing our sample with telephone listings that started with "Ba." We chose enough observations to get approximately 60 new establishments, which required 313 listings that appeared in the phonebook at least once between 1999 and 2001. These 313 listings contained 35 records with area codes from outside the code covering San Francisco (415). We excluded these 35 records from the analysis because businesses from outside the area code presumably have to pay to be listed in the business white pages, meaning that the appearance or disappearance of such businesses would often occur for reasons unrelated to opening (or closing). Of the remaining 278 records, 156 are tracked for all 3 years, 51 are present for two years, and 71 are only listed in only one of the phonebooks. These records indicate 58 openings (and 61 closings).[45]

Of 58 openings identified from the phonebook search, 52 (90%) of the establishments could be identified in the NETS database. Many listings were difficult to match because companies often do business under multiple names, and because of differences in spelling or abbreviations. Thus, this matching required that we also try to match using company name keyword search, phone number reverse lookup, address information, or alternate company names provided by workers at the particular establishment. While many of the NETS opening dates corresponded well with those indicated by the phonebook listings, many did not; see Figure 15. Given the disagreements, we attempted to obtain each business establishment's opening year directly from the company, or through their website. We were able to obtain approximate start date information from 33 of these 52 establishments. Our comparison reveals that many of the opening years that were indicated by changes in phonebook listings were inaccurate, as

[45] There were also three records for which the listing appeared in 1999, was absent in 2000, and reappeared in 2001 with the same name and phone number. This is one indication – more are described below – that the phonebooks do not provide a completely accurate means of tracking openings and closings.

reported in Figure 16. The figure reveals that while the phonebook method necessarily assigns these openings to 2000 or 2001, the actual openings are spread over a long span of years. In contrast, the NETS data match opening dates much more accurately, as indicated in Figure 17.[46] For those establishments that could not be reached directly, the NETS and phonebooks start dates were generally in agreement. Presumably this is because many of these were young establishments that failed subsequently, so there was not much scope for the start dates to differ.[47]

To this point, we have discussed tracking establishment births. As another check on the NETS data, we also attempted to locate NETS records for business establishments that were listed in the San Francisco phonebook for all three years. If NETS records indicate opening or closing years within 1999-2001, then we might be concerned that NETS is inaccurately reporting the timing or incidence of openings or closings. We randomly chose 72 of the 156 records in the phonebooks in all three years, and we were able to locate 66 (92%) in the NETS database, which represents a slightly higher percentage than those that we could identify from the earlier subset of phonebook-inferred openings. Of these records, according to the NETS data all but six continued to exist through 2002 and only one record indicated a closing by 2001,[48] indicating a close correspondence between the NETS and phonebook data for continuing establishments.[49]

Our inquiry regarding the ability of the NETS to track establishment births suggests that the data are quite accurate, although this turns out to be based more on direct checks with businesses themselves than on correspondence with openings as identified by new appearances of businesses in phonebooks. Phonebook listings apparently fail to accurately measure start dates for a number of reasons. In many

[46] Even if the NETS data were completely accurate, we would not expect an exact correspondence with the start dates obtained from our efforts to contact businesses directly. In our phone calls, we often talked to employees who had limited tenure and did not know the founding date, in which case we were only able to obtain information that provided a lower bound for the number of years that a particular establishment had been in business.

[47] The start dates in the NETS for the establishments for which we were unable to confirm the opening year were 1979 (1), 1995 (1), 1997 (1), 1998 (2), 1999 (1), 2000 (8), 2001 (5).

[48] According to NETS, this last establishment (an attorney) closed in 2000, but a phone call to the listed number indicated that this establishment is still in operation. Of the other five business establishments that, as reported by NETS, discontinued operations prior to 2003, phone calls verified that the listed number was no longer working.

[49] Given the inaccuracies in openings based on the appearance of businesses in the phonebook, and the difficulty of verifying information directly with companies that have closed, we deemed this method inappropriate for assessing the ability of the NETS data to identify establishment closings.

cases, new phonebook entries corresponded to new listings of individual employees at already established firms, such as attorneys at law firms. Also, efforts to identify openings using changes in phonebook listings from year to year are complicated by the fact that there are many duplicate listings, where different phone numbers were listed for the same establishment, or the same phone number (and possibly same address) was listed for a given establishment under different company names. We would still like to do more to verify that the NETS tracks closing accurately, but this is difficult given that phonebooks are relatively inaccurate, and it is difficult to verify information on businesses no longer in existence.

5. Relocation and the Business Climate Debate in California

5.1 The debate over business relocation

We use the NETS data to address a substantive question that is relevant to the overall issue of business establishment dynamics, which often figures prominently in policy debates, and for which the NETS database is uniquely suited. Specifically, business relocation is often cited as a source of job loss, especially at regional and state levels.[50] Back in the 1970s, the relatively rapid growth in the Sunbelt states was usually thought of as a result of massive business migration from traditionally industrial regions to the South and West (Allaman and Birch, 1975). While job loss due to business relocation is rarely precisely measured, it is often invoked in the rhetoric of policy debate and political campaigns. For example, when Kimberley-Clark moved its headquarters from Wisconsin to Texas in 1985, it sparked heavy criticism of Wisconsin's business climate, leading the governor to lose his job in the following election (Dresang, 2002). Similarly, local governments work actively to attract companies to relocate to their jurisdictions (Klier and Testa, 2002). For example, in 2001, when Boeing announced that its headquarters would leave Seattle, cities like Chicago, Dallas, and Denver engaged in fierce competition to recruit the company.[51]

During the past decade, the debate over business relocation has been prominent in policy discussions in California. In the early 1990s, California experienced an economic downturn that

[50] The recent heated debate over outsourcing to developing countries indicates that this also occurs at the national level.
[51] See, for example, http://www.conway.com/ssinsider/incentive/ti0106.htm (viewed May 2, 2005).

coincided with federal defense cutbacks triggered by the end of the Cold War. California's alleged poor business climate became a target for harsh criticism from both politicians and business leaders. Business executives, in particular, contended that job loses in the state stemmed from California's hostile business environment (see, for example, Groves, 1992; Howe, 1993; Vartabedian, 1993; and Weikel, 1992). To make matters worse, relocation consultants, corporate headhunters, and policymakers in nearby states, such as Nevada, Arizona, Washington, Utah, Texas, and New Mexico, recognized an opportunity to lure businesses from California (Howe, 1993). It was reported in 1992 that Nevada budgeted $400,000 to recruit discontented businesses from California, and in Arizona a special legislative session was convened to approve economic incentives for luring aerospace firms from California (Weikel, 1992). When Intel was planning a $1 billion expansion, Ann Richards, then Texas Governor, flew to Intel's California headquarters with a group of mayors in order to persuade Intel to consider Texas as a potential location for its plant (Howe, 1993; Weikel, 1992).

Then-Governor Pete Wilson designated two blue ribbon commissions (chaired by George Schultz and Peter Ueberroth, respectively) to investigate California's supposedly deteriorating business climate (Schrag, 1998). Ueberroth's Council on California Competitiveness issued a long report filled with recommendations for improving the business climate (Council on California Competitiveness, 1992). It called for a reform to overhaul the costly workers' compensation program, bewildering regulations, and the declining education system, while endorsing tax reforms to promote entrepreneurship and business investment. However, the economic boom of the latter part of the 1990s, fueled by the technology sector in California, seemed to make the public and policymakers forget their concern with the state's business climate, albeit only temporarily.

The issue resurfaced during the economic downturn that followed the bursting of the Internet bubble in 2000-2001. In April 2003, Representative Duncan Hunter (R-CA) organized a summit in San Diego on whether or not California was becoming increasingly inhospitable for businesses. Hunter told businessmen and government officials that regulations, fees, taxes, high energy costs, and rising salaries

and compensation were making California unattractive when compared to neighboring states – and that many firms are believed to have left California for just those reasons (Freeman, 2003).

Finally, during the recall election in 2003, the public was inundated with criticism of California's business environment. Candidates for governor routinely referred to the state's "onerous business regulations and over-taxation" (Roberts, 2003) that were believed to push businesses away. On September 22, 2003, two weeks before the voters went to the polls to decide on the recall, and a few days after the state legislature passed a new health insurance mandate, a group of 38 chief executives ran a full-page ad in the Los Angeles Times, the San Diego Union-Tribune, the San Francisco Chronicle, and the San Jose Mercury News. It included an open letter to Governor Davis, candidates for governor in the recall election, and members of the California Legislature, the central message of which was that taxes, regulations, and mandates were making it harder and more costly to do business in California, resulting in diminished faith of the business community in California's management. These chief executives demanded that the state policymakers should "put a moratorium on new taxes, fees and regulatory mandates," "reinstate the manufacturer's investment tax credit," and "cut back the overall tax burden on business."

After Arnold Schwarzenegger won the recall election and became Governor, he adopted an aggressive strategy focusing specifically on business relocation. In August 2004, billboards were erected in cities such as Atlanta, Boston, Dallas, Las Vegas, Phoenix, Portland, and Seattle, featuring an arms-folded Schwarzenegger with a California state flag T-shirt reading: "Arnold Says: 'California wants your business.' (Actually, he says, 'Kah-li-fornia.')." He has also dubbed his big moving truck "Arnold's Moving Co." and promised to lend it to any business owner who wants to move to California. It is unclear whether this kind of campaign would actually influence business decisions, but it certainly provoked a response. Only two months later, in October 2004, big signs appeared in Los Angeles, San Francisco, and San Diego featuring a picture of Massachusetts Governor Mitt Romney. Its message apparently responds to Schwarzenegger's challenge: "Smaller muscles, but lower taxes! Massachusetts means business." At the same time, a Nevada Economic Development coalition put up "wallscapes" on

buildings in Los Angeles, Sacramento, and San Francisco, with images of a beaten and bruised worker below the question "Will your business be terminated?" (Tamaki, 2004). The same picture also appeared in major California newspapers.[52]

5.2 "Evidence" underlying the business relocation debate

Claims regarding the importance of business relocation that have arisen in this debate, however, have rarely relied on empirical evidence of relocation behavior. Rather, they have generally relied on surveys that elicited subjective assessments from employers. For example, in 1991, a statewide survey conducted by the California Business Roundtable reported that close to one-quarter of the 1,462 responding companies had plans to leave California, and another survey in Orange County raised similar concerns (Weikel, 1992). And later, at the height of the most recent focus on business relocation (in 2003), the California Chamber of Commerce and the California Business Roundtable released their 13th Annual Business Leaders Survey, in which they asked 400 California business executives about relocating. Nearly one-fifth of California businesses reported that they were planning to expand and/or relocate outside of the state, and 15 percent of the businesses said they were approached by recruiters from other states, among which 51 percent were offered monetary or other relocation incentives.[53] The next annual survey by the California Business Roundtable and Bain & Company (2004) painted an even grimmer picture, reporting that close to 40 percent of company executives surveyed had a plan to relocate businesses and jobs away from California and that most of them planned to move to other western states. Of course, whether these plans had any correspondence with subsequent observed behavior remains an open question.

