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Notes

Chapter 1

1. Throughout this book, designations used to refer to the various regions of the United States carry their modern denotations, with slight amendments that reflect the political realities of the period (e.g., *Virginia* includes current-day West Virginia) or as noted.

Chapter 2

1. The survey is meant, not to be exhaustive, but rather to discuss the major studies.
2. A rise in the real daily (or monthly) real wage may not imply a rise in the *annual* standard of living, if annual days of work decline sufficiently and the decline is involuntary (i.e., unemployment, see chap. 7) or if the rise in the wage compensates for more adverse working conditions (see chap. 3). If neither condition obtains, however, real consumption can increase and the worker is better off.
3. The manuscript 1850 Census of Manufactures and the manuscript and published 1860 Census of Manufactures provide additional wage evidence for manufacturing; these can be combined with the 1820 census and the McLane Report to produce estimates of average annual wages in manufacturing for four years (see Sokoloff and Villaflor 1992). Because of geographic limitations in the census, the Sokoloff-Villaflor series cover only the Northeast.
4. The absence of firm weights led Abbott (1905) and Hansen (1925) to choose the Aldrich over the Weeks Report. Lebergott (1964), however, made a persuasive case that the Weeks Report was superior to the Aldrich Report; its coverage was better, it was less affected by sampling variability, and the weighting problem could be solved by using the census figures as benchmarks.
5. For a recent study of cyclic fluctuations in wages in the late nineteenth century using the Aldrich Report, see Hanes (1993).
6. For example, if one is interested in using the Weeks Report to measure changes in the wages of common labor before 1840, there is only *one* usable observation for the period 1830–32.

7. Coelho and Shepherd present no other occupation-specific estimates at the regional level, apparently because the regional sample sizes were too small (see Coelho and Shepherd 1976, 217, n. 31). They do, however, aggregate the entire sample to the national level and produce money wage series for the other four occupations (see their app. table A2).

8. If the regional price deflators are used, however, the decline is larger in the Northeast (in percentage terms) than in the Midwest (see Coelho and Shepherd 1976, table 4, p. 213).

9. The conclusion that the real wages of engineers in the Mid-Atlantic and East North Central regions rose in the 1850s is not very robust. In the Mid-Atlantic case, there was no upward trend in the real wages of engineers between 1851 and 1859 and then an 11 percent increase between 1859 and 1860. In the East North Central states, there was supposedly a 30 percent increase in the real wages of engineers between 1851 and 1852 but no change between 1852 and 1860. Given the small number of engineers in the Weeks Report sample for the 1850s (there are only eighteen observations for the entire country in 1851 and nineteen in 1852), Coelho and Shepherd's (1976, 217, n. 31) caution that the skilled occupation groups in the Weeks data "may not be as homogenous as they appear at first glance," and the absence of an upward trend in the 1850s, there is every reason to be skeptical that a real wage increase occurred.

10. Using an unweighted average of the New England and Mid-Atlantic decadal average real wages as the base, the regional gap in real wages between the East North Central states and the Northeast was 8 percent for common laborers and 8.5 percent for engineers (calculated from Coelho and Shepherd 1976, tables 6–7, pp. 218–19).

11. The same conclusion is affirmed in an analysis-of-variance-type regression of the full sample (the regression controls for occupation and region) that, unfortunately, does not allow skill differentials to vary across regions (i.e., interactions between the occupation and the regional dummies) (see Coelho and Shepherd 1976, table 8, p. 220). In the case of engineers and common laborers, the Weeks Report data suggest that skill differentials were similar in the Northeast and the East North Central states in the 1850s but were substantially higher in the South Central states in the late 1850s; on the basis of decadal averages, the ratio of engineers' to laborers' pay was 1.69 in the 1850s in the Northeast and the East Central states but 2.28 in the East South Central states (the figure for the East South Central states pertains to 1857–60 only) (see Coelho and Shepherd 1976, tables 6–7, pp. 218–19).

12. Scattered quotations are available for other types of workers, including engineers, painters, and cooks (Smith 1963, 301), but Smith makes no use of these in his analysis.

13. Smith (1963, 299, 301) noted that he was able to produce monthly wage series for three geographic sections of the canal, but, unfortunately, the published article reports only annual series for the entire canal.

14. If the base year is 1828 instead of 1830, real wages for both common laborers and carpenters increased in the 1830s.

15. Lebergott (1964, 150) provided "best guesses" at real wage movements over medium-length periods (such as 1835–50). Like Coelho and Shepherd (1976), Lebergott (1964, p. 150) also argued that real wages fell in the 1850s.

16. The exact nature of the disagreement between Zabler and Adams is somewhat unclear, at least as far as carpenters are concerned. The ratio of the pay of house carpenters to that of common laborers averaged 1.25 in Philadelphia in the 1820s; as noted in the text, the corresponding ratio for the iron industry was 1.22.

17. Had the cost of living been the same in Philadelphia and rural eastern Penn-

sylvania, a Philadelphia carpenter would have had to work only 46 percent of the year (= 0.57/1.25), or roughly 141 days (based on a 310-day workyear). Had the cost of living been 50 percent higher in Philadelphia, annual earnings would have been equalized by working 68 percent of the year or an annual unemployment rate of 32 percent.

18. Unfortunately, there is no wage quotation for common labor in 1820, but a decline in the money wage for common labor is evident comparing the late 1810s to the early 1820s (from $1.00 to $0.75 per day).

19. Adams also argues (1968, 413) that the use of wholesale instead of retail prices exacerbates annual fluctuations in real wages, but he provides no evidence supporting this position.

20. For example, the growth rate of real wages for ships' carpenters was 4.6 percent per year from 1821 to 1830 but only 1.6 percent per year from 1820 to 1830.

21. However, unemployment was high in the early 1840s, consistent with Adams's argument. Also, suppose the relative price of goods other than food and shelter declined in the 1850s to a degree sufficient to offset the rise in the budget share for food and shelter. Then labor welfare could be higher even though leisure fell.

22. To explain the absence of real wage growth, Adams (1986, 637) speculates that the supply curve of labor to the farm sector in Maryland may have been close to perfectly elastic during the period. Even if this were true, however, it does not imply an absence of real wage growth because the supply price to agriculture would depend (as Adams recognizes) on the opportunity cost of labor in the nonfarm sector. Thus, if real wages were rising outside Maryland agriculture, the supply price to the farm sector would also rise, assuming that the two labor markets were integrated.

23. Taking 1820–29 as the base decade (= 100), the index number for w/p (the real wage) for 1850–55 was 100.8.

24. Sokoloff (1986a, 23) reaches a similar conclusion; he deflates Rothenberg's nominal wage series by the David-Solar price index, thereby producing real wage gains from the 1820s to the 1850s.

25. The basis for this belief is unclear since, as David and Solar (1977, 61) point out, there is "considerable obscurity surround[ing] the number and distribution of establishments" in the Massachusetts data.

26. As noted previously, Lebergott's figures are weighted averages of the state estimates published in the 1850 and 1860 volumes; the weights are the census counts of common laborers in each state.

27. Note that, by construction, the nominal wage appears in both the numerator and the denominator of the David-Solar real wage index, which may dampen its volatility. This effect is probably not large since housing costs are assumed to be roughly 15 percent of the total household budget.

28. Hoover (1960) constructed a retail price index for the period 1851–80 using retail prices from the Weeks Report.

29. Williamson and Lindert did not actually report a separate index for skilled labor; rather, they report a series of skill differentials (the ratio of skilled to unskilled wages). The relevant series is given as "urban skilled workers" in app. D of Williamson and Lindert (1980, 307).

