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I explain changes in the wage structure favoring more skilled workers since 1980 using job task data from the Occupational Information Network and wage data from the March CPS. Using recently developed partialling out estimators including debiased machine learning, I obtain wage effects for a suite of tasks, including a novel computer task category, by education-experience group and year. I interpret these effects as shadow prices and use trends in these effects to determine bias in technical change over the period. I find that changes in these wage effects are supportive of technical change biased towards nonroutine analytic and communication tasks, but not of a general bias towards nonroutine task categories.

WAGE EFFECTS OF CHANGES IN THE TASK COMPOSITION OF THE

WORKFORCE FROM 1980 TO 2015

by

Joshua K. Gorsuch

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Approved by

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Committee Co-Chair

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Committee Co-Chair

APPROVAL PAGE

This dissertation written by Joshua K. Gorsuch has been approved by the following committee of the Faculty of The Graduate School at The University of North Carolina at Greensboro.

Committee Co-Chair ___________________________________

Committee Co-Chair ___________________________________

Committee Members ____________________________________

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CHAPTER I

INTRODUCTION

Since 1980, the wage gap between skilled and unskilled workers grew substantially. Figure 1 shows median hourly wages for men in the U.S. from 1980 to 2015 by education and potential experience. What we see is that the wage gap for skilled (i.e. educated) workers rose dramatically over the last 40 years. For workers with less than 10 years of potential experience, this gap roughly doubled from about \$5 to \$10 in hour in 2015 dollars, with a more modest increase in the gap for workers with more than 25 years of potential experience. This happened over a period in which the relative number of college graduates and high experienced workers rose dramatically. The value of work done by high skilled labor has risen while the quantity of high skill laborers has increased. The catch here is that “work done by high-skill laborers” conceals enormous complexity. High-skill laborers do all sorts of work. Does the productivity increase apply to all the work they do, or only some types of work that high-skill laborers disproportionately supply? Are high-skill laborers supplying more of some types of work, or are they benefiting from labor-augmenting technology?

Over the same period, jobs changed in other ways, including through the introduction of computers in the workforce. These trends have not gone unnoticed by economists, and many have advanced theories connecting this skill gap in wages to changes in jobs, most notably skill-biased technical change. What I contribute to this

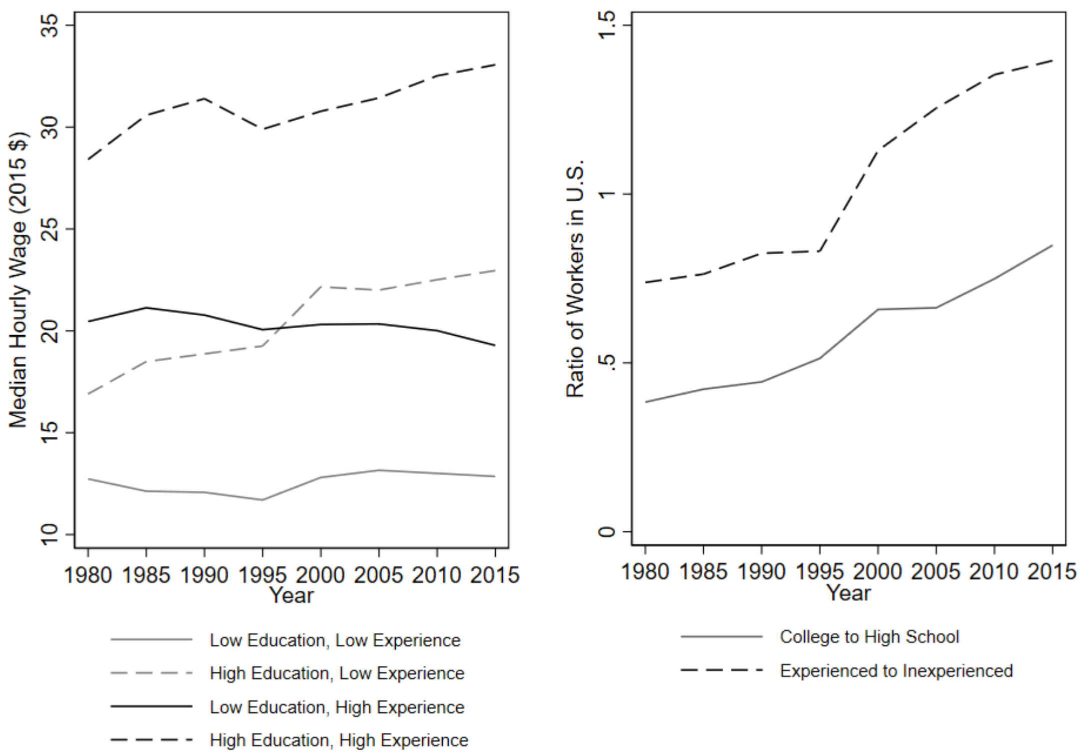

Figure 1. Median Wages and Relative Labor Quantities
Source: CPS

discussion is to use both modern datasets and recently developed empirical methodologies to make and test these theoretical claims in the familiar economic framework of prices and quantities. This allows a vastly more flexible approach to modeling bias in technical change and to modeling wages in general since we can make fine distinctions between labor types.

Unpacking the details of what makes high-skill workers more or less productive over time has been a major focus of the labor economics literature. In chapter 2, I review the relevant parts of the literature. This includes a look at the cohort size literature that analyzed the effect of the entry of the baby boom generation into the labor market, with special attention to Freeman (1979), then a discussion of the literature on the task-based framework of jobs, then the literature on the wage effect of computer usage at work. These discussions will show that economists have made great progress on deciphering what led wages to rise for high-skill workers. That said, progress in the task-based framework has been hampered by data limitations. Also, much of the recent research on biased technical change is inaccessible even to experts in related fields due to highly idiosyncratic empirical methods, as opposed to one based on the relationship between prices and quantities. These are issues that the latter half of this dissertation works to resolve.

In chapter 3, I replicate the work of Freeman (1979) as an exercise. What I show here are the limitations of the framework in which workers each provide undifferentiated labor, with labor only differing between education-experience groups, along with the powerful interpretability of Freeman's model of relative wages. Freeman (1979) explains relative wages using relative labor quantities in the framework of labor types being imperfect substitutes, work that spawned the cohort size literature and continued to influence the later literature on biased technical change, particularly through Katz and Murphy (1992).