In the only study we have come across that tries to measure actual relocation activity, the Los Angeles Department of Water and Power, together with several large utility companies in Southern California, commissioned a study of the relocation of manufacturing plants. This study reported that California lost 1,035 industrial facilities to other states, Mexico, or Puerto Rico between 1980 and 1992, including both those that moved out of California and those set up outside California by California

[52] See, for example, the San Francisco Chronicle, January 31, 2005, p. E5.
[53] The survey results are available at http://www.calchamber.com/index.cfm?navid=463.

companies (although it is not clear the latter group should be counted as losses). Taking into account the multiplier effect, they estimated a loss of job opportunities ranging between 168,000 and 224,000 in that period (Bules & Associates, 1992). A critical limitation of this last study, however, and a missing element in all of the discussions about relocation that we have cited, is that business relocation is almost always discussed in the context of firms leaving California, as if traffic moves in only one direction. The study by Bules & Associates (1992) focused exclusively on California's losses, without even mentioning that firms also moved to California and that companies in other states also set up branches here in California. One could argue that when companies such as Gateway, Iomega, and Sony Electronics moved their headquarters from other states to California, this was a positive development that mitigates any negative effects of relocations from California to other states.

Moreover, the debate is often framed as if relocation is the key determinant of employment change. Yet the formation of new business establishments, the death of existing ones, and employment changes at continuing establishments, also affect employment change. Thus, the debate over business relocation largely ignores five of the six arrows in Figure 1 that describe the dynamic processes underlying employment change, focusing only on the loss of jobs from establishments that move out of the state. Of course, each of these sources of job creation and destruction is potentially important in determining employment change, and it is misleading to focus on only one of these – and even then, without good evidence. The NETS data can obviously fill in many of the gaps in understanding the importance of business relocation, in both directions, in employment change, and more generally in highlighting which processes – births, deaths, expansions, contractions, moves in, and moves out – drive employment change.

6. Evidence on Business Relocation and Employment Dynamics in California

This section presents our findings regarding business relocation in California and employment dynamics more generally, using the NETS data. At noted earlier, because D&B's coverage increased sharply when they started to use yellow pages to identify business units in 1992, we exclude the 1989-

1991 data available in the NETS from our analysis.[54] In each case, we state our key result, and then provide some detailed discussion.

1) California generally loses establishments and jobs due to business relocation, but the impact is negligible

As Table 7 shows, in every year during the 1993-2002 sample period, some establishments left California, taking jobs away; at the same time, others moved into California, thus bringing jobs to the state. Measured by either the number of business establishments or the number of jobs, California experienced a net loss owing to relocation in every year. The fact that there was never a net gain in any of these 11 years is indeed quite striking.[55]

However, compared with the size of its overall economy, California's net loss from relocation has to be considered negligible. In terms of number of establishments lost to other states, the worst years are 1993 and 1994. In each of these years, California lost about 750 establishments to other states, which amounted to 0.05% (five one-hundredths of a percent) of the total number of establishments in California. To put this in perspective, at that rate it would take about 20 years for California to lose 1% of its business establishments. The job numbers tell a similar story. In terms of job loss from relocation, 1994 and 1997 represent the worst years. In these years business relocation cost 0.1% (one-tenth of a percent) of California jobs. At this maximum speed, the leakage of jobs would take 10 years to eliminate 1% of California's jobs. Another way to see that these job change numbers are negligible is to compare them to ongoing changes that the state experiences. For example, based on state economic statistics from the California Employment Development Department (EDD), from July 1990 to January 1993 6.1% of California jobs disappeared, while from December 1997 to December 2000, employment in California

[54] This implies that we will measure relocation beginning only in 1993, because a move in 1992 would be detected by comparing an establishment's 1992 address with its 1991 address, and without the 1991 data, moves in 1992 are unidentifiable.

[55] We also examined the effect of relocation in 1991 and 1992, which also show net loss in both years in terms of establishments and jobs.

grew by 8.2%.[56] These comparisons suggest that whether during an upturn or a downturn, business relocation simply does not play a major role in employment change.

2) Establishments are much more likely to move locally than across state boundaries

While establishment moves are quite common, most of these moves are within state. Out of 255,838 cases of establishment relocation originating in California during 1993-2002, 246,283 (or 96.3%) were moves within California. While cross-state moves draw a lot of attention, they are rare cases. In fact, 35.4% of all the moves originating in California occurred within a city and 78.5% of the moves did not go beyond the county boundary.[57] Because there are fewer establishments moving in than moving out of the state, within-state moves represent slightly higher proportions of relocations to California destinations.

As a result, the impact of relocation on employment on the local level, while still modest, is more pronounced than its effect on state employment. In 1993, though less than 0.01% establishments moved out of California, 0.4% of establishments moved outside their own county, and 1.2% of establishments moved beyond their own city. The employment changes associated with these moves represented 0.1%, 0.6%, and 1.5% of total California employment, respectively.

3) Inter-state moves are more likely to occur between California and other western states

When establishments do move across state boundaries, distance still seems to play a role. Table 8 shows that when business establishments moved out of California, they were likely to go to other western states. In particular, Nevada, Arizona, Texas, Oregon, Washington, and Colorado top the list of destination states. Nevada is far ahead of any other state, attracting 57% more establishments from California than Arizona, which is second on the list. Even by the number of jobs eliminated by out-migration, western states still stand out, although North Carolina and New York move closer to the top of

[56] See http://www.calmis.cahwnet.gov/file/lfhist/cal$shlf.xls (viewed May 3, 2005).
[57] As discussed in the previous section, within-city moves may be undercounted in the NETS, in which case these percentages would be even higher.

the list. Given that many of these western states have a smaller economy than the average state, this relationship is even more striking.

Among establishments that moved into California during 1993-2002, New York tops the list of origin states. But western states still ranked fairly high: Nevada, Texas, Arizona, Washington, Colorado, and Oregon are all among the top 10. In terms of jobs moved to California, relocations from New York, New Jersey, Texas, and Illinois greatly outnumbered other states. Given that the Nevada, Arizona, and Oregon economies are fairly small, the larger number of establishments moving between these states and California must have a lot to do with their proximity to California. Also, although we show that business relocation has had a negligible impact on the California economy, it may have been a considerably more significant factor for the smaller western states.

It is worth noting that California did not experience a net loss of establishments (or jobs) to all other states. For example, far more establishments came to California from New Jersey and New York than relocated in the opposite direction. And while the pattern of losing jobs to other states from relocation, on net, is more pervasive, relocations between California and New Jersey added far more jobs in California.

4) Employment growth is primarily driven by expansion, contraction, births, and deaths

Table 9 presents decompositions of employment growth over every three-year period during 1992-2002. For each period, in the top panel we show California employment in the starting year, in the ending year, the overall net change, and then the number of jobs created or eliminated by each process of employment dynamics. The bottom panel shows the decomposition of employment change. In principle, we can decompose annual employment changes in the same way. But as noted earlier, year-to-year employment changes are not as reliable in the NETS data because of rounding and imputation; the problem should be much less serious for employment changes over longer periods.

Table 9 shows that in every year the expansion of existing establishments always creates more jobs than are lost through the contraction of existing establishments. This is perhaps not surprising,

because at any time we expect that the surviving business establishments tend to be those that are growing rather than shrinking. The net effects of births and deaths of establishments on overall employment change are positive in some years and negative in others. This tends to reflect the business cycle. In boom years many new establishments are created, and at the same time existing establishments are less likely to go out of business. As a result, jobs created by new establishments outnumber jobs eliminated by establishments that close in such years. Conversely, during slower economic times business formation is lower and more businesses tend to close, result in a net loss of jobs because new businesses do not suffice to cover the loss of those that die. This is reflected in the table. For example, during 1995-1998, establishment deaths in California cut 454,000 jobs more than the number of jobs created through establishment births. But during the next three years, from 1998-2001, business establishment births and deaths resulted in a net gain of 848,000 new jobs.

Finally, consistent with the results in Table 7, establishment relocation always had a negative effect on employment change in California. The important new information provided by Table 9 is the comparison of the contribution of relocation to employment change with the contributions of other sources of employment change. The bottom rows of Table 9 indicate just how small the role of business relocation is. As the last row shows, the employment loss from relocation ranges from about 6,000 to 44,000, averaging around 20,000 per year. But the employment changes from the expansion-contraction processes and the birth-death processes are much greater, often by a factor of 20 or more. In other words, employment changes in California are primarily driven by expansion-contraction and birth-death processes, rather than by relocation.

The relative importance of different types of establishment dynamics in generating employment change is illustrated more clearly in Figure 18. The two panels in Figure 18 display the sources of job creation and destruction, respectively, in each three-year period during 1992-2002. The top panel shows that, without exception, in each period the birth of new business establishments is the major source of job creation, while the expansion of existing establishments is also important. The number of jobs created by business establishments that moved to California is trivial compared to the number of jobs created by the

other two sources. Likewise, the bottom panel shows that the death of establishments is the major factor in job destruction. Contraction at existing establishments is also substantial but less important. Finally, business relocation out of California again contributes only minimally. But recall that, on net, it is expansion minus contraction that generally yields the largest share of employment growth, and is always positive.

5) Decompositions of the sources of employment change are sensitive to the interval over which the change is measured, but regardless, the contribution of business establishment relocation is negligible

There is an important caveat to the type of decomposition provided in Table 9 and Figure 18. If we take the whole period 1992-2002, then the interval length over which we measure employment changes over time has no impact on the net job growth that we observe over the total period. However, the magnitude of gross job creation and destruction, as well as its decomposition, is dependent on the interval length. Intuitively, as the interval gets shorter the gross flows become larger because more employment change due to fluctuations are captured. Also, the longer the interval chosen, the greater the contribution of births and deaths to gross flows. To see this most simply, note that all establishments in existence during a period are born and die during that period as the period gets infinitely long. However, recall that we argued that the NETS was not as reliable when looking at employment changes over shorter time intervals.