30. Williamson (1975, 36) does argue, however, that nominal wage gaps between farm and nonfarm common labor declined from the 1810s to the 1830s in Massachusetts and thus that his series overstates the growth in unskilled pay prior to 1830.

31. There is some confusion between Williamson (1975) and Williamson and Lindert (1980) over the use of Layer's series. According to the notes to table 9 in Williamson (1975, 37), Layer's series was used for the period 1834–49, Layer being

"preferred to Abbott . . . since the Aldrich sample is too small and inconsistent prior to 1850." According to the text of Williamson (1975, 36) and the notes to app. G of Williamson and Lindert (1980, 320), Layer's series was used only through 1839. In what follows, I assume that Williamson and Lindert (1980) is the authoritative discussion.

32. The growth rate is calculated from decadal averages of index numbers of real wages for 1820–29 and 1850–59 (Williamson and Lindert 1980, app. G, p. 319). The ratio of the 1850s index number to the 1820s index number was 1.576 (= 89.1/56.5). Taking logarithms, and dividing by 30 (the number of years between 1825 and 1855, the midpoints of the respective decades), gives a growth rate of the skill differential of 1.52 percent per year.

33. According to Brady, the cost of living rose by about 9 percent from 1834 to 1836, compared with an approximately 30 percent increase in the wholesale price level (see Margo 1992, 189).

34. If one uses Brady's (1966) data, substitutes wholesale prices for coffee and tea, and excludes clothing items with extremely steep price declines, the revised David-Solar price index shows an increase in the cost of living between 1834 and 1836 of about 18 percent. As pointed out in chap. 3, the David-Solar real wage index shows too high a level in the late 1830s because the numerator is based on data from an unusually high wage location. Part of the reason, therefore, why the David-Solar real wage index registers a decline in the early 1840s is a consequence of the biased level in the late 1830s. However, the price deflator is also responsible, as evinced in table 2.1 above. Price indices based on wholesale prices, such as Williamson and Lindert's (1980) or those developed in chap. 3, register dramatic declines in the early 1840s, in excess of declines in nominal wages, thereby causing real wages to rise. In chap. 7, I argue that nominal wages were rigid in the short run before the Civil War, partly on the basis of the episode of the early 1840s (see also Goldin and Margo 1992b). If retail prices declined less than wholesale prices—and, in particular, according to the pattern revealed by the David-Solar price index—then real wages might conceivably have fallen. However, as noted in the text (see also chaps. 3 and 7), there are good a priori reasons to doubt that wholesale prices, particularly for basic food items like flour, were substantially more variable than retail prices.

35. Weiss's (1992, 26) variant A estimate of per capita income grows at an average annual rate of 1.21 percent per year between 1820 and 1860. Using Weiss's figures on the total labor force, the aggregate labor force participation rate increased from 0.328 in 1820 to 0.358 in 1860, or an average annual rate of growth of 0.22 percent per year. Thus, output per worker grew at 0.99 percent per year, considerably slower than the growth rates implied by the real wage series.

36. For an excellent general history of the Quartermaster's Department, see Risch (1962).

37. The ability of post quartermasters to hire civilians varied over time. Normally, it would appear, the hiring of civilians had to be approved at some level, even if directly authorized by the post's commanding officer. General Order no. 43, issued in 1851, made obtaining such authority much more difficult; it forbade the hiring of civilians for any purpose except as authorized by army division headquarters (Prucha 1953, 168). Although the number of civilians hired in the 1850s clearly fell in certain regions (see, e.g., table 2.2 below in the case of forts in the South Atlantic region), it did not in others.

38. By *gaps,* I mean that not every type of worker (e.g., teamsters) was hired at every post in every month. The idiosyncratic nature of the army's demand for civilian labor (e.g., no carpenter could be found among the troops) is the most likely explanation for the gaps, although it is certainly possible that some civilians were hired but that no administrative record survives.

39. For an exhaustive survey of the available records, see Heppner and John (1968).

40. Workers at arsenals were frequently paid by the piece, and output seems to have been recorded on a daily basis. Hence, the records are voluminous and difficult to use. The naval records are similar to the *Reports* but tend to be more voluminous, and the locations tend to more or less duplicate those from the *Reports*.

41. Payrolls were also collected from forts in Florida between 1835 and 1842, forts in Texas during the Mexican War, and forts in the Far West. With the exception of forts in California (see chap. 6), data from these forts are not used in this book.

42. The extract from the full sample used in chap. 3 differs in that certain forts and wage outliers are excluded and attention restricted to particular occupation groups, but the broad distributional patterns are the same.

43. This number, however, overstates the amount of independent information in the sample because some workers were hired for longer periods of time than a month and therefore appear more than once in the sample.

44. The temporal detail in the table understates the degree to which certain time periods are under- or overrepresented; when the data are disaggregated by year, some years are severely underrepresented. As a result, it is necessary in some cases to pool data across years, which means that some of the resulting wage estimates must be interpolated (see chap. 3).

45. The unusually large number of observations for Kansas pertains only to the 1850s.

46. For example, Smith (1963) provided series for only common laborers, carpenters, masons, and teamworkers.

47. Of free males aged fifteen and over reporting nonfarm occupations in the 1850 census, fully 43.2 percent were either carpenters, clerks, laborers, masons and plasters, painters and glaziers, or teamsters (computed from DeBow 1854, 126–28).

48. For an analysis of the data on agricultural yields in 1860, see Schaefer (1983).

49. The specific instructions for collecting the wage data stated nothing more than that "the information called for in the six columns is so simple and so plainly set forth in the headings that it is deemed unnecessary to add thereto" (DeBow 1853, xxv).

50. Such activities can be inferred from the occupation information in the *Reports*. For example, a white-collar worker might be designated as a "clerk in the quartermaster's department" or an "assistant to quartermaster"; since the post quartermasters were involved in record keeping and so forth, it seems reasonable to infer that their civilian employees assisted them in such activities.

51. It is clear that post quartermasters had to compete with private-sector employers when they attempted to hire civilians (see, e.g., Prucha 1953, 166). Ruhlen (1964, 19) cites a report describing the construction of an early Nevada fort: "At the time the post was located a great competition for mechanics, materials, &c was created by the settlement and building of the several cities of Carson, Silver, and Virginia."

Chapter 3

1. For example, a tight specification would allow wages to vary over time according to a linear time trend rather than dummy variables for each year (see the text). The coefficient of the time trend is an estimate of the average annual rate of growth. This growth rate may be estimated accurately but at the cost of ignoring annual movements in wages around the trend.

2. It is important to note that this is a pooled time-series cross-sectional regression as opposed to a panel regression. In a panel setup, observations—or, rather, individuals—would be followed through time. To follow individuals through time, it would be necessary to link them across years using some identifying feature, such as a name. Although names were sometimes included on the payrolls, the reporting of names was irregular enough (especially for common laborers) that panel estimation proved infeasible.

3. In logs, the value of X^* in period j is $X^*\beta + \delta_j$, while the value in period k is $X^*\beta + \delta_k$. Thus, the relative value of the bundle in the two periods is $(X^*\beta + \delta_j) - (X^*\beta + \delta_k) = \delta_j - \delta_k$, which does not depend on X or β, as stated in the text.

4. Twenty-six days is the standard figure used in such conversions (see Lebergott 1964, 245).

5. Compared with modern-day wage regressions, the list of X variables is very small; in particular, it does not control for work experience, which, on the basis of modern evidence, might be expected to have an effect on wages. The assumption in constructing the wage series is that the distribution of *unobserved* characteristics does not vary over time. If it does—e.g., if workers at the forts were, on average, more experienced in the 1850s than in the 1820s and wages were a positive function of experience—then the growth rates of the series would be biased upward. To get at this issue, it would be necessary to trace observations through time or else link the payrolls to another source that would provide proxies for unobserved characteristics, such as the manuscript Censuses of Population of 1850 or 1860. However, any such linked sample would be small in size and could not be used to construct annual wage series.