In short, we can learn much from the relationship between prices and quantities. Freeman regresses the log wage ratio for experienced and inexperienced workers on the log of the labor ratio and obtains both the elasticity of substitution between them and a trend in relative productivity that received less attention. The limitation of his methodology is that he has only time series data and must assume some parameters constant over time to identify the model. The example serves to motivate the following work of obtaining prices and quantities for tasks as an approach to answering questions about biased technical change.

The remainder of this dissertation works through a multistep process to describe changes in the task composition of the workforce over this time period and show how wages have changed in response. These chapters both expand on previous methods of the task-based literature and produce datasets usable in a variety of areas, including modeling wage inequality, biased technical change, and occupation selection. Put simply, describing a situation in terms of prices and quantities makes the situation widely understandable. Give me prices and quantities and I can tell a story to an undergraduate. See this worker whose wages went up? This worker has been doing the same amount of tasks A and B for 10 years, and over that time the price of task A rose. The price of task A rose because a trend in technology that affected all workers doing task A.

In chapter 4, I adapt existing techniques from the task-based literature to measure tasks done by workers at the individual level from 1980 to 2015. My methodology is drawn primarily from Acemoglu and Autor (2011) and Peri and Sparber (2009). The approach here is significant for three reasons. First, I treat individual workers as performing a vector of task quantities as opposed to the more common approach of sorting occupations into single task categories. This approach is more flexible and is better able at deal with cases in which there are more than two or three possible tasks. Second, the approach here permits task quantities to be aggregated across workers, provided assumptions hold about the distribution of task quantities in the population. This is a valuable property because the tasks provided by an individual worker affect the worker's wage, but can reasonably be assumed too small to influence the market price. Meanwhile, the aggregate task quantity can be used to model price levels (i.e., we have quantities.) This is a valuable but underutilized advantage of the method used in Peri and Sparber (2009). Third, I introduce computer tasks as a category in the task-based framework. This category has not been explored and its inclusion has a variety of benefits I discuss in more detail later.

In terms of results, I find that the relative intensity of tasks between education-experience groups has been surprisingly stable since 1980, with the introduction of computers being the most obvious change to the task composition of the workforce. I also find substantial change in the task compositions of local (state) labor forces[1] that suggests local specialization in tasks is an underappreciated factor affecting the structure of wages, though further analysis is beyond the scope of this dissertation.

In chapter 5, I estimate implicit shadow prices for specific tasks based on wages for individual workers and their task quantities. Wage data is from the March CPS and task data is based on the values obtained in chapter 4. The method is based on the approach in Peri and Sparber (2009), but replaces their estimator with a powerful, recently developed partialling-out estimator that uses lasso to select control variables, introduced in Chernozhukov et al. (2018). Conceptually, the methods are similar. I obtain

[1]That is, the aggregate quantities of a suite of tasks within a state. For details on the construction of aggregate task labor quantities, see chapters 4 and 6.

shadow prices as the coefficients in a regression of wages on task quantities, and allow prices to vary by education-experience group and time.

The shadow price estimates in chapter 5 show that wage differences between groups are affected both by higher prices for tasks disproportionately done by high skill workers and by higher pay for high skill workers within task categories. The results confirm other findings in the literature, particularly a rise in pay for nonroutine analytic tasks and a fall in pay for routine manual tasks. I further find that the introduction and expansion of computer tasks worked to counter the trend of a steepening experience-wage profile within education groups. Additionally, I find that differences in the shadow prices of tasks between states is in some cases substantial, which can provide useful identifying variation in estimating the determinates of task shadow prices.

In chapter 6, I model task shadow prices as a function of aggregate task quantities. This is where I reap the benefits of the work in chapters 4 and 5. With measured prices and quantities for tasks, I can estimate trends in task prices representing bias in technical change. A positive trend across all tasks for an education group indicates skill-biased technical change, while a positive trend across education groups within a task indicates task-biased technical change. This resolves the original question about what types of labor became more valuable and why in an accessible framework.

My general finding in chapter 6 is that task-biased technical change is an economically and statistically significant effect while pure skill-biased technical change is close to zero or not statistically significant after accounting for task quantities. I also find that a task-based nested CES based specification is a poor fit for most tasks, and that flexible estimation techniques are most appropriate, given a lack of clear alternatives in the literature.

CHAPTER II

ESTABLISHED EXPLANATIONS FOR CHANGING WAGE STRUCTURE

Over the past few decades, the wage structure of the labor force has changed substantially, largely in the favor of skilled workers. A major component of this shift is attributable to skill-biased technical change, as proposed in Bound and Johnson (1992). Given this name, it is natural to jump to a narrative in which the introduction of computers and communications technology raised the productivity of high skill workers and replaced routine work previously done by unskilled or low skill workers. That story may be true, but due to a lack of detailed information about the degree of computer use by individual workers and what else those workers were doing on the job, the literature has connected computer usage at work to changes in wages in only a few narrow contexts.

My purpose here is to draw on three previously unconnected strands of the labor economics literature to measure jobs tasks, including a measure of computer tasks that I introduce, and their wage effects in a common framework. I draw on the segments of the labor economics literature discussed below to establish a feasible methodology to establish what types of labor workers provide, how well they are compensated, and how different groups of workers affect each other's labor market outcomes.

The three elements of the framework I will rely on already exist, but in disjoint areas of the labor economics literature. The relevant areas are the cohort size effects literature, the wage effect of computers literature, and the task-based literature. The cohort size literature is centered around determining the effect of the entry of the unusually large baby boomer cohort on labor market outcomes, including both cross sectional and longitudinal experience-wage profiles, substitutability between experience groups, and in some cases education decisions. The literature measuring the effects of computers on wage establishes empirical methods to estimate how much computer usage increased wages for individual workers as computers were introduced to the workforce. Finally, the task-based literature is concerned with categorizing what specific types of labor workers perform at their jobs, in a way that makes many occupations comparable along a few dimensions, and the difference in wage and employment growth for workers doing different tasks. The remainder of this chapter is a review of these segments of the literature.

Cohort Size Effects Literature

Substantial discussion of the effects of cohort size on labor market outcomes began in the late 1970s and continued into the 1990s. Much of this endeavor was prompted by work by Richard Easterlin, who argued that immigration restrictions and other developments should cause cohort sizes to have larger economic impacts than in earlier periods, as summarized in Easterlin (1978). These arguments led directly to two foundational works on cohort size effect in Freeman (1979) and Welch (1979).