Nonetheless, Tables 10 and 11 illustrate how the interval length influences the share of employment change that we associate with each process of employment change, and the magnitude of gross flows overall. Table 10 shows that when we look at shorter intervals the gross changes due to each of the six dynamic processes are larger. And Table 11 shows that the shares of both gross job creation and gross job destruction attributable to births and deaths, respectively, are larger the longer the interval over which we look. There is, unfortunately, no "right" answer as to what time interval should be used to characterize the process of employment change. But what these tables show is that conclusions about the

contributions of each process to employment change are sensitive to this interval. Regardless, though, whatever interval is used, employment change due to establishment relocation is negligible.

This sensitivity of the decomposition of employment change to the interval length also makes it difficult to compare results from the NETS with results from other data sources. There are a few studies and data sources, from those discussed in Section 3, that provide results overlapping with the period we cover with the NETS data. Results from the BED and LDB are available on a quarterly basis for the United States as a whole, and results from ES-202 data directly (for the earlier 1990-1994 period) are available at a quarterly frequency, and at frequencies of one, two, and three years, for West Virginia. Of course, all of these data sources are ultimately derived from the ES-202 data, and hence none have information on establishment relocation. In addition, these alternative data sources, following the ES-202, exclude independent contractors, self-employed, contingent workers, and workers at certain nonprofit organizations.

For the purpose of comparison with the NETS data, which is only available annually, we use the seasonally-adjusted BED series. However, because the BED data are quarterly, they detect sharp fluctuations in employment changes and associated job creation and destruction due to seasonal events, such as weather, reduced or expanded production, harvests, major holidays, and the opening and closing of schools.[58] Moreover, as just discussed, with a shorter interval we expect to find higher shares of gross job creation and destruction attributable to expansion and contraction, and lower shares attributable to births and deaths. The same comments apply to the other sources reporting these data at a quarterly level. This is confirmed in the first four columns of Table 12, which report the employment change decomposition for the three quarterly sources, and for the NETS using a one-year interval. In the NETS, at a frequency of one year, the role of expansion and contraction is about half that in the quarterly sources, and the role of births and deaths roughly triple.

[58] According to the Bureau of Labor Statistics, the effect of such seasonal variation can be "very large" and is "likely to obscure" other changes. An overview of the BED data is available at http://www.bls.gov/bdm/bdmover.htm, viewed May 3, 2005.

The ES-202 data for West Virginia also provide measurements on an annual basis, and as the next column of Table 12 shows, the larger role assigned to births and deaths is not solely a function of the interval length. In this alternative data source, about 40% of gross job creation is attributed to births, compared with nearly 60% in the NETS, and the corresponding numbers for the contribution of deaths to job destruction are 41% and 66%. Part of the explanation for the smaller shares of job creation and destruction attributed to expansion and contraction in the NETS may stem from the earlier "stickiness" in employment change, discussed earlier, resulting from rounding and imputation of employment in the NETS; this remains an important area for future research. Of course the data sources do cover two very different states in periods with little overlap, which may also help account for the differences. Note, though, that as we extend the interval to two and to three years, the discrepancies between these two data sources lessen, although some differences remain. The better match as we move to the three-year interval is consistent our earlier conclusions that employment change measures in the NETS become more accurate as the window lengthens, and for windows of three years the measures are quite accurate.

6) Births and deaths of business establishments contribute by far the largest share of job "reallocation" underlying net employment change, while establishment relocation is a trivial source of reallocation

To this point, we have focused in large part on the net changes in employment generated by the dynamic process of establishment births and deaths, expansion and contraction, and mobility. It is also instructive to characterize employment change in terms of the job "reallocation" that the different dynamic processes depicted in Figure 1 create. To understand what we mean by reallocation, for a given net employment change due to, for example, births and deaths, consider two alternative scenarios. In one, the gross employment changes from births and from deaths are low, and in another they are high (but still offsetting to the same degree). In the latter scenario with large gross changes, many establishments are created and many die. What are the implications for employment change? Although the net effect is the same, when gross flows are high workers experience more reallocation, because all jobs are eliminated at

establishments that close, and new jobs are created at establishments that open. Thus, when the sizes of the gross flows are higher workers experience more reallocation. Indeed, we find it useful to define rates of reallocation for births and deaths, expansions and contractions, and relocation, to see how each of these three sets of dynamic processes contributes to the amount of job changing in the economy. The measures we define are as follows:

Expansion-contraction job reallocation =

(job growth at expanding establishments + job decline at contracting establishments)/total employment

Birth-death job reallocation =

(jobs created by new establishments + jobs eliminated by establishment deaths)/total employment

Mobility job reallocation =

(job at establishments that moved in + jobs at establishments that moved out)/total employment

These three measures can be thought of as roughly capturing the amount of job changing created by the dynamic processes that underlie employment change, as employment changes underlying each of these reallocation measures entail a worker leaving one job and a worker taking another job. Of course the correspondence is not perfect because some workers leave the workforce and others enter. And it is a lower bound because there can be job turnover unaccompanied by changes in employment at the establishment level.

Figure 19 displays these measures of job reallocation, for the same three-year periods covered in the earlier decompositions. The figure shows that the births and deaths of business establishments contribute by far the most to job reallocation, even if, as shown earlier, births and deaths sometimes cancel out and make smaller contributions to net employment change. Expansion-contraction job reallocation is also sizable, although about one-half or less that of birth-death job reallocation. Finally, the bottom line in the graph shows that compared with other types of employment dynamics, employment relocation plays a trivial role. That is, it is not just the net effect of relocation that is small, but also its overall contribution to reallocation in the labor market. Business establishment relocation neither

contributes much to net job change in the California economy, nor to movement of workers into and out of jobs.

The measures of reallocation are also informative about the potential for each of the dynamic processes underlying employment change to lead to more dramatic variation in employment. Given that births and deaths contribute large gross flows into and out of employment, a quite modest change in the balance between births and deaths could lead to dramatic shifts in net employment change. In contrast, a change in the imbalance between establishments moving into and out of California seems unlikely to ever have much of an impact. This is important to keep in mind in thinking about the potential implications for changes in business relocation activity to impact overall employment growth in the economy. The very low reallocation associated with relocation implies that even if the rate of mobility out of the state doubled, and establishments completely ceased to move into the state, there would be very little impact on net employment change. In contrast, a much smaller relative change in the birth or death rate of business establishments – whether attributable to a policy change or some other shift – could lead to far sharper changes in employment.

7. Discussion and Conclusions

In this paper, we assess and present findings from a newly constructed longitudinal database covering business establishments – the National Establishment Time Series, or NETS. The NETS database is particularly well-suited to studying the underlying dynamics of employment change, specifically the processes of business establishment expansion and contraction, births and deaths, and relocations. As such, it builds on earlier research on this topic using the Longitudinal Research Database to study manufacturing, and numerous other data products based on the ES-202 data to study all sectors of the private economy. However, the NETS has some important advantages, including capturing business relocation, more complete coverage, and the ability to disaggregate to a fine geographic level, as well as ease of access and the absence of confidentiality restrictions, which permits researchers to study particular companies.

At the same time, the NETS is based on Dun & Bradstreet data, which in the past have come in for criticism. As a consequence, we devote a great deal of attention to assessing the quality of the NETS. Overall, we conclude that the NETS data are quite reliable and in many respects comparable to the more frequently used administrative and Census data. It captures new businesses and start dates quite accurately. Coverage of business moves in the NETS is good, which enables researchers to tackle a source of job creation and destruction that has been understudied. One limitation is that because data are often initially imputed for new establishments, and there is considerable rounding of employment, short-term employment changes are not measured very accurately; we suggest that a minimum interval of three years be used.

Partly as an illustration of the value of the NETS data, and partly out of substantive interest, we use the data to study employment dynamics in California. We provide overall decompositions of the sources of employment change in the state and also provide some descriptive information on job reallocation, which captures the rate at which new jobs are created and old ones end. And we focus particular attention on the empirical importance of business relocation into and out of the state, which has figured prominently in policy discussions.

Overall, we find that the birth-death and expansion-contraction processes of business establishments are responsible for nearly all gross job creation and destruction, and that cross-state business relocation is virtually a negligible factor. We also find that the job creation and destruction process involves continuous rematch between a large proportion of jobs and workers, with the largest contribution to this rematch stemming from births of new establishments and deaths of existing ones. implying a high level of labor market reallocation that is not reflected in the relatively smooth aggregate employment statistics. Regarding business relocation, we find that cross-state business relocation resulted in a net job loss to California in every year during the period 1993-2002. However, there is no sign of a substantial business exodus from California, and compared to the size of the California economy, the net loss from relocation is negligible.

What are the policy implications of these findings? At the outset, it is critical to emphasize that our results minimizing the role of business relocation do not imply that there is no merit to claims that the business climate in California is inimical to job growth. As our results emphasize, births and deaths of establishments, and expansions and contractions of existing establishments, play a hugely important role in employment change, and if public policies or other factors are deterring births and expansions, or encouraging deaths and contractions, then there could be merit to criticisms of the business climate. By the same token, however, given the negligible role of business relocation in employment change, it is important to be wary of arguments appealing to stories about businesses leaving California as evidence of an adverse business climate. We have no doubt that, at any point in time, there will be case of businesses leaving California, and other cases of businesses elsewhere choosing to relocate to states other than California. But the availability of such anecdotal evidence does nothing to establish a trend or even a change in behavior. And given the far more dominant roles of births, deaths, expansions, and contractions, we are skeptical that changes in relocation behavior, even if they are occurring, would be of sufficient consequence to serve as a barometer of the business climate.

Our findings have more limited implications. First, a focus on business relocation is badly misdirected, and unlikely, even if successful at attracting new businesses and retaining old ones, to contribute visibly to job growth. To the extent that policy has any role to play, the evidence suggests that efforts to foster the formation of new businesses and to help existing businesses survive and grow would be better placed. Second, at least as far as we can tell thus far, there has been little if any change in the rate at which businesses are leaving California or failing to come here. There is a potential caveat to this conclusion. Our data at present extend only through 2002. We therefore cannot rule out the possibility that the anecdotal evidence that has most recently been raised regarding business relocation captures an emerging trend. However, if this trend turned out to be empirically significant with respect to overall job growth, it would have to represent a break from past behavior that differs by an order of magnitude, at least – a break that we regard as unlikely.