6. Regressions for the South were also estimated excluding slaves; however, to produce nominal wage series from these regressions requires much more interpolation (see the text regarding the procedure to produce the nominal wage series) than if slaves are included. Because almost all slave artisans at South Central forts were employed in the 1830s, it was not possible to allow the coefficient of slave status in this regression to vary across decades.

7. Fishback and Kantor estimate the wage premium for unemployment risk in a hedonic regression. Their regression also includes dummy variables for pay periods (whether the worker was paid weekly or monthly). As argued in the text (see also Fishback and Kantor 1992, 832), the coefficients on these dummy variables also capture unemployment risk. Fishback and Kantor's conclusion, however, that workers were less than fully compensated for unemployment risk is based on coefficients of days lost in the hedonic regression, *controlling* for the pay period; thus, they may be underestimating the degree to which workers were compensated for unemployment risk.

8. Lebergott (1964, 245–50) provides evidence on the day wage premium for a variety of occupations. In agriculture, the premium stood at about 46 percent in 1869, drifting down to 29 percent at the turn of the century. In Pennsylvania ironworks in the early 1830s, twenty-six times the day wage was 36 percent higher than the average monthly wage. Labor contracts for seamen provide some of the most revealing evidence; Lebergott shows that monthly wages were significantly lower on longer-distance voyages—until the replacement of steam for sail, which cut the length of voyages significantly.

9. In such cases, rations were valued at \$0.12 or \$0.20, depending on internal evidence in the reports (see Margo and Villaflor 1987, 878).

10. Alternatively, slaves may have been more costly to supervise than free labor and may also have been less likely to bring tools to the job, and both these factors would be reflected in a lower wage.

11. Choosing an X^* is not necessary if one wishes merely to calculate a nominal

wage index; such an index can be computed directly from the coefficients of the time-period dummies because the dependent variable is measured in logarithms (see Margo 1992). However, if one wishes to produce a nominal wage *series* (i.e., dollar values), it is necessary to choose a value of X^*.

12. The fort location weights are similar to those in Margo (1992).

13. It is worth noting that the nominal wage indices can be computed from the nominal wage series simply by dividing each wage by the appropriate 1850 benchmark.

14. These data can be found in "Naval Hospital Payrolls," Bureau of Yards and Docks, Record Group 71, National Archives. It is important to note that these workers were building hospitals and other buildings at the yard, *not* ships (ship carpenters earned a premium above ordinary carpenters).

15. The coefficient estimates are as follows: 1835: skilled, −0.236; 1836: skilled, −0.167; unskilled, −0.206; 1837: skilled, −0.218.

16. If Pittsburgh observations are allocated to the Midwest, the nominal wages of white-collar workers in the Midwest grew at an average annual rate of 1.77 percent (based on the decadal averages for the 1820s and 1850s), compared with 1.24 percent per year if Pittsburgh observations are allocated to the Northeast. If the Pittsburgh observations are excluded from the Northeast, the nominal wages of white-collar workers in the Northeast increase at an average annual rate of 1.18 percent per year (based on the decadal averages for the 1820s and 1850s), compared with an average annual rate of 1.43 percent per year if Pittsburgh observations are included in the Northeastern samples. The upshot is that white-collar wages in Pittsburgh increased comparatively rapidly (relative to other locations in the Northeast) and thus that their inclusion (exclusion) raises (lowers) the regional growth rate.

17. For details on the construction of the price indices for 1821–56, see the appendix to Goldin and Margo (1992b). Extending the original Goldin-Margo price indices to 1860 followed the procedures described in the Goldin and Margo paper. Some slight additional modifications were made to trend in the price indices for the 1850s, making use of the 1850 and 1860 census data on the weekly cost of board.

18. In this respect, they are similar to the price deflators employed in various studies of antebellum real wages, such as Williamson and Lindert (1980; see also chap. 2 below).

19. There is also the problem of properly pricing the services provided by goods that are affected by technological progress, a problem that I do not address. Nordhaus (1997) argues that conventional real wage series, such as those developed in this book, systematically understate growth rates in living standards by failing to account for the value of such services properly.

20. Lebergott (1964, 548) reported price index numbers for 1830, 1840, 1850, and 1860. I substituted the good-specific retail price change between 1830 and 1860 for the decadal average of the respective subindex in my Northeastern price deflator. Thus, e.g., Lebergott's calculation shows a decline in the retail price of textiles of 26 percent (from an index number of 100 in 1830 to an index number of 74 in 1860), whereas the cotton subindex of my price deflator declines by 12.5 percent from the 1830s to the 1850s. I do not substitute Lebergott's price index for coffee since this shows a rise over the period, compared to a decline in the wholesale price. It is important to note that, had I averaged Lebergott's figures between successive census years (e.g., 1830 and 1840) to produce decadal midpoints and used the midpoints instead to infer the bias, the bias would have been smaller.

21. As noted in chap. 2, the Hoover housing price index is problematic because it is based on company housing (see Lebergott 1964).

Chapter 4

1. The proof is very simple. Suppose that aggregate labor supply is fixed at $\underline{L}$. Efficiency requires that production be at a point on the production possibilities frontier. In the two-sector model in the text, the value of output is

$$pF(L_A, T) + G(\underline{L} - L_A, K),$$

where F is a production function for agriculture, G is a production function for nonagricultural output, T (land) and K (capital) are specific (fixed) factors, and p is the relative price of farm output. V is maximized when

$$pF_L = G_L$$

or when $VMP_F = VMP_G$. Note that this formulation assumes that both economic activities (can) take place at the same time, with the result that the opportunity cost of labor in one sector (e.g., agriculture) is the value of the marginal product in the other sector. Suppose, instead, that production were seasonal, specifically, that the marginal product of labor in sector i was zero for some portion of the relevant time period (e.g., a year). Seasonal cycles can be said to mesh perfectly if, when the marginal product in sector i is zero, the marginal product in sector j is positive (and vice versa). With perfect seasonal cycles, labor is no longer mobile between the two sectors; rather, it moves between, say, nonparticipation (leisure) and one or the other sector, depending on the time of year. While seasonality was certainly important during the antebellum period and there was some meshing of the agricultural and nonagricultural cycles (see, e.g., Earle 1992; Engerman and Goldin 1993), it would be incorrect to claim that it was perfect, in the sense discussed above.

2. Implicit in the model is the assumption that the demand for both goods is perfectly elastic—i.e., p_{nf}/p_f (f = farm, nf = nonfarm) is a constant. If, instead, the relative demand for nonfarm goods were an inverse function of the relative price, an improvement in technology could drive down the value of the marginal product in the nonfarm sector, and labor would migrate into the farm sector.

3. In the so-called Harris-Todaro model, the resulting wage difference between the formal and the informal sectors adjusts to compensate workers for the cost of unemployment (see Williamson 1991).

4. Weiss's (1992, 24–25) major revisions for the early decades of the century involve the reallocation of slave labor (attributed by Lebergott to agriculture) to the nonfarm sector, while his revisions at midcentury involve the reallocation of substantial numbers of common laborers to farming. Because the revisions to the early years have more of an effect on the South than on the North, while his revisions for midcentury are more uniform in their geographic effect, Weiss's figures imply a slower pace of industrialization outside the South than previously thought.

5. Or, indeed, at all. According to Field (1978), the puzzle is why industrialization would take hold in such a land-abundant economy as the United States.