Foundational Work

Welch (1979) focuses on the effect of cohort size on longitudinal age experience profiles. Using Current Population Survey (CPS) data from 1967 to 1975 and a log-log regression, he finds a substantial negative effect of cohort size on wages for all education groups, especially in the early career phase. An important secondary finding was that the persistent component of the negative effect was smaller for workers with only a high school degree, at about 8% as opposed to 20%. The author estimates several permutations with differing sample restrictions and measures of wages. Later works refine these methods and generally support the conclusions.

Freeman (1979) again uses CPS data, but uses national averages from 1947 to 1974 to estimate the elasticity of substitution between experienced workers and new entrants by education group, and based on a regression of relative wages onto relative cohort size finds that workers with less education have higher substitutability between experience groups. Elaborating on this finding became an important topic in the subsequent literature. The author also improves on related work by including business cycle controls and testing for autocorrelation. That said, the results are based on few observations and some years are missing for some specifications of the model. Stapleton and Young (1988), addresses some statistical issues with the methods here, and chapter 3 offers a further discussion, replication, and extension.

Revisions and Refinements

Since this initial work, discussion on the topic flourished, and many authors introduced improvements, refinements, and new implications. Connelly (1986) takes on the important task of incorporating schooling choice into the model. The author accounts for what has been called the “flight to substitutability” wherein workers in large cohorts shift to the education group in which experienced and new workers are more substitutable and the effects of cohort size are smaller. This is a theory-focused paper, but the author shows that existing estimates can be used to find the degree to which education decisions changed. The author shows that this effect is sensitive to the time preferences of workers and that lower discount rates lead to larger reductions in education in response to large cohort sizes.

Stapleton and Young (1988) continue in the same vein as above, but with more empirical support. The authors document the fall in both college enrollment and the college degree premium in the 1970s and predict, correctly, the reversal in both trends in subsequent decades. The authors use CPS data and an adjusted form of the model from Freeman (1979) that better controls for differences in hours worked, and again find that high school degree workers have higher substitutability between experience groups. Based on their elasticity estimates, they simulate a baby boom to determine the adjustment to college enrollment that optimizes present value of lifetime income.

Bloom, Freeman, and Korenman (1988) build on prior work by modeling the effect of cohort size on expected wages, separating the effect on unemployment and the effect on the wages of the employed. The authors also estimate these effects on multiple countries including Australia, Canada, France, Japan, Sweden, and the UK, showing the general pattern of cohort size negatively affecting wages holds, but that the relative size of the unemployment and wage effects differ depending on labor market institutions. Relevant here is their finding on demographic shifts within industries from 1970 to 1980. The authors characterize this as an increase in the proportion of youth spread broadly across all industries. I interpret the values they report differently. My interpretation is that young workers entered industries that use low skill workers more intensively. The total increase in the proportion of young workers across all industries was 0.03. Industries such as construction, mining, retail trade, and personal services saw increases twice as large, while industries such as public administration, finance, investment, real estate, professional services saw no increase at all.

Berger (1989) contributes to the discussion on cohort size effects by incorporating position in the demographic cycle. That is, the cohort that precedes a larger cohort may be differently affected than one of equal size that follows the large cohort. Using CPS data from 1964 to 1984 and a quadratic in experience model of wages, the author finds that large adjacent cohorts have a negative effect on starting wages but cause steeper experience-wage profiles, which can be attributed to greater human capital investments. The implied experience wage profiles show that the pre-peak cohort does especially well late in their careers.

The cohort size literature discussed above will inform several of my methodological decisions, including data source selection and model specification. The CPS is the standard dataset used in this literature for good reason. The models require a large dataset with wages and employment, along with a variety of controls. Of the publicly available datasets fitting that description, the CPS is the only one covering the necessary period and at a high frequency. Based on the literature, the appropriate functional form has relative wages as the dependent variable and relative labor quantities as the key regressor if we want to estimate substitutability.

Wage Effects of Computers Literature

Given the time period covered in the literature discussed above, we might expect the adoption of computers to be important to labor market changes, and consequently to come up frequently in published work. In practice, computerization was difficult to incorporate into empirical work due to data unavailability. Typically, technology took the form of a parameter in the production function that may or may not drift over time, if it was identified at all. In some cases, computers could enter the production function as physical capital or R&D spending, but the first attempt to obtain wage effects for individual workers was only possible after the CPS added computer use at work as a variable in 1984.

Krueger and Criticisms

Krueger (1993) is the earliest example of estimating the effect of computer use at work on wages, but is limited by the data available at the time. The author uses a binary indicator of computer use and is limited to a regression with pooled cross sections of the October 1984 and 1989 CPS. Later work including DiNardo and Pischke (1997) show that a similar methodology leads to sizable estimates of wage premia for the use of pencils, calculators, and other office supplies. Most plausibly, more productive workers were the first to receive computers, so selection on unobservables led to upward bias in the estimate, with available control variables being inadequate as proxies. Later work, such as Autor, Katz, and Krueger (1997), supports the existence of wage premia for computer users, though how to estimate the premium remained disputed.

The problems with the empirical methods above stem from two underlying issues. First, markets were out of equilibrium in the sense that employers were bringing computers into the workplace without knowing which workers would see the highest productivity gains. Determining and accounting for the selection method employers use is certainly difficult and may not be feasible with available data. This problem should resolve itself over time. Currently almost all workers use a computer for work in some way and employers better know who gets productivity gains. Second, the coarseness of the data is limiting. We observe only whether a worker uses a computer or does not use a computer. In practice there is major variation in how much workers with computers use them. This problem can also be overcome with methods I discuss later in this chapter.

Later Work

Pabilonia and Zoghi (2005) marks a shift in the literature in part by reorienting the focus from computer use to computer skills and in part through improved identification techniques. Using matched employer-employee panel data from Canada and instrumenting for computer use with an indicator for recent changes in the workplace, the authors find that the direct effect of using a computer in any form on the job was small, but the effect of having experience with computers was moderate, which the authors interpret as a skill premium. The shift in focus from computer use to computer skills was partly due to theoretical considerations, but also by shifts in the labor market. Computers became less devices assigned to a specific workers and more ubiquitous fixtures of workplaces in which most workers had access, but used computers to varying degrees.

Another later example, less subject to the problems mentioned above, comes from Dickerson and Green (2004)[2], in which the authors use a British survey dataset and find that workers that used computers received a wage premium between 1997 and 2001, usually of between 10% and 20%. Their work is framed in the context of computer skills, though the underlying data is drawn from a question about the importance of the activity of using computers, rather than directly asking about the worker's skill. Rather than estimating education-experience groups separately, they use a quadratic in experience and dummy variables for education as controls. Another relevant finding is sizable differences in computer use within occupations. This is partially attributable to having coarsely defined occupations, with only 9 major occupational categories. This highlights the need for detailed occupation codes in the analysis.