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Figure 1: Employment Dynamics – A Schematic Representation

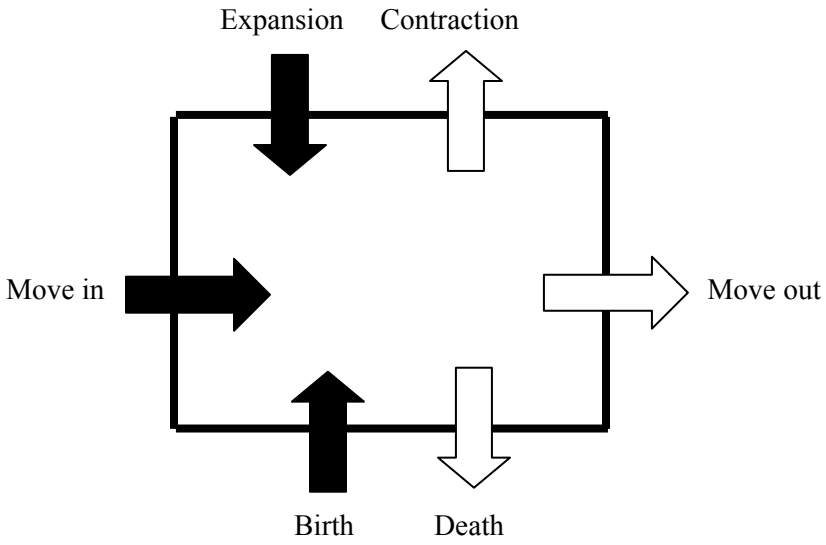


Figure 2: NETS and QCEW Employment by County and Industry (1997-2000)

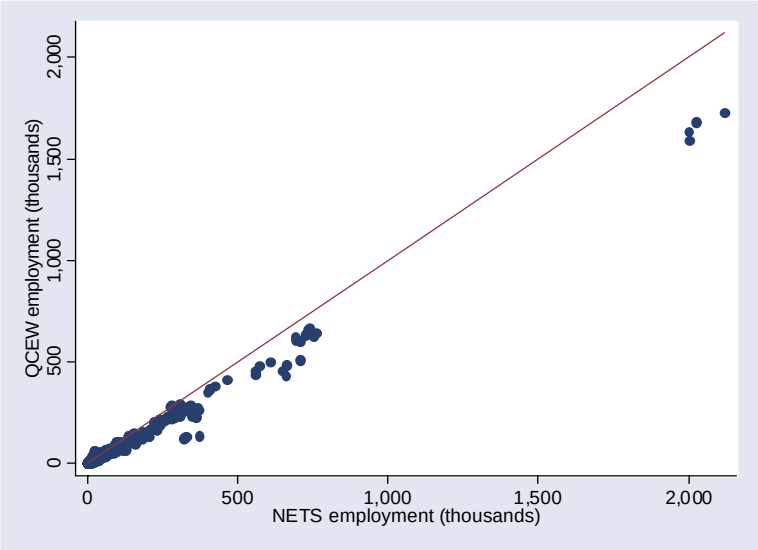


Figure 3: NETS and CES Employment by County and Industry (1989-2002)

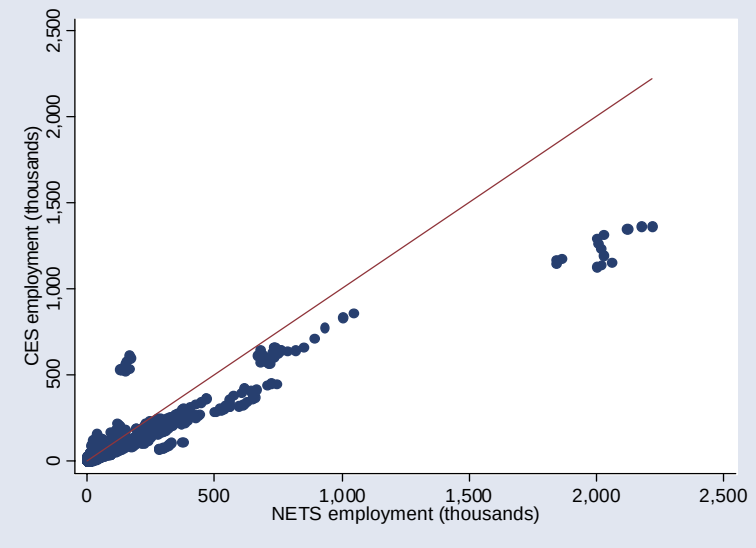


Figure 4: NETS and SOB Employment by Industry and Size Category (1994-2002)

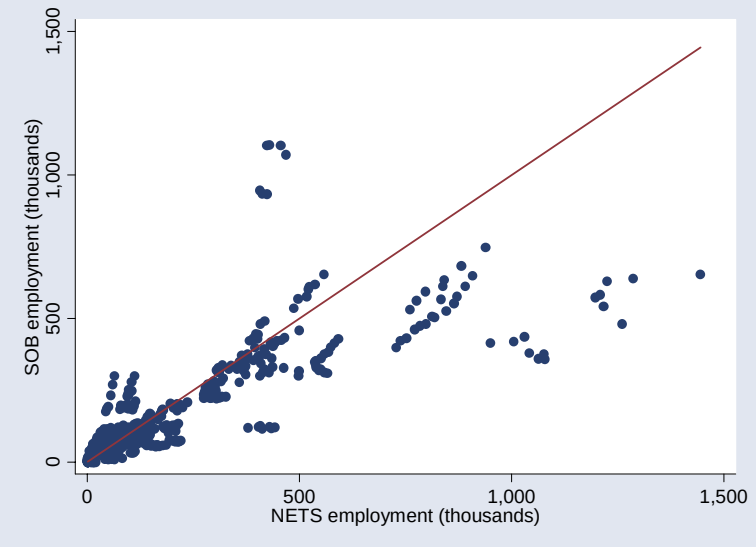


Figure 5: NETS and SOB Employment by Size Category (2002)

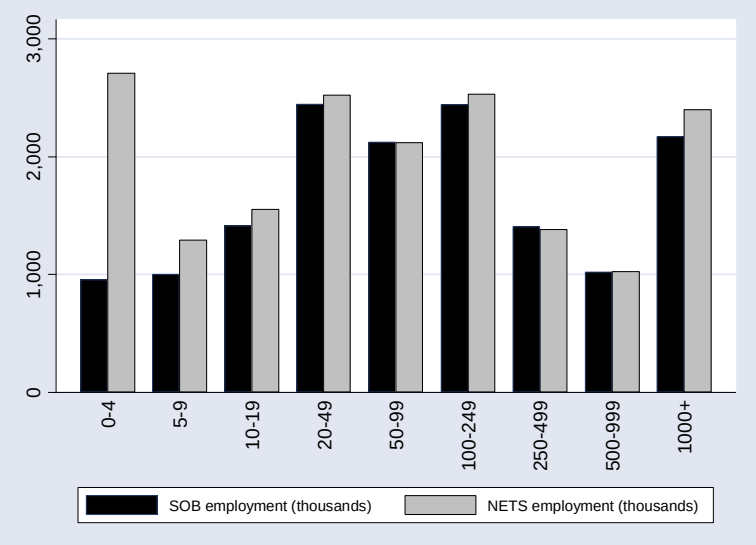


Figure 6: NETS and SOB Employment by Size Category (2002), Subtracting One Employee from Each Establishment

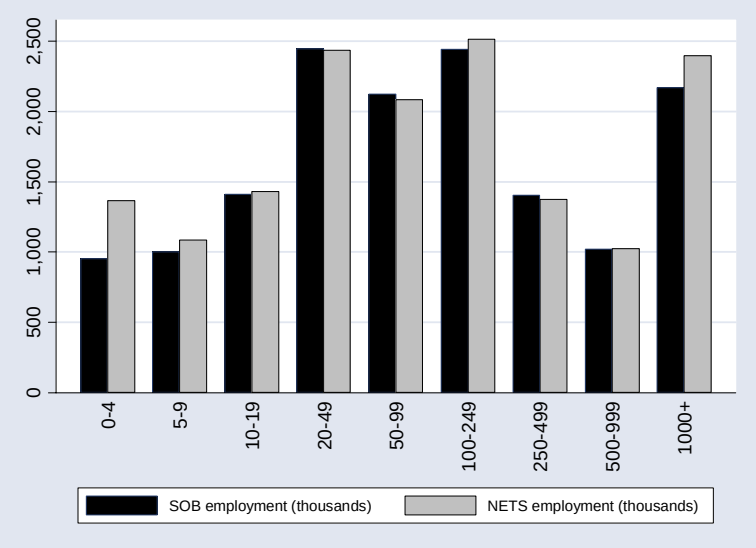


Figure 7: NETS and SOB Employment by Size Category (2002), Subtracting One Employee from Each Establishment, and Reassigning to New Establishment Size Categories