6. The labor that was vented off the farm was, according to Field (see also Goldin and Sokoloff 1984), children and, especially, young women. Migration to the frontier was difficult and costly for adult males, all the more so for young women (but, for evidence that hired hands who were young might also migrate long distances on their own in search of work, see Schob 1975). Young women who might work as domestics on farms were forced to seek work in manufacturing. A serious problem for Field's argument, however, is the fact that industrialization apparently raised the earnings of young women relative to those of adult males, while the

opposite would have been true if Field's model were correct (see Goldin and Sokoloff 1984).

7. For example, Simkovich (1993) explicitly assumes the absence of wage gaps in his analysis of Field's argument (see, e.g., his fig. 3).

8. In this respect, far better data are available for studying postbellum wage gaps (see, e.g., Hatton and Williamson 1991). Lebergott (1964) produced extensive estimates of farm, common, and manufacturing wages for the antebellum period, but he did not use them for the purpose of estimating wage gaps.

9. All Bidwell and Falconer's wage comparisons refer to farm labor paid by the month or by the day and "mechanics" paid by the day, even though mechanics were certainly skilled laborers by the standards of the day.

10. The nonfarm wage series used to construct the Vermont wage ratio was drawn from the Aldrich Report of the 1890 census and does not appear to be specific to Vermont (see the notes to Williamson and Lindert 1980, table E, p. 313), although it is specific to the Northeast. Since Vermont farm wages were relatively low by Northeastern standards, particularly in the 1830s (see Lebergott 1964, 539), it is likely that Williamson and Lindert's estimates of the Vermont wage gap are biased upward.

11. Williamson and Lindert (1980, 72 and app. E) claim that their Massachusetts common wage pertains to urban labor, but nowhere does Wright (1889) give any information about the geographic location of the common laborers whose wage rates he collected for his sample.

12. Later in the chapter, however, I show that Williamson and Lindert's analysis of the published 1850 data was faulty and that their conclusion that (aggregate) nominal wage gaps "were trivial" cannot be sustained.

13. Comparing the wages of farmhands with those of carpenters or mechanics (à la Bidwell and Falconer 1925) would confuse a true wage gap with one produced by differences in human capital. Ideally, we would have a sample of individuals employed in both sectors and use hedonic regression to control for worker characteristics, but such samples are not available for the antebellum period. For an example of such a study, see Hatton and Williamson (1991).

14. The harvest was an apparent exception. Harvest labor was paid a wage premium, evidently because the supply of labor to the farm sector during the harvest was less than perfectly elastic (see Schob 1975; and Rothenberg 1992). The farm wage data analyzed here refer to monthly labor, whereas harvest labor was typically hired by the day or the task (Schob 1975).

15. Twenty-six days per month is the standard figure assumed for full-time work during the antebellum period (see Schob 1975; or Lebergott 1964).

16. Ideally, I would use county-level estimates of the number of hired hands and (unskilled) nonfarm laborers, but these are simply not available. It is debatable whether slaves should be excluded from the weighting scheme since slave labor was hired out into nonfarm (or even farm) tasks (see Fogel and Engerman 1974). To test the sensitivity of the results to the inclusion of slave labor, I recomputed the Southern sample averages (the adjusted row) for 1850, excluding slave labor as follows. First, slaves were excluded from the count of the farm labor force of each state (North Carolina, Virginia, Kentucky, and Tennessee), as given by Weiss (1992, 53). Next, I estimated the nonfarm slave labor force, assuming that the agricultural slave labor force was 0.74 times the total slave labor force (see Weiss 1992, 54). Finally, using Weiss's (1992, 37) estimates of the total labor force, the free nonfarm labor force was computed for each state. Using these weights, the adjusted farm wage was $14.50 and the adjusted nonfarm wage $16.46 for the Southern sample in 1850, very similar to the figures reported in table 4.1 below.

17. For example, the use of manufacturing labor in place of my estimate of nonfarm labor would not change the substantive results.

18. The use of β may be more appropriate since it gives more weight to counties with relatively more employment in the nonfarm sector, which was expanding over time in settled areas (but see n. 16 above).

19. Weights for arbitrarily defined local economies (e.g., ones that cut across county or state boundaries) would require computations based on the manuscript Censuses of Agriculture and Manufacturing, computations that, while technically feasible, are well beyond the scope of this chapter. It should be noted that the use of census political units (e.g., counties, states, census regions) in the analysis of market integration has a long history in the literature (see also chap. 5). For example, state per capita income data have formed the basis of numerous studies of labor market integration, such as Williamson and Lindert (1980).

20. The Northern aggregate gaps are larger than the state-specific gaps because of weighting; in computing the mean farm wage, more weight is given (via the distribution of improved acres) to the frontier states, where the farm wage was relatively low compared with, say, Massachusetts.

21. However, it is quite likely that nonfarm labor within counties was concentrated in towns or cities, where the cost of living was higher than in rural areas. As I later demonstrate, the aggregate wage gap declines once cost-of-living differences are taken into account. I cannot replicate this analysis, however, within counties because I lack suitable sector weights (e.g., improved acres) at the minor civil division (MCD) level. Thus, use of the β weights probably overstates the true within-county gap.

22. This regression is estimated at the minor civil division level, pooling the data across states and over time (seventy-nine MCDs reported a monthly common wage instead of a daily wage). The dependent variable is the log of the common wage; state dummies, a dummy for 1860, and a dummy for the misreported observations are the independent variables. It would be desirable to estimate a similar regression for farm labor, but the number of misreported observations on the farm wage was judged to be too small (ten observations) to obtain a reliable estimate.

23. The adjustment factor shifts the distribution of farm wages closer to the distribution of nonfarm wages, thereby closing the wage gap, on average (and throughout the distribution). Since the adjustment factor is constant, it does not compress either distribution, as I suspect county-specific adjustment factors would.

24. Studies of labor market integration have frequently had to rely on food prices to adjust for geographic differences in the cost of living (see, e.g., Rosenbloom 1990; and chap. 5 below).

25. Defining an *urban* county as one that had at least one urban area with population greater than ten thousand, the cost of board was higher, on average, in such counties by 34 percent in 1850 and 25 percent in 1860 (these estimates were derived from regressions of the log of the weekly cost of board on the urban dummy, also including state dummies).

26. Williamson and Lindert's estimates compare the daily wage of farm labor with board and the daily wage of common labor with board. Williamson and Lindert (1980, 31) assert that, because both wages include board, "cost of living differences are unlikely to matter much." However, in equilibrium, the difference between the daily wage with and without board should approximate the value of board.

27. Williamson and Lindert's estimates of daily wages of common and farm labor at the regional level and for various states are taken directly from Lebergott (1964), who produced them from the average wages reported in the 1850 census. The regional estimates are weighted averages of the state-level wage figures. In the

case of the common laborer, the weight is Lebergott's estimates of the number of nonfarm laborers; in the case of the farm wage, the weight is Lebergott's estimate of the number of free white farmers.

28. Williamson and Lindert converted monthly farm wages to daily farm wages using an adjustment factor derived from table A-30 in Lebergott (1964, 546). Lebergott's table gives estimates of monthly and daily wages with board, both purportedly for farm labor, in 1832. Williamson and Lindert's adjustment factor is simply the ratio of the monthly to the daily wage. But Lebergott's text (pp. 258, 267–68), table A-23 (p. 539), and table A-25 (p. 541) make it clear that the *monthly wage* in his table A-30 refers to farm labor and the *daily wage* to common labor. As Lebergott (p. 267) notes, the ratio of the day wage of common labor to the monthly wage of farm labor in 1832 was approximately the same as the ratio prevailing in 1850.