Task-based Literature

The task-based literature contains a framework for analyzing the many types of labor that workers perform. This framework has influenced the literature on biased technical change, and shifted the discussion away from a productivity trend for skilled workers and towards a productivity trend for tasks that require skill. Incidentally, there are currently no examples in this framework that include computers as a type of task, and this framework is substantially different from approaching computers as physical capital or in terms of a skill that some workers have. I elaborate on the argument for a computer task category in chapter 4.

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[2]This was written contemporaneously with Pabilonia and Zoghi (2005), but had a shorter publication lag.

Theoretical Framework

The task-based literature stems from Gibbons and Waldman (2002), though the premise is much older. The authors here argue for a model of human capital that is specific to the tasks a worker does but portable from job to job. Essentially, a job is a bundle of tasks. This fact has many implications for our ability to disaggregate labor, wages, and human capital. A task has a marginal productivity and the component of wages derived from each task can be estimated, so long as we have data on task quantities.

Empirical Framework

The following literature establishes how to categorize and measure tasks, and in some cases how to estimate shadow prices for tasks based on their marginal productivity. An important property of this framework is that tasks are features of the job, not the worker, in contrast to skills. If we know a worker's occupation, we have a clear indication of the tasks a worker does, though we could miss some variation between workers within an occupation. In a sense, what this approach does is take a high-dimensional space of occupations and project it onto a small-dimensional space of tasks, so we have a few types of labor and we can compare workers in different occupations.

Perhaps the broadest discussion of this framework is found in Acemoglu and Autor (2011), who examine changes in the U.S. wage structure since the mid-1960s using a variety of datasets, and comparing workers based on education, experience, gender, and occupation class. The authors describe the standard schemes for categorizing tasks. In the standard approach, tasks are sorted into routine and non-routine, or into analytic (or abstract), manual, and communication (or interactive). When the schemes are combined, communication is generally considered non-routine, leading to a suite of five task categories. The paper goes on to discuss differences in wage growth by education and experience group, but for my purposes the most relevant subjects they discuss are their canonical model of wages, based on a nested constant elasticity of substitution (CES) production function, and their methods of measuring tasks empirically. Their general finding is that wages in occupations intensive in non-routine analytic tasks have risen dramatically, while wages in occupations intensive in routine tasks have stagnated, with analogous findings for employment growth.

The nested CES has been a valuable tool in specifying wage equations, and I discuss the model in detail in chapter 5, when I estimate shadow prices in a hedonic model of wages. The basic idea is that the labor input within the standard CES production function is itself a CES whose inputs are types of labor, and those labor inputs may also be in the form of a CES production function of increasingly specific types of labor. This has been applied elsewhere, such as Goldin and Katz (2007), though not always by name. Ottaviano and Peri (2012) discusses how to specify the nesting structure and how to compare alternative nesting structures. The nesting structure informs us of which worker types should be compared in regressions modeling relative wages.

The empirical approach to task quantities detailed in Acemoglu and Autor (2011) makes use of the Occupational Information Network (O*NET). The O*NET dataset is widely used and important in the task-based literature, and I describe it in more detail in chapter 4. The basic premise is that for each task category, several variables from the

O*NET dataset are selected and combined into a single variable rating each task. The properties of this new variable depend on the construction method.

Acemoglu and Autor (2011) construct task variables by standardizing the O*NET variables, summing them within categories, and standardizing the sum to have a mean of zero and standard deviation of one. This method yields an intuitive rating for the intensity of tasks within occupations, but is not a task quantity that we should be comfortable aggregating since by construction it sums to 0 over the population. Where an aggregate is required, this method can be extended by identifying occupations that are highly focused on one task and summing the number of workers in those occupations, as shown in Autor and Dorn (2008), Autor and Dorn (2009) and Autor and Dorn (2013) in which the authors examine the rise in the share of low skill service sector jobs, termed the routine share of labor. We should expect this extension to work best when there are few task categories. Tasks often performed at low intensity will not be fully captured by this method since occupations are sorted based on their most intensive task.

While the method of aggregation above is the most used, one alternative is applied in Peri and Sparber (2009). The authors look for evidence that an increase in immigration within states leads low skill native workers to increasingly specialize in communication tasks, while immigrants specialize in manual tasks. They use O*NET and decennial census data over the period 1960 to 2000, but to aggregate tasks across workers they suppose a distribution of task quantities for the population and match percentiles in the O*NET data to that distribution. This method is more able to cope with more task categories and captures more of the tasks being performed at low intensity. The downside is the need to assume the distribution of tasks in the population, and theory gives little guidance on what the distribution of tasks in the population should be, apart from bounded below by 0 and above by some finite number. The authors go on to estimate task-specific wages via a hedonic model on individual workers. Based on changes in task-specific wages and the quantities of each task performed by natives and immigrants, the authors find that natives adjust to increased immigration by switching to communications tasks, and consequently the total effect on wages for low skill natives was modest.

Applications

More generally, the task-based framework has been used in the literature on inequality and often in the literature on immigration. For examples of empirical work, see Goos et al (2009), and Crinó (2010) on offshorability, Peri and Sparber (2009), and Haas Lucht and Schanne (2013) on immigration, Black and Spitz-Oener (2010) on the gender pay gap, and Firpo, Fortin, and Lemeuix (2011) and Scotese (2012) for a more general treatment. An important branch of the literature discusses how and why productivity trends differ by task (i.e., task-biased technical change), including Autor, Levy, and Murnane (2003). We can describe biased technical change among tasks in a similar framework to skill-biased technical change, as seen in Adermon and Gustavsson (2015), who find different trends in productivity for the tasks workers perform.

In fact, Adermon and Gustavsson (2015) is part of a general trend moving away from the original formulation of skill-biased technical change as an effect on high skill workers and towards a view of biased technical change as affecting nonroutine tasks that high skill workers perform disproportionally. See Böhm (2020) for further discussion of this shift, and an alternative empirical approach to pricing task labor inputs.