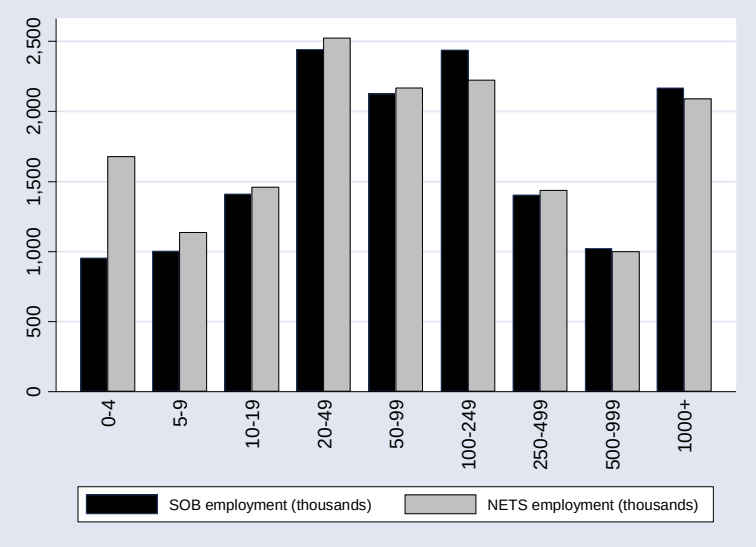


Figure 8: NETS and SOB Number of Establishments

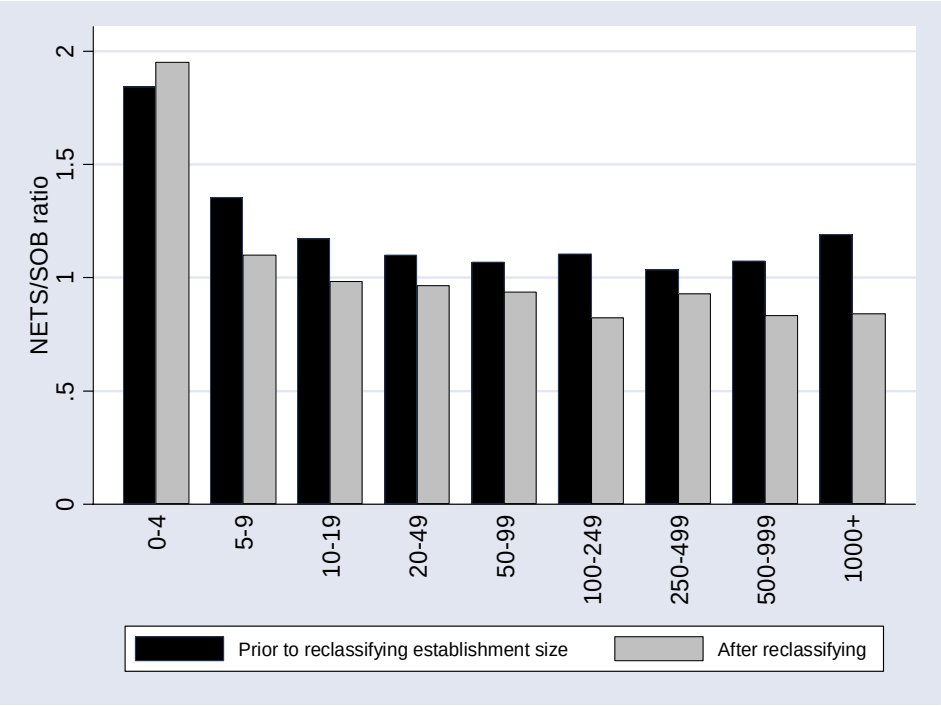


Figure 9: NETS and SOB Workers per Establishment

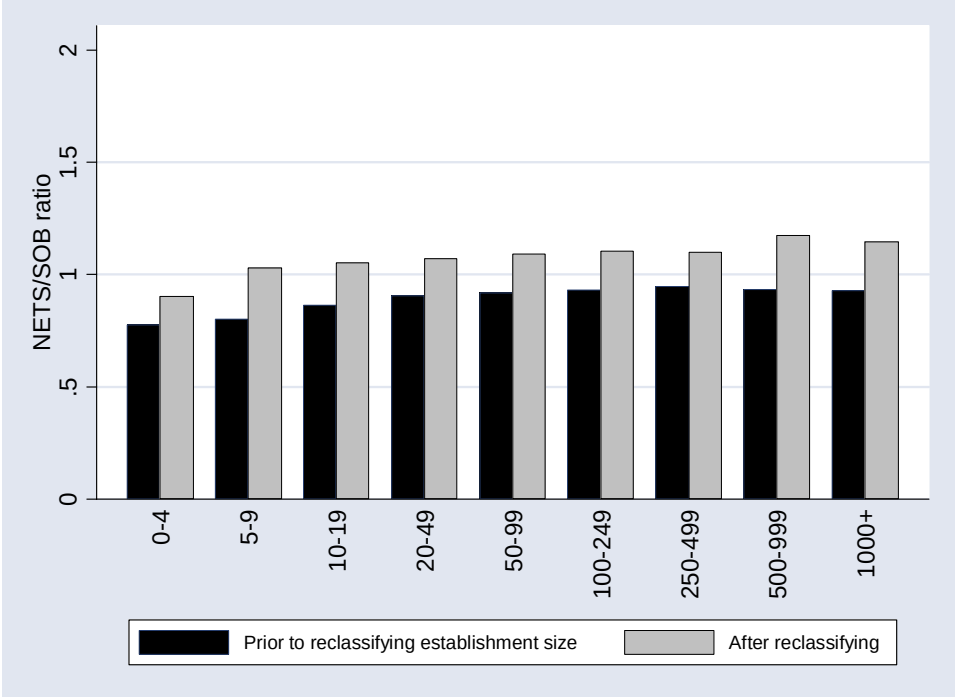


Figure 10: Histogram of NETS Establishment Sizes, 10-100 Employees (2000)

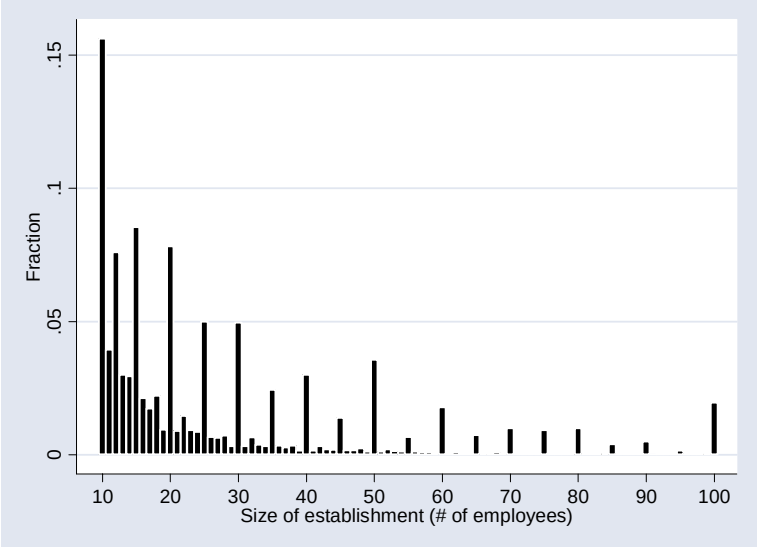


Figure 11: Histogram of NETS Establishment Sizes, 101-500 Employees (2000)

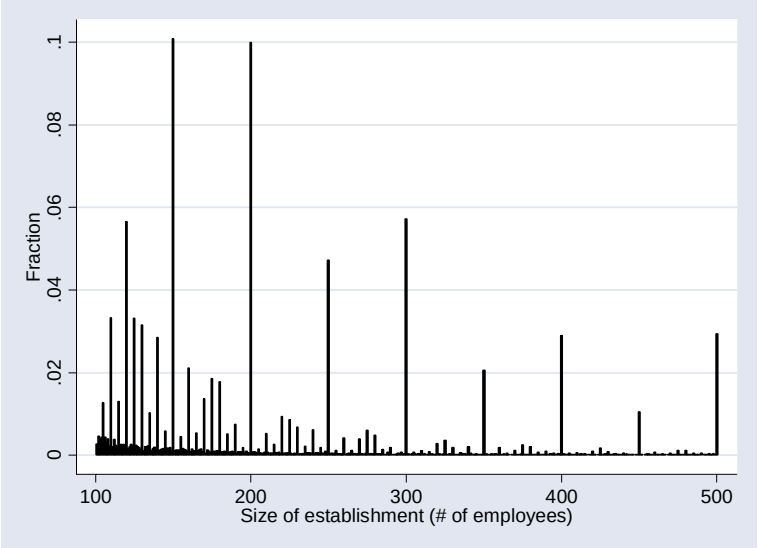


Figure 12: Distribution of Incidence of Actual Employment Data

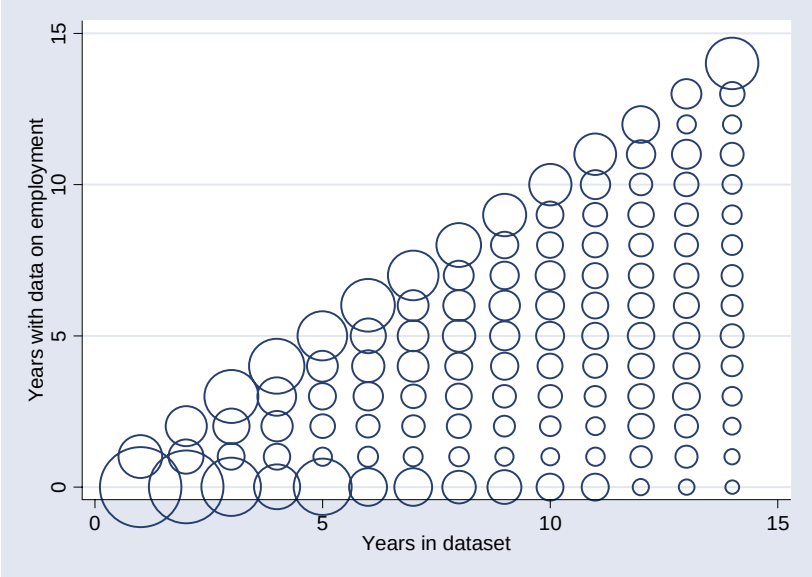


Figure 13: QCEW/NETS Employment Changes, by Industry and County, One-Year Changes (1997-1998, 1998-1999, 1999-2000)

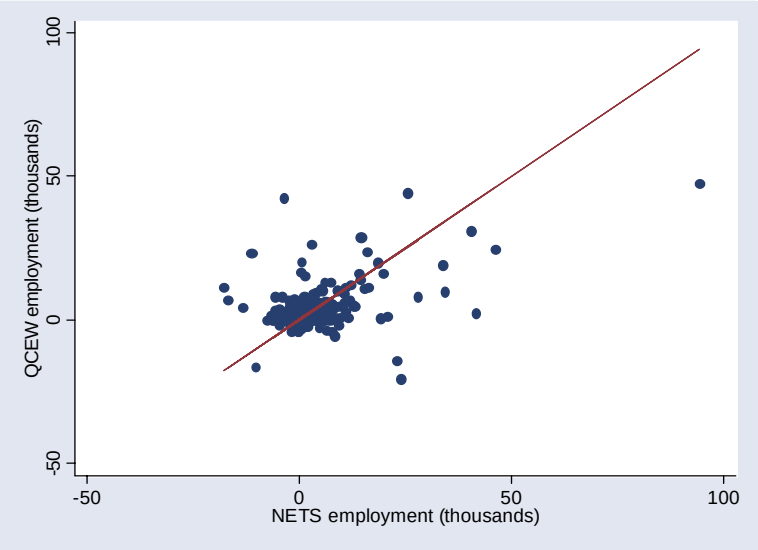


Figure 14: QCEW/NETS Employment Changes, by Industry and County, Three-Year Changes (1997-2000)