29. I assume that the average female wage was 55 percent of the average male wage; this ratio is derived from fig. 3.1 in Goldin (1990, 62). Assuming the same female-male wage ratio for each county, I derive an estimate of the male wage on the assumption that w_m is a weighted average of the male and the female wage, the weight being the proportion female.

30. This procedure was used for each state except Tennessee, where it resulted in implausible nonfarm weights. In the case of Tennessee, the common wage was aggregated using census figures on the manufacturing labor force.

Chapter 5

1. Coelho and Shepherd's (1976) regional series begin in the 1850s. On capital market integration before the Civil War see Bodenhorn and Rockoff (1992).

2. Per capita incomes in the East South Central region, however, were nearly twice as high as in the South Atlantic or West South Central regions.

3. A related explanation involves liberal policies toward the disposal of public land, which effectively subsidized western movement (see, e.g., Fogel and Rutner 1972; Temin 1969; or Lebergott 1985).

4. On the other hand, the implicit assumption is that the "weekly cost of board to laboring men"—the census definition of the concept—meant the same thing to all respondents; i.e., the quantity of food purchased by the weekly expenditure was (approximately) the same in all parts of the country. If board meant much fancier meals in, say, New York City than in rural Iowa, this assumption would be false.

5. The weekly cost of board divided by the weekly wage (without board) is an estimate of the budget share for food. If the weekly wage for common and farm labor is estimated by multiplying the daily wage without board by six days per week (the daily wage for farm labor is the monthly wage plus the [imputed] value of board divided by twenty-six days of work), the mean value of the budget share is 0.39, or 39 percent. This might seem low, but it must be remembered that this is an estimate for a single male, without dependents, working full-time. Allowances for unemployment and dependents would push the budget share up into the range observed for working-class households during the period.

6. Note that board is used in this manner only for the benchmark (i.e., 1850) deflator. Changes in the relative price indices over time also reflect changes in nonfood prices because nonfood prices are included in the indices (see chap. 3 below; and Goldin and Margo 1992a).

7. To derive the estimate of the relative (Midwest-Northeast) cost of living in 1851, I multiply the benchmark for 1850 (= 0.77) by the ratio of the Midwest-Northeastern price deflators for 1851.

8. I use farmers as well as laborers because chap. 5 found that the wages of farm and common laborers were similar. The implicit assumption is that the ratio of farm laborers to total farmers in each region were similar.

9. The timing of the convergence in relative wages in the North contrasts somewhat with Ross's (1985, 43) assertion that migration "in the late 1820s and 1830s brought the artificially high wages of the labor scarce frontier . . . more in line with lower eastern wage levels."

10. By *other forces,* I mean the effects of factor price equalization. It is well known that falling costs of internal transport produced convergence in output prices between the Midwest and the Northeast (Berry 1943), and it is plausible to argue that factor price equalization occurred in response to output price convergence (O'Rourke and Williamson 1994; Slaughter 1995).

11. Specifically, the aggregate North-South gap (in logs) was −0.027 for 1821–30, 0.103 for 1831–40, 0.175 for 1841–50, and 0.076 for 1851–60.

12. In choosing the North over the South, unskilled immigrants were, according to my findings, migrating to those places where real wages were higher on average. Skilled immigrants in the 1840s, however, would have enjoyed higher real wages in the South (see table 5.3) and, thus, must have avoided the South for other reasons (e.g., slavery or the climate).

13. Booming conditions in the cotton market may also explain some of the narrowing of the North-South wage gap in the 1850s (see Fogel 1989).

14. That is, $0.045 = [\exp(0.0152 - 0.0141) \times 40] - 1$. For artisans redistribution added 3.6 percent over four decades, for common laborers 2.8 percent.

15. An alternative approach to measuring the effect of migration on aggregate wage growth is to allow the regional occupation shares to change but to fix the wage gaps at their initial level. For the purpose of illustration, I fix the wage gaps at their average levels for the period 1825–30. Recomputing the aggregate series under this assumption, and then reestimating the trend growth rates as in table 5.5 ($\ln w = \alpha + \beta T$), the resulting β's are 0.00174 for common laborers, 0.00226 for artisans, and 0.00183 for white-collar workers. These rates imply much larger effects of migration; e.g., migration raised the growth rate of common wages by 16.7 percent (= 0.00174/0.0104). The larger effects result because the alternative approach does not permit wage convergence to occur as a consequence of internal migration.

16. If w/p is computed relative to the state average and all variables are in logs, this is equivalent to adding terms like $\beta w/p_{\mu 50} + w/p_{\mu 60}$ for each state to the right-hand side of the regression, where μ = the state average. Since these terms are the same for all counties in a state, an equivalent procedure is to include a full set of state dummies.

17. The growth rate of population is log (population 1860/population 1850). The change in urbanization is a dummy variable that takes the value 1 if the county had an urban area of population ten thousand or more in 1860 but not in 1850.

18. Perhaps the best way to control for amenities or disamenities would be through county fixed effects (dummy variables), but this would require additional years of data. On the other hand, offsetting the bias in β toward zero is a bias toward −1 because of measurement error in the real wage. On the assumption that the errors in measurement are concentrated in the tails of the distribution, the effects on the estimate of β can be gauged by trimming the data, i.e., excluding observations with high or low values of $\ln(w/b)_{1850}$. For example, if the regression for common/farm labor is subjected to a 10 percent trim (the bottom and top 10 percent of observations are excluded on the basis of their values of $\ln[w/b]_{1850}$), the estimate of $\beta = 0.779$, similar to the estimate for the full sample.

19. According to the regression coefficient, about 80 percent of the relative wage

gap for a typical county was eliminated within a decade. This is much faster than, e.g., the speed at which the Midwest-Northeast wage gap was eliminated over the period 1820–60 (see table 5.2).

20. The weight $\gamma_j = s_j/s$, where s_j is Weiss's estimate of the total labor force (Weiss 1992, 37) less his estimate of the number of slaves in the farm labor force (Weiss 1992, 53). This overstates the free labor force in the South because some slaves were engaged in nonagricultural pursuits. However, experiments with different weights that also netted out estimates of nonagricultural slaves produced virtually identical deflators.

21. The regional adjustment factor is the unweighted average of the ratio $(\Sigma \delta_{jk}b_{jk})/b_j$ for the two states in each region in the eight-state sample (chap. 2), where k indexes counties, b_{jk} is the weekly cost of board at the county level (from the manuscript census returns), and δ_{jk} is the county's share of the state population. The adjustment factors differ from unity, indicating that, properly weighted, the average cost of board within states differed from the state averages published in the 1850 census.

Chapter 6

1. The California Gold Rush was not the only gold rush of the nineteenth century, but it was certainly the most famous. For a good general history of nineteenth-century gold rushes, see Marks (1994).

2. The Sutter's Mill discovery was actually not the first in California. Small amounts of gold were found in southern California near Los Angeles in 1842. The gold was quickly recovered by experienced Mexican miners, and a search for additional gold ensued, but none was found (Lavender 1976, 3).

3. According to Wright (1940, 342), forty-two thousand migrants are estimated to have arrived overland, while another thirty-nine thousand arrived by ship at San Francisco. In 1850, fifty-five thousand are alleged to have arrived overland, thirty-six thousand by sea. However, approximately twenty-seven thousand departed by sea in 1850 (the number departing in 1849 is unknown); whether they left permanently is not known. The estimates of arrivals by land derive primarily from newspaper reports and must be regarded as extremely rough. Records of ship arrivals were kept at the San Francisco customhouse and are presumably more accurate.