Empirical work in the task-based framework tends to involve heavily processing what raw data is available, so a major takeaway from the task-based literature is which data sources to use and how, in order to avoid the problems of branching paths[3]. For analyzing the United States workforce, O*NET is the clear frontrunner, despite its limitations. The literature offers much guidance on variable selection and construction using O*NET data. A second valuable takeaway is how to categorize workers into groups when estimating wage effects. The literature suggests a nesting structure of education-task-experience, as stated in Haas, Lucht, and Schanne (2013). This means that I can estimate task-specific wages within education-experience groups and compare relative task wages and relative task labor quantities within experience groups while keeping to a simple linearizable regression model. Detailed rationale for the regression specification is in chapter 5.

The task-based literature is less clear about whether and how to organize the data geographically. Peri and Sparber (2009) aggregates labor quantities within states. Most other research is focused on national trends, making geographic differences less relevant. A key benefit of obtaining state or local level aggregates is the increased identifying variation of a panel dataset, which is especially important if we suspect parameters can

[3]Sometimes called forking paths or researcher degrees of freedom, this problem occurs when there are a large number of seemingly arbitrary choices regarding data transformations, model specifications, etc.

vary over time. Additionally, geographic variation is plausibly large, and is of interest for purposes other than my own.

Absent from the literature is a discussion of work done with computers in a task-based framework. There are occasional mentions of computers in the theory sections of papers as a form of capital that can substitute for tasks, as in Acemoglu and Autor (2011) and Autor, Levy and Murnane (2003), or as a binary control variable in some specifications, as in Peri and Sparber (2009). Scotese (2012) skirts the issue by comparing trends in occupations whose tasks could plausibly be replaced by computers with occupations whose tasks could not plausibly be replaced by computers, by the author's assessment. Nowhere in the literature are computer tasks treated as a task category, although there are relevant variables in the O*NET dataset. I give arguments for why this is a viable approach and solves some problems with measuring computer use in chapter 4.

A final methodological issue I have yet to mention is the use of hedonic modelling. This is applied in the task-based literature and elsewhere and on the surface is simple. Wages are determined by the features of the job (or worker), which in this case are the task quantities that the worker provides, based on the worker's occupation. Peri and Sparber (2009) do this to calculate task-specific wages for native and immigrant workers. Firpo, Fortin, and Lemieux (2011) implicitly work in a hedonic framework, though they use an unusual suite of task categories and are ultimately focused on decomposing changes in the wage structure, rather than directly claiming to find task-specific wages. They use CPS and O*NET data to argue that changes in task composition had a large impact on within occupation wage structure. Notable is that the wage effects are in rare cases negative.

To understand this result, we should recall that once wages are decomposed, some of the usual assumptions no longer hold. Specifically, if labor is undifferentiated, employers can prevent workers from overcrowding by having superfluous workers stay home, so marginal product cannot fall below zero. When workers provide bundles of tasks, employers may end up with excess labor in some tasks because the marginal worker has positive productivity even if one of the tasks that worker performs does not. In some cases, a task may also be treated as an amenity, or performing a task may involve learning on the job such that productivity rises in later time periods and current productivity below zero is rational. Any of these cases should be the exception rather than the rule if tasks are categorized properly, but Firpo, Fortin, and Lemieux (2011) shows that we should not be surprised by the occasional appearance of negative wage effects for tasks.

Summary

Over the past four or five decades, wages for skilled workers increased disproportionately. Much of this trend appears to be related to technology, plausibly driven by the computerization of the workforce. Initially, the productivity trend seemed to affect workers based directly on their skill level, but more recent work suggests that some tasks benefitted more than others and the fact that the trend benefitted skilled workers was in a sense coincidental.

Omissions

The key gaps in the literature are several. We lack a continuous measure of computer usage at the individual level. We have not resolved ambiguity about how computers complement and substitute for various types of labor. To elaborate, if computers substitute for routine tasks and complement nonroutine tasks, is this at the individual worker level or in the aggregate? Have routine tasks been shifted between occupations in response to computerization? Has the introduction of computers changed how well experienced and inexperienced workers substitute for one another? Do we have reason to expect the substitutability between worker groups should remain constant over time, as frequently assumed for the purpose of identification? The results in chapters 3 to 6 will shed light on these questions.

Contributions

In terms of what this dissertation adds that the prior literature is lacking, there are two main contributions. The first is a more comprehensive measurement of tasks. In addition to adding computers as a task category, I add to previous work by providing a method in which tasks in a broad suite are aggregable. Earlier methods could calculate a "routine share of labor" or quantities for two tasks, rather than aggregate quantities for many tasks. The second is a clearer estimate of bias in technical change. Once tasks are measured and priced, estimating trends and testing differences in them will reveal whether productivity gains are higher for particular tasks or particular groups of workers.

CHAPTER III

EARLY FINDINGS REVISITED

In this chapter, I revisit part of the cohort size literature to show how the general approach should extend to a task-based framework, while I identify omissions from the literature that can be addressed when labor quantities have been decomposed into tasks. The cohort size literature traces back largely to a few papers in the late 1970s. One of the foundational papers in this literature is Freeman (1979), which examines the effect of relative cohort size on wages at a time when the leading edge of the baby boom was entering the labor market. Freeman found that cohort size had a substantial impact on relative wages. His results implied an elasticity of substitution of 2 for college graduates and higher for workers with only a high school degree. Below, I replicate this result for the period Freeman considered using microdata and re-estimate the model on later time periods up to 2017 to check the stability of the relationship.

As a little background, two factors make the decades following the 1960s amenable to the study of the effects of cohort size in the United States. First, immigration policy at the time was relatively restrictive, reducing endogeneity concerns. Second, the large increase in birth rates through the 1950s led to unusually high variation in cohort sizes for the next few decades. This can be seen in Table 1 and in Figure 2. Generally, relative cohort sizes were much more stable in the periods after the baby boomers entered late working age. All else equal, this increased variation should make estimates more