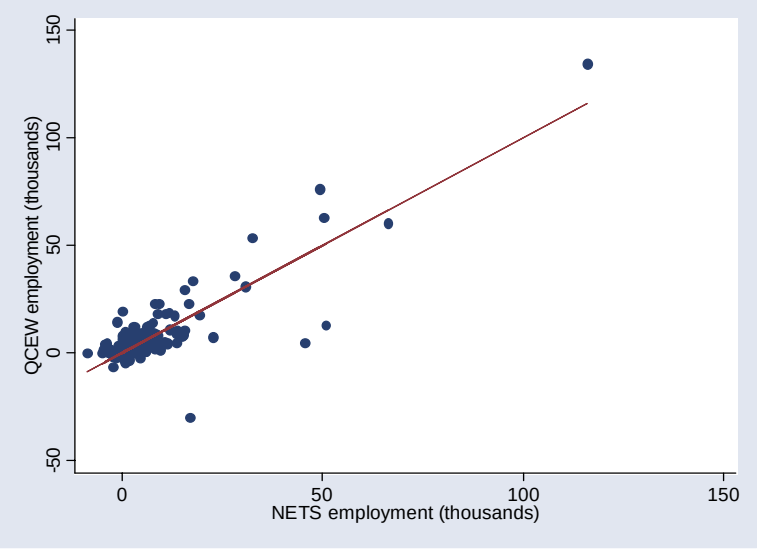


Figure 15: Establishment Openings by Year, NETS-Reported vs. Phonebook-Inferred

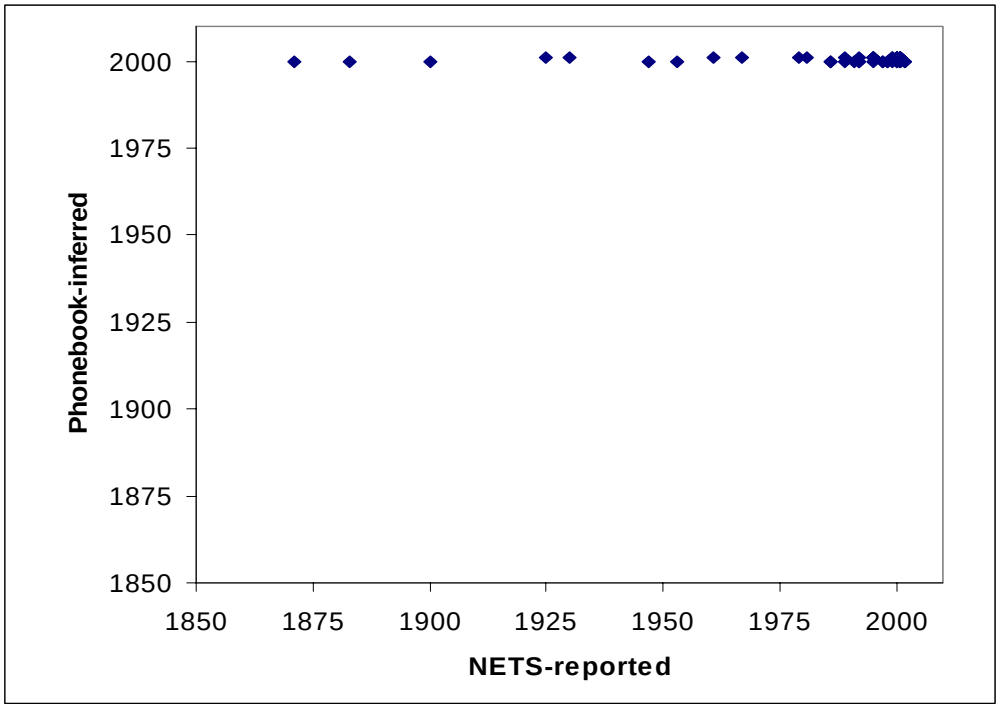


Figure 16: Establishment Openings by Year, Company-Reported vs. Phonebook-Inferred

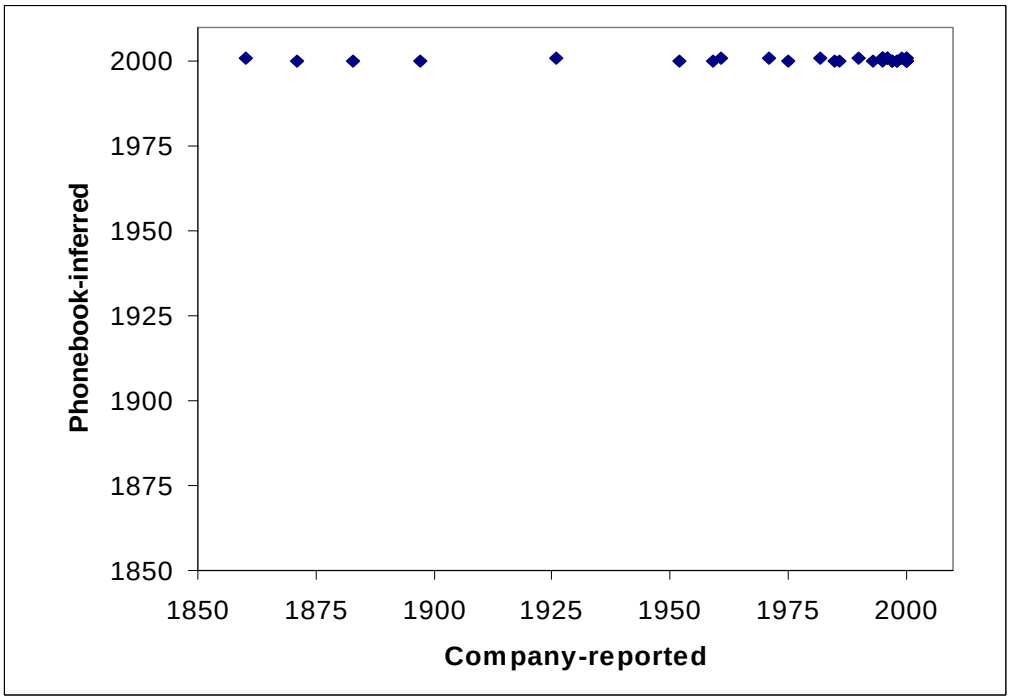


Figure covers establishments identified as opening in 2001 or 2002 from phonebooks, and identified in the NETS.

Figure 17: Establishment Openings by Year, Company-Reported vs. NETS-Reported

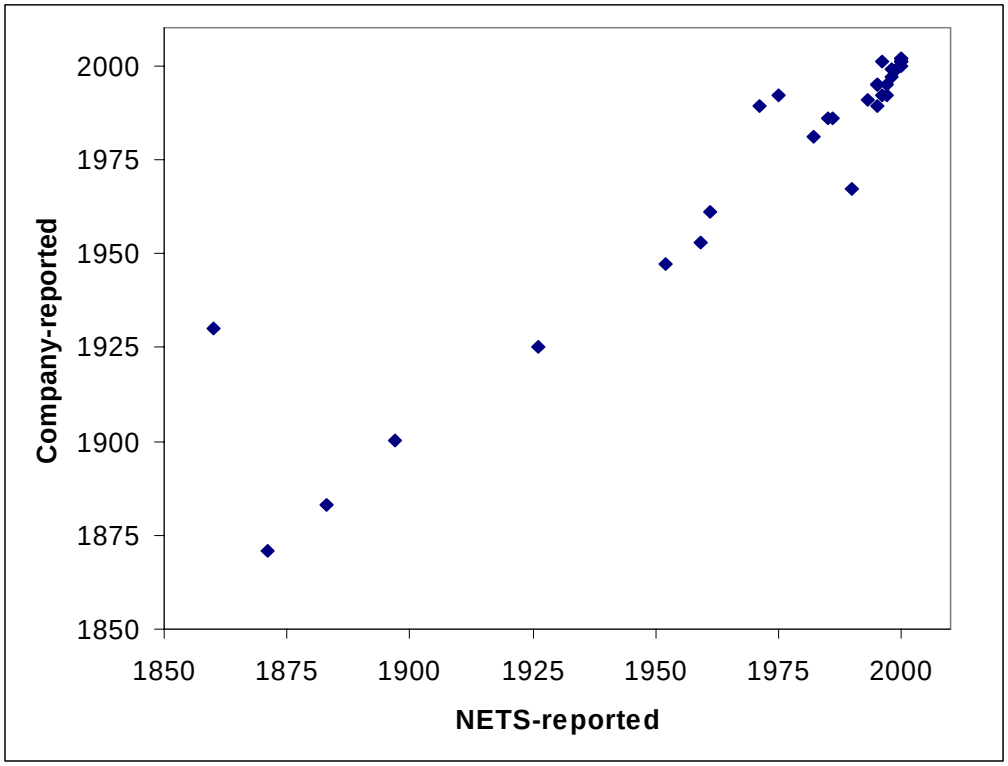
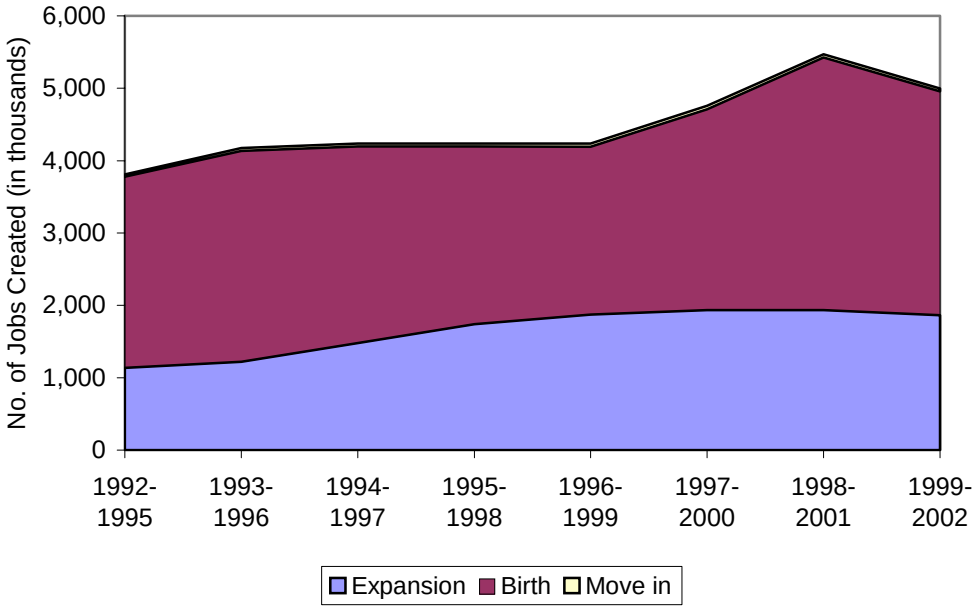
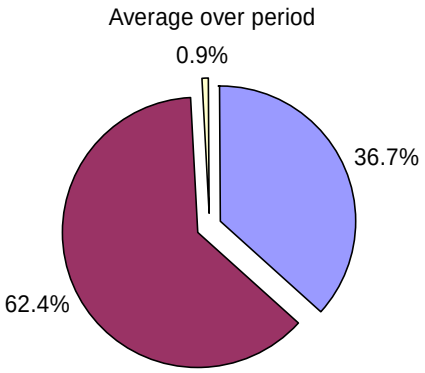


Figure covers establishments identified as opening in 2001 or 2002 from phonebooks, and identified in the NETS, as in Figure 15.