4. The share of miners per person in 1850 is biased upward because of damage to the manuscript census returns from a few nonmining areas, including San Francisco. However, adding the population counts from the 1852 state census to the denominator would still produce a substantial decline in miners per capita between 1850 and 1860.

5. The 1850 population figure is biased downward because of fire damage to the census manuscripts for San Francisco and other counties.

6. Evidently, sending clothes to Hawaii for cleaning (presumably to be picked up on the miner's next trip to San Francisco) could be cheaper than buying a new shirt or trousers or paying inflated prices for cleaning in San Francisco. Caughey (1948, 35) notes that the price of a coarse shirt in 1849 was $16.00, apparently high enough to make the cost of transporting dirty shirts to Hawaii for washing economically feasible.

7. A Dutch disease model is used to study the effects of supply-side shocks, usually the discovery of natural resources, on other sectors of an economy (e.g., manufacturing) (see Corden and Neary 1982). The name *Dutch disease* comes from the fact that the rise in the price of oil in the early 1970s apparently caused

resources to flow into oil production in countries like the Netherlands, at the expense of manufacturing.

8. By assuming equal endowment shares, I am relegating property rights issues (who owns the gold) to the background. For an analysis of property rights issues, see Umbreck (1977) and Clay and Wright (1998).

9. An alternative, essentially equivalent model divides the population of a gold rush economy into three groups: capitalists (owners of K), workers, and mine owners (owners of ore). The capitalists and mine owners hire labor in a competitive market. Aggregate demands for X and Z are then the sum of the group-specific demands (the incomes of the capitalists and mine owners are the rents accruing to K and O). There will be a balanced trade condition similar to the budget constraint in the text.

10. The maximization problem is

$$\max_{X,Z,L_x} U(X, Z)$$

subject to

$$p_x X + p_z Z = p_x F(L_x, K) + g(L - L_x)S,$$

where F is the production function for X, K is capital, and S is the endowment of gold ore. The first-order conditions are

$$U_x/U_z = p_x/p_z,$$

$$p_x F_L = g'S.$$

11. For example, $h = p_x^{\beta} p_z^{1-\beta}$, where β is the share of income devoted to X (recall that the price deflators developed in chap. 3 take this form).

12. The opposite would be true if demand for labor in the local sector fell, but this cannot happen if X is a normal good.

13. Capital might also be attracted if, in the new equilibrium, L_x is higher than initially.

14. That is, with specialization, there would be a marginal worker who, in the initial equilibrium, would be indifferent between harvesting gold and producing the local good.

15. Alternatively, individuals might believe (erroneously) that the gold rush will last forever and then be surprised by its end.

16. By *convex adjustment costs,* I mean that, at any time t, the marginal cost of moving L^* units of labor into the gold rush economy is increasing in L^*. Convex adjustment costs are frequently analyzed in dynamic models of labor demand (see, e.g., Hamermesh 1993; Carrington 1996).

17. By *long-run equilibrium value,* I mean the value of w in the absence of adjustment costs. With adjustment costs, there is no constant equilibrium value of w for all dates t in the interval (t, t').

18. That is, there is option value to waiting if the returns to migration are uncertain (see Dixit and Pindyck 1994). The transport costs of migration are sunk because the good (transportation) is perishable; however, for evidence that some migrants took goods with them to sell in the gold fields, which would be a way of recouping some of the sunk costs, see Caughey (1948, 98).

19. Such a belief would be rational if, as was the case in California, property rights to the gold were not established ex ante. In other words, if property rights had been established, it is plausible that some migration would have been delayed, producing a less elastic labor supply response. I am grateful to Lee Alston for this point.

20. See also Fraser (1983), who argues that the introduction of money by army quartermasters into the local indigenous economies of the Southwest had effects on local prices.

21. In particular, the time series are too short to distinguish between the effects of the real shock—the discovery of gold—on wages and any effects of the increase in the money stock.

22. Gerber (1997) has recently constructed annual nominal and real wage indices for California during the Gold Rush using such anecdotal evidence. Gerber's indices differ significantly in terms of the nature of the data and the manner of construction from those presented in this chapter. In particular, Gerber presents a single wage index meant to capture the average nominal daily wage covering a variety of occupations. Each occupation is weighted according to occupation shares derived from the 1850 census. However, the number of occupations included in the average differs from year to year (e.g., two in 1848, fifteen in 1853). Thus, Gerber's index is not a true fixed-weight index (in a true fixed-weight index, the same occupations would be represented every year); fluctuations in the average from year to year are almost certainly introduced when new occupations are added, not a desirable characteristic for a wage index. Gerber produces his real labor cost series by dividing the nominal wage index by Berry's overall wholesale price index. Although year-to-year movements in Berry's overall index and the price deflator are similar, the downward trend in prices from 1847 to 1860 is more pronounced in Berry's overall price index. The overall index includes nonconsumption goods (unlike the price deflator developed in this chapter), and thus its use as a price deflator to produce a real wage series is questionable—hence, presumably, Gerber's term *real labor cost.*

Prior to 1850, Gerber's data derive principally from the personal correspondence of miners and travel accounts. The 1850 estimate is based on the federal census, while those for 1851–60 derive from scattered figures reported in the newspapers *Alta California* and *San Francisco Prices Current.* Wage quotations from southern California were excluded from Gerber's index on the grounds that wages were lower in the southern part of the state, implying that "the state was not a well-integrated economic region" (Gerber 1997, 5). While my regressions also reveal that wages were lower in southern California, the payrolls themselves clearly demonstrate that labor could still command impressive pay by national standards, such as the herdsmen employed at the fort at San Diego who earned $100 per month in 1850, a figure far in excess of wages for similar work elsewhere in the United States. This suggests that southern California locations were not immune to the wage effects of the rush (and, therefore, should not be excluded from the sample on a priori grounds). The number of wage quotations in Gerber's sample is extremely small—e.g., the 1847 estimate was apparently based on only two wage quotations, that for 1848 on six (Gerber 1997, 24).

It is difficult to determine the accuracy of wage quotations contained in letters and other testimony of migrants (or even in newspapers) in the light of the known hyperbole characteristic of accounts of the early years of the rush. A large sample of wage observations from payroll records, such as those used in this chapter, would seem clearly preferable for constructing wage indices because such data record actual wage payments, unlike the quotations evidently underlying Gerber's index.

In any event, comparisons of the nominal and real wage series presented here with those by Gerber are difficult to make because of the very different methods of construction. However, the basic findings are very similar. Gerber finds that real wages peaked in 1849; my real wage series for common laborers and artisans (the occupations examined by Gerber) also peak in 1849. Gerber also finds, as do I, that the Gold Rush permanently raised real wages in California (according to his real labor cost index, real wages were 4.9 times higher in 1860 than in 1847) and that the wage effects of the rush were largely over by the mid-1850s. Using my estimates of labor supply (see app. 6B), Gerber's wage series also imply an inelastic supply of labor into California in the late 1840s (ε = 0.15 from 1847 to 1848, d[ln L] is my estimate of the change in labor supply from 1847–February 1848 to March–December 1848, and d[ln w] is the change in Gerber's [1997, 17] index of real labor cost from 1847 to 1848), followed by a flattening (e.g., ε = 1.85 from 1847 to 1852).

23. For example, common laborers hired at army installations in the San Francisco Bay area could command up to \$120-\$150 per month in 1850 and \$5.00–\$6.00 per day, while some carpenters were paid up to \$12.00 per day (although most were paid less, \$10.00 per day). According to the manuscript Census of Social Statistics for San Francisco, day laborers earned \$6.00 per day and carpenters \$9.42 (quoted in Gerber 1997, 5). San Francisco wages were higher than the averages estimated for the entire state, which indicates the importance of having a large sample covering a variety of locations rather than a few isolated locations that may or may not be representative of the average.