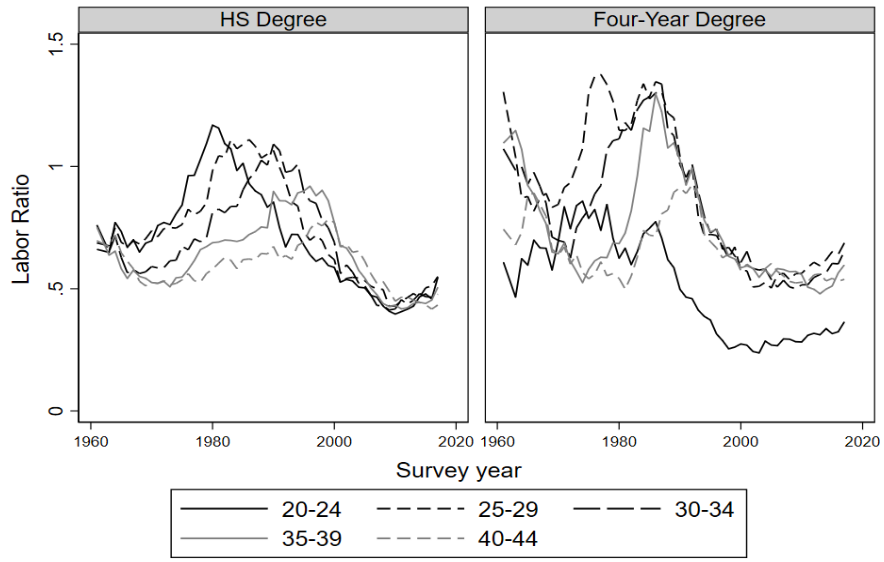

Figure 2. Relative Cohort Sizes

Table 1. Relative Cohort Sizes over Time

	20-24	25-34	35-44	45-54	55-64
1965	0.118	0.211	0.230	0.205	0.155
1975	0.146**	0.243*	0.182	0.187	0.146
1985	0.142	0.284**	0.212*	0.148	0.142
1995	0.114	0.256	0.259**	0.184*	0.121
2005	0.114	0.215	0.231	0.222**	0.154*
2015	0.113	0.218	0.199	0.210	0.196**
Notes: Fraction of male workers ages 18-65. *Early Boomers (1950) in cohort, **Peak Boomers (1955) in cohort, data from March CPS					

precise, though other factors specific to this period such as the draft may complicate the analysis. These and other factors are discussed in a series of essays by Richard Easterlin in the 1950s and 1960s. Empirical work on the subject was scant until Freeman (1979) and Welch 1979 looked at the effect of cohort sizes on wages, at a time when the leading cohort of baby boomers could be observed entering the labor market. These papers spawned a substantial literature on cohort size that improved both in terms of model specification and data quality.

These later papers elaborated on Freeman's work by looking at changes in a variety of labor market outcomes and particularly at estimates of the age-earnings profile. These include Berger (1989), which demonstrated the importance of adjacent cohort sizes, and Bloom, Freeman, and Korenman (1988), which demonstrated that much of the effect on wages was due to changes in unemployment. In general, attention shifted from relative cohort size to own and adjacent cohort sizes and how they influenced, for example, returns to education. In that sense, the work was absorbed into other literatures and after the 1990s was rarely a primary topic. Later work in the literature also struggled with efforts to separately identify supply and demand shifts caused by cohort sizes and did not reach a consensus on how to do so empirically.

While these offered valuable new insights, Freeman's initial framework offered a benefit that has received less attention. Freeman (1979) estimates the elasticity of substitution (or complementarity), allowing a look at how older and younger workers differ as inputs into a production function, as well as a trend in labor demand. With his approach, we can ignore issues such as the functional form of the age-earnings profile.

This substitutability interpretation was not Freeman's primary aim but is especially useful in determining how and why outcomes change and sidesteps much of the complexity in modelling other outcomes. In the literature, there is a strong tendency to treat job attributes as given and explain productivity differences based on worker characteristics. This story is incomplete if changes in substitutability arise from changes in the production function as firms adapt to demographic and technological shifts.

Below, I replicate the results in Freeman (1979) using microdata and estimate the model on a longer sample period. Freeman draws from several sources, most of which are based on Current Population Survey results. This leaves the degree to which results are sensitive to sample restrictions, the choice of age brackets, and the years for which data were available unclear. Freeman consequently estimates different models on different time periods due to data limitations, and their results may or may not be comparable.

Theory

While the idea that a worker's productivity, hence wages, should rise as human capital is accumulated through work experience is obvious, less obvious is that the worker's productivity should change in response to the increase in experience of other workers. If we think of workers as offering differentiable labor inputs, this makes sense. Some groups of workers offer more scarce or plentiful types of labor. To oversimplify a little, we may think of young workers providing low skill, physically intensive labor and old workers providing high skill, low physicality labor, with middle aged workers providing a degree of both. The ability to replace brute strength with expertise is limited. It may also be the case the older and younger workers are not equally substitutable for capital inputs, though data limitations make this more difficult to see in practice.

The concept of substitutability and the methods of measuring it are well-known but bear elaboration in this context. Inputs are substitutable if when one input is reduced output can be maintained at a constant level by increasing another input. If the amount of the second input needed is a constant multiple of the reduction in the first input, those inputs are perfect substitutes. Note that the multiple need not be one; if we need two packets of Sweet & Low to replace one packet of Equal, they are still perfect substitutes. If the amount of the second input needed rises the more the first input is reduced, they are imperfect substitutes.

Figure 3 gives an illustration of the substitutability between inputs in the form of isoquants. Each line represents a fixed level of output and contains all the combinations of two labor types that could produce them. Generally, we expect them to be curved inwards as seen here. If the isoquant is perfectly straight, the inputs are prefect substitutes. If the isoquant forms a right angle, the inputs are perfect complements. What point on the isoquant is actually used in production generally depends on the prices of the inputs if we are thinking in a micro framework where this is the production function of a price taking firm. In other contexts, we may need to think of prices as adjusting to relatively fixed quantities. In either case, the slope of the isoquant indicates the relative marginal productivities of the workers. If type 2 is abundant, type 1 is relatively more productive, as indicated by the steeper slope.