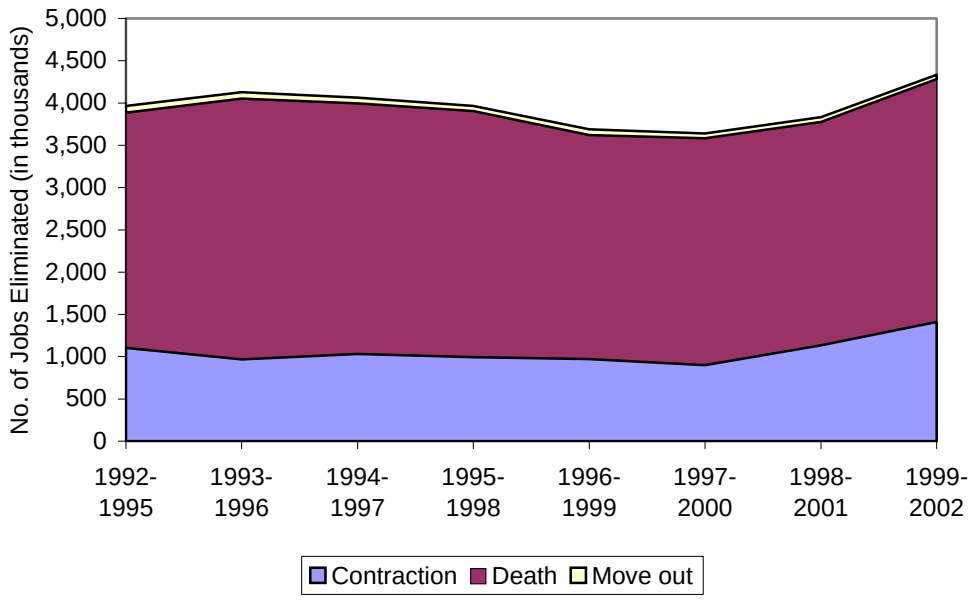
Figure 18: Job Creation and Destruction

(a) Sources of Job Creation





(b) Sources of Job Destruction



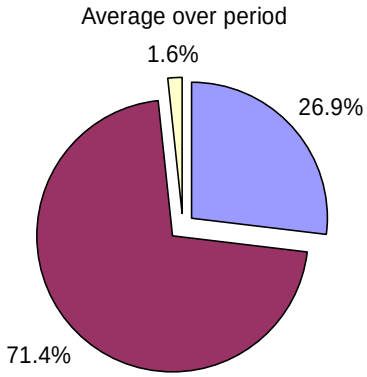


Figure 19: Business Dynamics and the Reallocation in the Labor Market

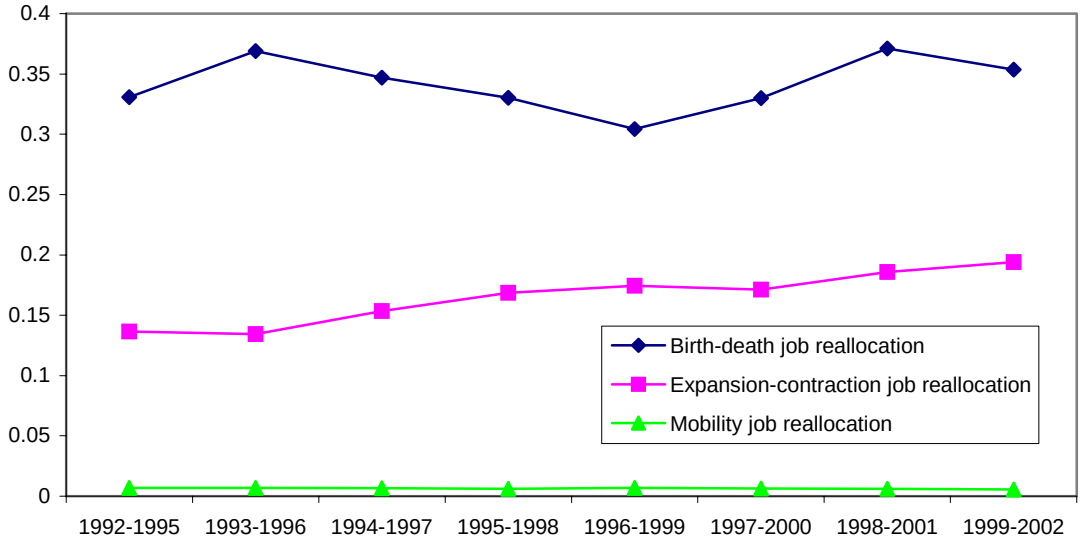


Table 1: Alternative Sources of Employment Data

Source	Years available in sample period	General description	Coverage
QCEW (ES-202)	1997-2003	Quarterly count of employment and earnings reported by employers covering 98 percent of U.S. jobs, available at the county, MSA, state and national levels by industry.	All workers covered by the California unemployment insurance laws and federal civilian workers covered by the Unemployment Compensation for Federal Employees (UCFE) program. Excluded: members of the armed forces; self-employed; proprietors; domestic workers; unpaid family workers; and railroad workers covered by the railroad insurance system.
CES	1983-2002	Monthly survey of over 400,000 business establishments.	Nonfarm payrolls
SOB	1994-2002	Same as QCEW	Same as QCEW
BED	1992-2002	Quarterly series of gross job gains and gross job losses statistics for the entire economy. Track changes in employment at the establishment level.	Same as QCEW

Table 2: NETS and SOB Employment (1994-2002)

	1994	1995	1996	1997	1998	1999	2000	2001	2002
NETS	16,371,012	16,241,156	16,314,659	16,546,553	16,512,479	16,864,781	17,666,262	18,149,748	17,527,918
Size of Business (SOB)[1]	12,696,157	13,047,314	13,312,913	13,739,592	14,257,229	14,642,495	15,144,896	14,997,165	14,967,297
Self-employed / independent contractor (SE/IC)[2]	2,084,696	2,093,767	2,008,958	2,083,693	1,851,667	1,893,306	1,877,283	1,899,806	1,895,814
Size of Business + self-employed / independent contractor (SOB+SE/IC)	14,780,853	15,141,081	15,321,871	15,823,285	16,108,896	16,535,801	17,022,179	16,896,971	16,863,111
Current Population Survey (CPS)	13,979,022	14,039,848	14,261,005	14,791,531	15,180,850	15,522,223	16,056,438	16,249,075	16,214,933
[NETS – (SOB+SE/IC)]	1,590,159	1,100,075	992,788	723,268	403,583	328,980	644,083	1,252,777	664,807
[CPS – (SOB+SE/IC)]	-801,831	-1,101,233	-1,060,866	-1,031,754	-928,046	-1,013,578	-965,741	-647,896	-648,178

[1] California Size of Business employment data includes individuals earnings that are covered by unemployment insurance for the pay period that includes September 12th, regardless of the type of payroll. The self-employed and independent contractors, as well as several other worker categories, are excluded from unemployment insurance coverage (California Unemployment Insurance Code, Chapter 3, Article 2, Section 656).

[2] The number of self-employed and independent contractors is calculated by multiplying the weighted proportion of individuals reported in these categories in the February Contingent Work Supplement (CWS) to the Current Population Survey by the annual average of household employment in California. The CWS was compiled in 1995, 1997, 1999, and 2001. In this table, the 1995 CWS is used to calculate the level of self-employment and independent contractors in 1994 and 1995; the 1997 CWS is used for 1996 and 1997; the 1999 CWS is used for 1998 and 1999; and the 2001 CWS is used for 2000, 2001, and 2002.

Table 3: Imputation of Employment Figures (1993-2002)

	Actual Figure	Bottom of Range	D&B Estimate	Walls Estimate
1993	52.23%	2.46%	13.29%	32.02%
1994	53.47%	2.40%	11.74%	32.39%
1995	54.01%	2.07%	11.52%	32.40%
1996	52.30%	1.64%	12.68%	33.37%
1997	55.57%	1.61%	11.20%	31.63%
1998	60.74%	1.25%	9.14%	28.88%
1999	66.78%	1.21%	7.01%	25.01%
2000	71.38%	1.26%	11.80%	15.56%
2001	69.30%	1.13%	21.87%	7.69%
2002	63.81%	0.95%	29.04%	6.21%

Table 4. Employment Imputation by Years in Database, % of All Establishments (1989-2002)

	Actual Figure	Bottom of Range	D&B Estimate	Walls Estimate
1	33.68%	1.05%	33.05%	32.22%
5	60.60%	2.29%	9.60%	27.51%
10	75.85%	1.85%	2.10%	20.19%
14	91.57%	1.92%	2.88%	3.63%

Table 5: Imputation Transition Matrix (1989-2002)

	Current year			
	Actual Figure	Bottom of Range	D&B Estimate	Walls Estimate
Previous year				
Actual Figure	95.82%	0.41%	0.01%	3.77%
Bottom of Range	15.64%	82.07%	0.00%	2.29%
D&B Estimate	0.60%	0.00%	94.51%	4.89%
Walls Estimate	23.34%	0.46%	1.19%	75.00%

Table 6: Share with Employment Changes, by Employment Imputation Type (1993-2002)

	Actual Figure	Bottom of Range	D&B Estimate	Walls Estimate
1993	11.43%	18.55%	0.02%	1.68%
1994	7.93%	11.66%	0.02%	2.00%
1995	11.22%	9.41%	0.02%	2.54%
1996	12.83%	10.61%	0.02%	3.32%
1997	13.00%	11.01%	0.01%	4.07%
1998	12.74%	8.63%	0.00%	4.17%
1999	12.43%	12.04%	0.01%	3.87%
2000	9.13%	10.49%	0.02%	1.82%
2001	9.78%	8.30%	11.52%	5.04%
2002	8.51%	7.17%	42.70%	0.01%

Table 7: Business Relocation and Its Effect on Employment in California, 1992-2002