24. Hispanic status is inferred from name or from ancillary remarks in the payrolls; as noted in chap. 2, the reporting of names in the payrolls is haphazard, particularly at larger forts.

25. Coman (1912, 2:317) also suggests that nominal wages of carpenters fell by 1853, but, unfortunately, she provides no source citations for her wage quotations.

26. To construct the deflator, I used Berry's annual price indices (budget shares in parentheses) for candles (0.098), coffee (0.075), flour (0.150), hams (0.189), raisins (0.033), rice (0.003), cotton sheeting (0.260), sugar (0.085), tobacco (0.025), and butter (0.133). The price deflator is a geometric weighted average of the commodity-specific indices, with weights equaled to the budget shares, as above.

27. The drop in prices in 1848 may reflect the sudden exodus of population from San Francisco for the gold fields. According to Coman (1912, 2:257), real estate prices fell sharply in 1848 in the immediate aftermath of the exodus, and the price deflator suggests that the same may have been true of other prices.

28. Berry drew his price quotations from newspapers, and therefore the bulk of the quotations pertain to urban prices (chiefly, San Francisco). Since most goods were transported to the camps from San Francisco (or Sacramento), retail prices at the camps would include transport costs as well as other markups (Coman 1912, 2:271). However, because of the dependence on urban areas for supply, there is no reason to believe that prices at the mining camps followed a vastly different annual pattern than that indicated by Berry's data.

29. The growth rate of real wages for common labor in California over the period from 1847–February 1848 to 1860 was 10.1 percent per year, compared with 1.9 percent per year for 1847–60, as computed from my aggregate national real wage series for common labor (see chap. 5).

30. Using the state-level figures on the weekly cost of board (from Kennedy 1864, 512) and on the daily wages of common laborers without board (from Lebergott 1964, 541), I compute

$$r = (w_c/b_c)/(w_n/b_n)$$

for 1860, where w is the wage, b is the weekly cost of board, c refers to California, and n refers to the rest of the United States (excluding the West): r = 0.99 (note that r is the same measure of real wages as used in chap. 5). Taken at face value, the calculation suggests that real daily wages in California for common laborers on the eve of the Civil War were virtually identical to real wages elsewhere. Similar results were found for male manufacturing workers in 1879 by Rosenbloom (1996, 644); not until the late nineteenth and early twentieth centuries did a sizable real wage gap in favor of the West open up (Rosenbloom 1996).

31. As noted in sec. 6.1, East Coast ships did occasionally trade manufactured goods for furs in pre–Gold Rush California, but their small numbers and infrequent stops make it difficult to maintain in any meaningful sense that pre–Gold Rush California was integrated into the American economy (see Coman 1912, 1:165).

32. For example, using the real wage series for common labor for California, the national aggregate series in chap. 5, and the observation (see n. 30 above) that real wages of common labor in California in 1860 were essentially the same as in the rest of the country, the average real wage of common labor in California over the period 1850–52 was 12.5 percent higher than the national average. If the calculation is made for 1849, the real wage gap in favor of California was 56.1 percent.

33. Estimates for 1850 and 1860 of capital in manufacturing (in current dollars) and of population are from DeBow (1853) and Kennedy (1862). Nominal capital per person is deflated by Berry's overall price index (1984, 235, col. 1). Improved acres and wheat output are from Kennedy (1862, 196, 200). For further discussion of the emergence of California agriculture during the Gold Rush period, see Gerber (1993).

34. The elasticity of labor supply appears to have been somewhat smaller in the case of the Australian Gold Rush. According to Maddock and McLean (1984, 1065), real wages in Victoria increased by 85 percent between 1850 and 1852. Maddock and McLean do not report labor quantities, but using population or net migration (see Maddock and McLean 1984, 1048) as a substitute yields elasticities ranging from 1.3 (population) to 1.7 (net migration). My estimates of the medium-run labor supply elasticity into Gold Rush California (e.g., 1848–51) are in the same range as Rosenbloom's (1991, 435) estimate (ε_{Lw} = 1.96) for common labor in the building trades in the late nineteenth century, which is derived from cross-city differences in wages and employment.

35. The assumption that daily hours of work in Gold Rush California did not increase biases the elasticity downward, compared with the Alaska figure. However, daily hours of work in Gold Rush California would have had to increase to more than twenty-four hours per day—obviously impossible—to equal the Alaska elasticity.

36. It is possible that my estimates of the labor supply elasticities into Gold Rush California are biased upward because of uncertainty. Some individuals may have been attracted to California by the (small) prospect of striking it rich rather than by the average returns to migration; faced with an alternative decision in which the prospective argonaut was guaranteed the average returns, the decision would have been to stay in the East (or wherever the place of origin).

37. However, it is easy to overstate the degree to which the antebellum labor supply was less flexible than today's in response to geographically localized shocks to labor demand. The evidence on real wages and labor supply elasticities in this chapter suggests that labor supply adjustment to the discovery of gold per se was essentially completed by the early to mid-1850s, the traditional dating of the end of the rush, lasting five to seven years. Recent work by Blanchard and Katz (1992) has examined the adjustment process of state economies to shocks to labor demand over the post–World War II period. According to Blanchard and Katz (1992, 33), the effects of shocks on labor supply to the average state take six to seven years to dissipate, similar to the duration of the adjustment period in the case of the Gold Rush. Blanchard and Katz also find that much of the short-run

adjustment to negative demand shocks comes in the form of higher unemployment rather than lower wages. Two years after a negative shock to labor demand of 1 percent, wages fall by about 0.2 percent, compared with about a 0.25 percent decline for labor supply, implying a short-run labor supply elasticity of 1.25, compared with my estimate of 1.01 for California between 1847 and 1849.

38. However, my estimate for common laborers in 1850 of $4.20 per day is reasonably close to the census figure of $5.00 per day. Readjusting my estimate for artisans for 1850 so that it refers solely to carpenters produces a daily wage of $6.95, compared with the census estimate of $7.60.

39. Wright (1940, 342) estimates that between 4,200 and 6,350 individuals arrived overland, which I arbitrarily average at 5,000. There are no records of arrivals by ship at San Francisco for this year, so I assume zero arrivals. However, there must surely have been some arrivals by ship, which implies that my estimate of the short-run elasticity is biased downward. For 1849, I sum Wright's estimate of overland arrivals plus 27 percent of reported arrivals; the 27 percent adjustment is the ratio of departures (by sea) to arrivals in 1850 (thus, I am assuming that all departures by sea were permanent). For 1851, I add arrivals by land and sea in both 1850 and 1851, again assuming that 27 percent of arrivals by sea in 1851 were permanent. Finally, I multiply the population estimates for 1847–52 by 0.77, the assumed ratio of adult men aged fifteen to forty to the total population. All figures are rounded to the nearest hundred. Note that I am assuming that all men in this age group in California were in the labor force (labor force participation rates for adult men of all ages in 1850 were close to 90 percent [see Weiss 1992]) or, equivalently, that the labor force participation rate in California did not change much over the Gold Rush (if it rose, then my labor supply elasticities are biased downward). The labor supply estimates for California are as follows: 1847–February 1848, 11,500; March–December 1848, 15,400; 1849, 55,800; 1850, 71,600; 1851, 144,800; 1852, 203,300; and 1860, 186,200.

40. The 1850 federal census estimate of population is understated because of damage to certain census manuscripts. The understatement produces a downward bias in my estimate of the labor supply elasticity for that year.