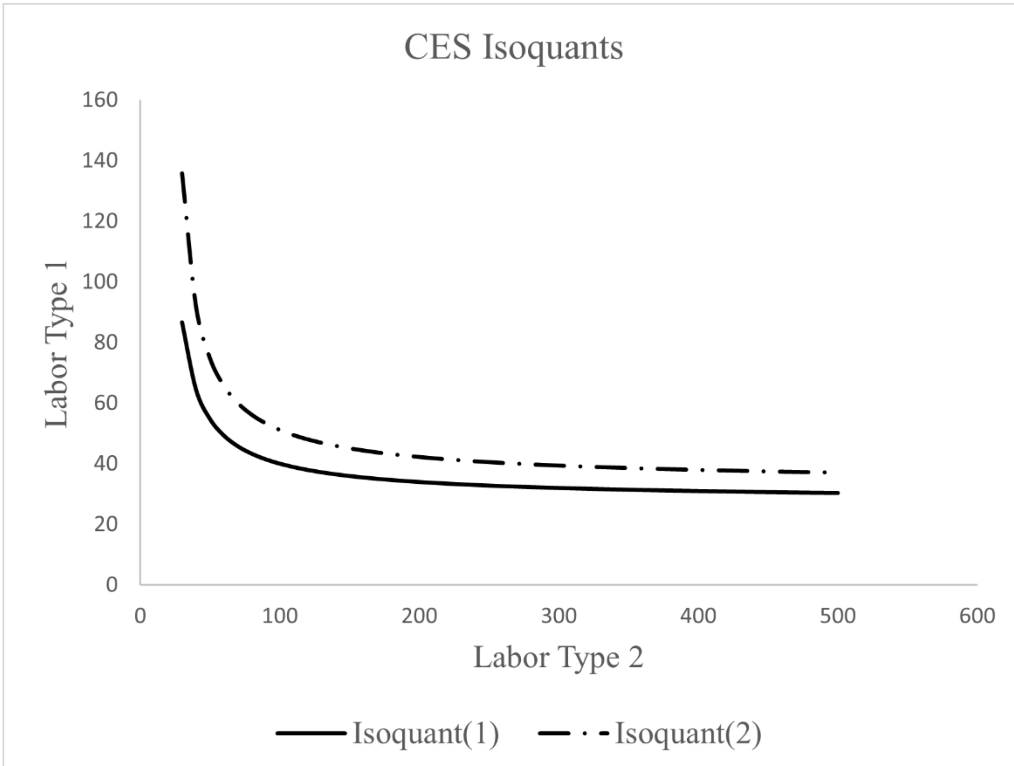

Figure 3. Isoquants, CES Production Function

In the context of labor, it is helpful to think of each worker as having a set of abilities that can change over time. If all these abilities rose and fell at the same rate over each worker's lifetime, workers of different ages should be perfect substitutes, since a fixed number of young workers could always replace a fixed number of older workers and we could normalize labor input measurements for age[4]. Since it is not plausible that a worker's physical and mental abilities change at the same rate over a lifetime, we should expect that older and younger workers be imperfect substitutes. That is, older and

[4]For a somewhat impolite example, consider dairy cows. A dairy cow's milk production rises over her lifetime, but cows of all ages are perfect substitutes. Human examples where workers produce an undifferentiated output, cannot change careers, and do not collaborate are hard to come by.

younger workers will specialize in different tasks, and as younger workers hypothetically become scarce, older workers will be used for tasks for which they are less specialized, requiring more of them to do the same work.

Workers of different ages differ in several ways, some of which are easier to observe than others. Older workers tend to be paid higher wages. The usual argument is that their wages reflect higher human capital accumulated through job experience. The degree to which this is the case varies by group. For more educated workers, the wage increase is large, while for workers with less than a four-year degree the difference is modest. This is generally interpreted as more educated workers having higher returns to experience. There are other situations in which higher wages for older workers arise from strategic behavior rather than productivity differences, but these mostly require what we would now consider age discrimination to work, and are implausible following legal prohibitions introduced in the 1970s and 1980s.

If the usual assumptions about competitive markets and perfect information hold, wages reflect marginal productivity, and we can interpret the wage ratio of older and younger workers as the marginal rate of technical substitution (MRTS) between them as inputs[5]. This is effectively the variable the models will describe. Further interpretation in terms of elasticities of substitution depend on assumptions about the production function. We should expect the MRTS to fall as the reference input becomes more common. That is, as younger workers become more prevalent, it should take fewer older workers to

[5]This assumption can be relaxed somewhat, as discussed later

match the output of each younger worker. Deviations from this should be surprising, and subject to careful interpretation. An input becoming relatively more productive as it becomes relatively more common is atypical.

A natural way to characterize the relationship between older and younger workers as inputs for production is using the elasticity of substitution. If there were fewer young workers, more older workers would be necessary to maintain a given level of output. If younger workers are group 1 and older workers are group 2, to express this in percentages, we use the formula

$$\sigma_{12}=dln(x_2/x_1)/dln(MRTS_{12})$$

Equation 1

which is conveniently in terms of variables we can find in publicly available datasets. Our need to estimate this will guide our model selection. We prefer that σ_{12} be a constant, but one that depends on the data, as opposed to the Cobb-Douglas case in which it is always one. Note that σ_{12} need not necessarily be a constant. Treating it as a constant is typically based on the assumption that the production function has a particular form, though it may be possible to estimate some kind of average σ_{12} or to make it constant within some subsample.

Note also that whether the elasticity of substitution is measurable in the data depends on labor mobility. If labor moves freely, the proportion of workers in a given age bracket is endogenous as workers go to the area with the best wages. The labor market must be defined such that workers are not migrating. As noted elsewhere, this means that relative cohort sizes should become relevant in the United States after immigration became heavily restricted in the 20th century. This line of reasoning also supports aggregating at the national level as Freeman does in his models.

From the reasoning above, the elasticity of substitution between older and younger workers should be higher for workers in fields where experience is a major factor in productivity. The easiest test case is to compare workers by education group. We should also expect the elasticity to be higher between more distant age groups. This leaves us with two sets of hypotheses to test.

Model

In practice, what we can observe are wages and some worker characteristics. Consequently, the equation we can establish empirically is a labor demand equation. Freeman and others assume inelastic labor supply in arguing that the linear model estimates the labor demand equation. Going from the demand equation to a claim about substitutability between inputs requires further assumptions. The benchmark case used in Freeman (1979) and elsewhere is a constant elasticity of substitution (CES) production function.

If we assert that aggregate output is

$$Q = F(al_1^{\rho}+(1-a)l_2^{\rho})^{1/\rho}$$

Equation 2

where l_1 and l_2 are the number of workers in different age brackets, F is total factor productivity, and a is relative productivity. The elasticity of substitution between labor inputs is $\sigma = 1/(1-\rho)$. It can also be shown that in equilibrium,

$$MP_1/MP_2 = (a/(1-a))(l_1/l_2)^{\rho-1}.$$

Equation 3

With some minor rearranging, this yields the convenient specification

$$ln(w_1)- ln(w_1) = ln(a/(1-a))-(1/\sigma)(ln(l_1)- ln(l_2))-v,$$

Equation 4

where w_1 and w_2 are wages and v is whatever deviation relative wages have from relative productivity. If we wish, we can eliminate the constant term by taking differences, or if we suspect it varies over time, we can think of them as relative productivity (demand) shocks. The advantage of the CES framework here is that elasticity is a constant, but the model allows parameters for total factor productivity, relative productivity, and returns to scale to change. The most obvious concern is that a may vary over time and that v may be correlated with labor shares.