A. By number of establishments					
	Moved in	Moved out	Net effect	Total number of establishments	Net loss as % of total
1993	612	1,364	-752	1,532,256	0.049%
1994	534	1,285	-751	1,515,142	0.050%
1995	519	1,104	-585	1,497,623	0.039%
1996	489	835	-346	1,521,247	0.023%
1997	504	763	-259	1,518,940	0.017%
1998	545	676	-131	1,492,105	0.009%
1999	582	669	-87	1,461,135	0.006%
2000	802	828	-26	1,519,325	0.002%
2001	752	1,032	-280	1,644,230	0.017%
2002	731	999	-268	1,814,938	0.015%
B. By number of jobs					
Year	Moved in	Moved out	Net effect	Total number of jobs	Net loss as % of total
1993	13,853	27,094	-13,241	16,266,713	0.081%
1994	8,977	25,452	-16,475	16,371,012	0.101%
1995	14,136	28,224	-14,088	16,241,156	0.087%
1996	13,158	18,352	-5,194	16,314,659	0.032%
1997	11,073	28,209	-17,136	16,546,553	0.104%
1998	15,098	16,709	-1,611	16,512,479	0.010%
1999	18,893	23,437	-4,544	16,864,781	0.027%
2000	15,589	16,994	-1,405	17,666,262	0.008%
2001	18,586	23,916	-5,330	18,149,748	0.029%
2002	12,656	16,551	-3,895	17,527,918	0.022%

Table 8: Top Destination and Origin States of Establishments that Crossed State Boundaries, 1993-2002

Top destination states of establishments that moved out of California			Top origin states of establishments that moved into California		
	Number of establishments	Number of jobs		Number of establishments	Number of jobs
Nevada	1,305	14,086	New York	613	16,304
Arizona	830	16,544	Nevada	438	5,189
Texas	815	34,819	Texas	437	9,713
Oregon	645	6,103	Arizona	416	6,095
Washington	566	7,986	Washington	385	5,902
Colorado	551	11,546	New Jersey	349	9,794
Florida	507	10,209	Illinois	291	9,014
New York	466	16,387	Colorado	280	4,566
Illinois	269	8,700	Florida	272	4,341
Utah	253	3,345	Oregon	257	2,417
Georgia	227	5,799	Massachusetts	215	6,441
Idaho	222	3,045	Pennsylvania	146	3,795
New Jersey	196	5,546	Virginia	137	3,859
North Carolina	182	19,162	Ohio	136	4,980
Massachusetts	180	5,162	Michigan	134	2,663
Virginia	175	4,851	Utah	126	2,118
Pennsylvania	166	7,172	Georgia	125	4,195
Ohio	157	7,887	Maryland	104	4,258
Tennessee	140	3,610	Hawaii	97	1,297
New Mexico	135	974	Connecticut	96	5,148

Rankings are based on number of establishments moving.

Table 9: Decomposition of Employment Growth in California

	1992-1995	1993-1996	1994-1997	1995-1998	1996-1999	1997-2000	1998-2001	1999-2002
A. Employment change								
Starting employment	16,394,151	16,266,713	16,371,012	16,241,156	16,314,659	16,546,553	16,512,479	16,864,781
Ending employment	16,241,156	16,314,659	16,546,553	16,512,479	16,864,781	17,666,262	18,149,748	17,527,918
Change	152,995	-47,946	-175,541	-271,323	-550,122	-1,119,709	-1,637,269	-663,137
Job creation:								
Expansion	1,134,603	1,220,681	1,480,284	1,742,557	1,874,193	1,933,519	1,934,525	1,862,952
Birth	2,641,169	2,915,369	2,716,969	2,456,024	2,317,230	2,776,719	3,488,940	3,092,281
Move in	34,327	37,993	41,994	37,355	46,076	49,515	45,268	42,277
Job destruction:								
Contraction	1,102,839	965,717	1,030,221	994,987	973,018	901,333	1,134,032	1,410,608
Death	2,781,915	3,086,093	2,965,193	2,909,694	2,648,325	2,682,980	2,640,929	2,870,695
Move out	78,340	74,287	68,292	59,932	66,034	55,731	56,503	53,070
B. Employment change decomposition								
Employment change =	-152,995	47,946	175,541	271,323	550,122	1,119,709	1,637,269	663,137
(expansion-contraction)	31,764	254,964	450,063	747,570	901,175	1,032,186	800,493	452,344
+ (birth-death)	-140,746	-170,724	-248,224	-453,670	-331,095	93,739	848,011	221,586
+ (move in-move out)	-44,013	-36,294	-26,298	-22,577	-19,958	-6,216	-11,235	-10,793

Table 10: Employment Change Decomposition (1992-2002), Various Interval Lengths of Observation

	1 year	2 years	3 years	5 years	10 years
Expansion	6,726,170	6,014,067	4,811,685	4,357,686	2,715,820
Birth	10,228,529	9,694,187	8,586,133	8,515,039	7,333,856
In-migration	142,019	139,145	116,950	127,460	111,104
Gross creation	17,096,718	15,847,399	13,514,768	13,000,185	10,160,780
Contraction	5,131,936	4,374,271	3,231,858	2,999,383	1,865,002
Death	10,606,077	10,116,790	8,332,538	8,671,637	6,991,783
Out-migration	224,938	222,571	194,775	195,398	170,228
Gross destruction	15,962,951	14,713,632	11,759,171	11,866,418	9,027,013
Net change	1,133,767	1,133,767	1,755,597	1,133,767	1,133,767

For 3-year intervals the analysis is limited to 1992-2001, which can be divided into period of 3 years length.

Table 11: Employment Change Share Decomposition (1992-2002), Various Interval Lengths of Observation

	1 year	2 years	3 years	5 years	10 years
Expansion	39.3%	37.9%	35.6%	33.5%	26.7%
Birth	59.8%	61.2%	63.5%	65.5%	72.2%
In-migration	0.8%	0.9%	0.9%	1.0%	1.1%
Gross creation	17,096,718	15,847,399	13,514,768	13,000,185	10,160,780
Contraction	32.1%	29.7%	27.5%	25.3%	20.7%
Death	66.4%	68.8%	70.9%	73.1%	77.5%
Out-migration	1.4%	1.5%	1.7%	1.6%	1.9%
Gross destruction	15,962,951	14,713,632	11,759,171	11,866,418	9,027,013
Net change	1,133,767	1,133,767	1,755,597	1,133,767	1,133,767

See note to Table 10.

Table 12: Comparisons Across Datasets

	BED, 1994-2002[1]	LDB, Sept.-Dec. 1999[2]	ES-202, WV, 1990-1994[3]	NETS, CA, 1992-2002	ES-202, WV, 1990-1994[3]	NETS, CA, 1992-2002	ES-202, WV, 1990-1994[3]	NETS, CA, 1992-2001	ES-202, WV, 1990-1994[3]
	Quarterly	Quarterly	Quarterly	1 year	1 year	2 years	2 years	3 years	3 years
Share of job creation									
Expansion	77.9%	79.8%	80.4%	39.3%	60.2%	37.9%	51.1%	35.6%	44.2%
Birth	22.1%	20.2%	19.6%	59.8%	39.8%	61.2%	48.9%	63.5%	55.8%
In-migration	N.A.	N.A.	N.A.	0.8%	N.A.	0.9%	N.A.	0.9%	N.A.
Share of job destruction									
Contraction	78.6%	78.6%	81.4%	32.1%	59.4%	29.7%	47.3%	27.5%	39.8%
Death	21.4%	21.3%	18.6%	66.4%	40.6%	68.8%	52.6%	70.9%	60.2%
Out-migration	N.A.	N.A.	N.A.	1.4%	N.A.	1.5%	N.A.	1.7%	N.A.

[1] Calculated from www.bls.gov/bdm/, viewed May 3, 2005, seasonally-adjusted series.
[2] Pivetz, et al. (2001).
[3] From Spletzer (2000, Tables 4 and 5).

Appendix A: Newspaper Article Search

The number of articles – both related and unrelated – that are returned by a full-text or index term search depend on the choice of specific search term(s). Of the articles returned by such keyword searches, typically only 20-50% of articles can be identified as pertaining to actual establishment relocations. Other articles typically either discuss the phenomenon of business relocations (without giving information of specific moves) or discuss residential relocation. Certain combinations of words are more effective at returning a high yield of actual business relocations.

We experimented with several different search terms. For the purpose of testing the effectiveness of different search strings we limited our testing to articles appearing in the 1996 Los Angeles Times. Using the Lexis-Nexis database, the search term "RELOCAT!" returns 1,218 articles. This term searches for any word that starts with those characters, which are limited almost exclusively to the words relocate, relocated, relocating, or relocation. Adding the terms "BUSINESS" and "MOVE OR MOVING OR MOVED",[59] and that the term "RELOCAT!" is found at least twice in each article, further improves targeting. Finally, requiring that articles be published in either the metro or business sections substantially reduces the number of irrelevant articles, while excluding few relevant articles. These refinements reduce the number of articles found by 84% while improving the likelihood of locating relevant articles from 19% to 46%. Any further specificity of search term seemed to exclude many relevant articles, with little improvement in targeting.

Table A1 summarizes all the strings of search terms with which we experimented, and Table A2 summarizes some results. The last one (search 5) is the criterion we used to find reported cases of business relocation in the Los Angeles Times during 1996-2000.

When identifying establishment relocation in these articles we use a similar definition to that used in the NETS database. We only include business establishment relocation, and exclude residential, event, or sports franchise relocations. A valid reporting of establishment relocation must give the name of a specific firm and the intended destination, though the citation need not include the origin of the move.

[59] "MOV!" was not selected as a search term because the word "movie" is quite common.

We are interested only in the relocation of actual establishments between two physical locations, and ignore reports of the relocation of jobs, divisions, or sections between establishments within the same firm that are also often reported in the news media. Finally, the newspaper article may not indicate that the origin office (or location) will remain open, or that the destination office existed prior to the move.

A sophisticated search string was not necessary for identifying business relocations reported in the Kiplinger California Letter because the publication has a specific section about business relocation.

Table A1: Search Strings

Search 1	RELOCAT!
Search 2	RELOCAT! AND BUSINESS
Search 3	RELOCAT! AND BUSINESS AND (MOVE OR MOVING OR MOVED)
Search 4	AT2(RELOCAT!) AND BUSINESS AND (MOVE OR MOVING OR MOVED)
Search 5	ATL2(RELOCAT!) AND BUSINESS AND (MOVE OR MOVING OR MOVED) AND (SECTION("BUSINESS") OR SECTION("METRO"))

Table A2: Articles Found Using Different Search Strings in the 1996 Los Angeles Times

	Search 1	Search 2	Search 3	Search 4	Search 5
Total articles	1,218	641	460	244	197
Valid articles in first 100 articles of year	19	21	24	41	46