Chapter 7

1. The occupational shares are 0.71 for unskilled laborers, 0.21 for artisans, and 0.08 for white-collar workers. To obtain these shares, I first summed the number of common laborers, teamsters, and artisans in the building trades (carpenters, masons, plasterers, painters, and glaziers) and clerks reported in the 1850 census (DeBow 1854, 126–28). The unskilled labor share is the share of common laborers and teamsters in the total (= 924,255/1,301,807); the artisan share is the share of building tradesmen in the total (276,229/1,301,807); the white-collar share is the share of clerks in the total (101,325). Adding in additional white-collar or artisanal occupations does not alter the substantive results.

2. To be fair, my aggregate real wage series exclude certain groups—the most important being slaves and free female labor—that contributed to the growth of total output, along with a variety of occupations not represented in the army data.

3. Poor harvests in the Northeast in the mid-1830s and in Europe in the early 1850s may be other examples of (negative) real shocks (see Sokoloff and Villaflor 1992). According to Sokoloff and Villaflor, the demand for labor fell as a result of the agricultural supply shock in the 1830s, producing declines in real wages of farm labor (and, by inference, common labor) in the mid-1830s (see chap. 3, table

3A.9). However, the price level began rising in the early 1830s, well before the alleged harvest failures (see table 3A.3 above; and Temin 1969).

4. The differential rates of growth of skilled and unskilled immigrants suggest that the skill differential ought to have risen in the Northeast in the 1850s, that being where the majority of immigrants arrived; in fact, the differential did increase by 9.2 percent comparing the 1850s to the 1840s (see chap. 3).

5. If the regression is estimated in first-differences, the coefficient is -0.90 (t = 7.73).

6. The correlation might also be biased away from zero because the nominal wage series were constructed from hedonic regressions that did not fit the data perfectly; hence, some of the year-to-year movement in real wages reflects the prediction error of the hedonic regressions (Williamson 1992). However, it should be kept in mind that the hedonic method is simply a way of producing an average; if, according to the regression, wages on average did not keep pace with prices during periods of inflation, it follows that, at the individual level, some wages moved in the opposite direction.

7. In general, antebellum consumers had three sources of retail supply: general stores, public markets, and direct purchases from manufacturers. For perishable items, often obtained from public markets, it is reasonable to assume that price fluctuations were felt directly (and immediately) by consumers regardless of the source of supply. Manufacturers supplied wholesale markets as well as consumers, so it is reasonable to suppose that, in these cases, annual price fluctuations at the wholesale and retail levels were similar. General stores obtained goods directly from wholesale markets or from jobbers. General stores tended to replenish their stocks twice a year or so; because there were few, if any, contractual mechanisms to hedge price risk, it is reasonable to assume that antebellum retailers passed on *some* portion of wholesale price fluctuations. However, if the retail markup were more stable than wholesale prices, retail prices would fluctuate less than wholesale prices. This would be true, e.g., if general stores possessed some local monopoly power. On antebellum wholesale and retail marketing, see Jones (1937).

8. This is particularly true for the nonbenchmark years for which David and Solar interpolated using prices in Vermont for goods not regularly traded in wholesale markets.

9. Effectively, I am assuming that the price of housing relative to that of other goods did not change between the late 1830s and the early 1840s.

10. However, there is evidence of downward rigidity of nominal wages: wage cuts in the early 1840s were slightly less than half price declines. Unemployment is thought to have been high in the early 1840s, and the downward rigidity may be an important reason why (but see Temin 1969).

11. *Stable* here does not necessarily mean "constant," only that deviations from the "normal" price level are well understood (as in the case of seasonality) or long-term trends (as in the Midwest, where the long-term trend in prices was upward).

12. Note that abrupt adjustments *are* evident in the wage series. For example, nominal wages of common labor jumped sharply in 1837 in the Northeast and Midwest and declined sharply in both regions in 1840.

13. The historical literature on height is now voluminous. A representative recent selection of papers may be found in Steckel and Floud (1997).

14. Komlos has tried to bolster his case by presenting estimates of food consumption that suggest declines for the relevant cohorts, although this has been disputed by Robert Gallman (1996) (for a reply, see Komlos [1996]).

15. Specifically, Weir (1997, 179) shows that median adult height at age twenty is positively and significantly correlated with the real wage at approximately age

twenty. For the sake of argument, I will assume that the effect would be the same if the real wage were measured from birth to age four.

16. It is reasonable to assume a positive income elasticity of demand for nutrients on the basis of the French results because Weir (1997, 173) shows that real wages and per capita meat consumption were positively related at the department level.

17. I use Weir's (1997, table 5.4, col. 2b) regression, which reports the coefficient of a pooled time-series cross-sectional regression of median adult height (age twenty) on the real wage. All variables are measured at the department level. To convert the coefficient (16.88) into an elasticity, I use the average height of the 1843 birth cohort (1,647.2 centimeters), the midyear of birth years covered in Weir's study. Thus, the elasticity of height with respect to the real wage is 0.01 (= 16.88/1,647.2).

18. However, Komlos and Coclanis also show that heights continued to decline among the birth cohorts of the 1840s, when, according to my estimates, real wages in the South Atlantic were higher than they were in the 1830s. The only way to explain the reversal of the correlation in the 1840s is to argue that the sharp rises in real wages in the early 1840s are not an indication of rises in labor welfare but a consequence of nominal wage rigidity that produced rising unemployment.

19. The finding that the ratio of artisans' pay to common laborers' pay fell before the Civil War appears to contradict evidence put forth by Williamson and Lindert (1980). Williamson and Lindert present two series of skilled-unskilled wage ratios, the first constructed from data collected by Carroll Wright (1885) and the second a linked series constructed by splicing together certain of the wage series discussed in chap. 2. Both series, which pertain primarily to artisans, show substantial increases in artisan-unskilled wage ratios from the 1820s to the 1850s, the opposite of my findings. Elsewhere, however, it is demonstrated that the increase evident in the series based on the Carroll Wright data is a consequence of failing to control for changes in the composition of Wright's sample over time and that the increase evident in the linked series is a consequence of too low a level of skilled wages in the 1820s. For a detailed discussion of these points, see Margo and Villaflor (1987).

20. Habakkuk (1962, 23) asserted that the "premium on artisan skills was generally lower in America than England in the early nineteenth century." My findings do not support Habakkuk. According to Williamson (1985, 13), the skill differential for artisans (ser. 4H, building trades) compared to nonfarm common laborers (ser. 2L) was 1.51 in the 1820s (this is an unweighted average of the skill differentials for 1819 and 1827). My estimate for the American Northeast (Habakkuk's focus since he was concerned with industrialization) is 1.67 for the 1820s (this is the ratio of the decadal average daily wage for artisans to that for common laborers, from chap. 3). (See also Adams 1968.)

21. Here a caveat is in order. Recall from chap. 3 that I am not able to control for years of work experience in the hedonic wage regressions. It is possible that the rising return to white-collar labor reflects changes in the composition of the sample such that the average clerk was more experienced later in the period than earlier. If this were true, and if experience had a positive effect on wages (as it does today), then I would be overstating the extent to which the white-collar skill differential grew during the antebellum period.

22. The ratio for the nation as a whole is computed from the national series of nominal wages in chap. 5. A monthly wage for common labor is computed by multiplying the daily wage by twenty-six days. This overstates the monthly common wage because no adjustment is made for an unemployment risk premium (see chap. 3); hence, the wage ratio is biased downward. Goldin's estimates (see also

Goldin and Katz 1995, table 5) are based on annual earnings. If, as seems plausible, white-collar workers labored for more days per year than common laborers, my estimate of 2.07 for the 1850s would be biased downward on an annual, as well as a monthly, basis.

23. Goldin and Katz (1995) also report much higher wage ratios for the South than for the rest of the United States.