Freeman briefly discusses the strengths and weaknesses of this specification. The primary benefit is that the elasticity of substitution can be estimated in a linear framework with available data. Notable limitations are that we require that σ be unrelated to other factors, especially capital intensity and labor in other age brackets.

The decision to estimate the model on time series data effectively trades power in some statistical tests in order to solve the problem of workers migrating endogenously. At the national level, for the time period in question, migration can be ignored. This is a key point argued in early work by Easterlin (1978). Aggregating at a subnational level would force endogenous migration to be addressed by some more complicated method. The downsides are that estimates are based on far fewer observations and that parameters must be assumed constant over at least some time period. In principle, it should be possible to test the CES-based specification by including other cohort sizes and other transformations of own cohort size. In practice, these tests are low power both due to small sample sizes and to partial collinearity between the variables increasing the standard errors.

We may also wish to consider other forms of endogeneity that may or may not be solved using time series data. Wages are an imperfect measure of marginal productivity in several ways. Older workers may receive alternative compensation in the form of health care benefits to a greater degree than younger workers. Suppose the true MRTS is given by $w_1/(w_2+h)$, where h is the additional compensation for older workers. Then

$$ln(w_1/(w_2+h)) = ln(w_1/w_2) + ln(w_2/(w_2+h)).$$

Equation 5

We can subtract the second term from both sides of the regression equation and think of this as an omitted variable. In that case, we need to be concerned if the proportion of wages to total compensation for older workers is correlated with the proportion of older workers in the population. It will almost certainly be the case that these are correlated over time, if for no other reason than that they have both risen in the previous decade. If the trend is linear, this will be captured by the time trend, so the trend should be included. Similar arguments could be made for other deviations of wages from marginal productivity, such as deferred compensation, though in many cases the notation becomes complicated. Freeman implicitly makes this case when arguing for a labor market condition proxy. Additional proxies may also have been appropriate.

While the CES model is helpful in specifying the equation we wish to estimate in terms of how the variables should be transformed, it is less informative as to which control variables should be included. Freeman expresses concern that business cycles affect workers differently depending on their age brackets, such as older workers being less likely to experience layoffs in a downturn. This concern is well supported, especially early in the period when strict seniority rules were more common. To deal with this, Freeman uses detrended GNP to control for the current phase of the business cycle. Thus, the full equation Freeman estimates is

$$ln(w_1)- ln(w_2) = \beta_0+\beta_1 (ln(l_1)- ln(l_1))+\beta_2\, \varepsilon_{GNP}+\beta_3 t+\varepsilon.$$

Equation 6

Freeman defines l_2 and w_2 using workers ages 45 to 54, which are typically the peak earning years. For l_1 and w_1 he uses workers 25 to 34 and 20 to 24 in separate regressions. If our specification is correct, a 1% increase in the number of workers in the lower age bracket relative to the number of workers in the higher age bracket will induce a decrease in wages of $\beta_1 = -(1/\sigma)\%$ for the younger workers relative to the older workers. Keeping in the framework of CES production, $\rho = \beta_1+1$, so 0 indicates linear production, -1 indicates Cobb-Douglas, and the Leontief production function is at negative infinity. Whether β_1 is above or below -1 will indicate whether the goods are gross complements or gross substitutes.

Data

The principal source of data is the Current Population Survey conducted by the US Census Bureau on behalf of the Bureau of Labor Statistics. Though conducted each month, the March supplement (ASEC) focuses on the economic variables most relevant here. Microdata for the March CPS is available from 1962 onwards, with data summaries available for earlier years. This is the most often used dataset in the cohort size literature. The advantages of the CPS are its large sample sizes and its wide selection of variables. Since the survey targets a number of households each year, the number of individual observations rises and falls along with average household sizes but tends to be between 150 and 200 thousand a year. The limitations of the CPS are the measurement error to be expected from self-reported data, the changes in data collection practices over time, the paucity of a personal history variables.

At the time, Freeman would have had access to some microdata on magnetic tape, but frequently used results from data summaries rather than the microdata. This allowed him to include results for years as early as 1947 but limited his analysis in other ways. In particular, he is unable to freely set age brackets and some sample restrictions are difficult to discern. He also imputes values for both dependent and independent variables in some years when data were unavailable. The replication will be based entirely on the available microdata, and consequently starts in 1961[6]. Since Freeman already changes sample periods from model to model based on data availability, modest shifts in the sample period are not a major concern for the fidelity of the replication.

The income data in the CPS has a complicated history that requires discussion. Income is recorded at both the household and individual level and is broken into categories including wages and business income. For examinations of labor demand, using nonfarm wage income is a common approach, and I follow Freeman in using it. This means that we exclude income for the self-employed and farmers. Additionally, for early years in the sample period values for the wage variable include unflagged imputations. While not explicitly stated in Freeman (1979), based on conversations mentioned in Welch (1979), Freeman addresses this by excluding observations with flagged imputations at the household level.

There are two further issues with the wage data in the March CPS. First, it is subject to changing top codes over the years, from as low as $50,000 in the 1970s to as high as $200,000 in 1995. From 1996 onwards, the March CPS uses a variety of complicated methods to anonymize observations with high incomes, including imputing the mean of wages above the top code and switching the wage values of high earners using a matching system. In many cases, this can be ignored, but we should be aware that the top code should have a larger effect on mean wage estimates for workers in higher

[6]The survey was conducted in 1962, but the data is over the previous year

age brackets. When examining relative wages as Freeman does, this would lead to an upwards bias in our estimates that is larger for years with a lower top code. Second, the CPS underwent a redesign in 1994 in which the wage and employment questions were changed to reduce measurement error. The attempt seems to have been successful, so we should be wary of differing measurement error before and after 1994 and check for structural breaks around that time.

The wage data in the March CPS is annualized, but it is arguably better to use hourly or weekly wages in some specifications, as Freeman sometimes does. Since the March CPS also reports usual hours worked per week and weeks worked in the past year, we can impute these values. Doing so may exacerbate measurement error. Another issue is that usual hours worked was not included before 1982, and prior to 1976 weeks worked was reported only up to an interval. Where necessary, these values are imputed as the midpoint of the interval.

The other key variable is more straightforward. The CPS includes age at the time the survey was taken. Since wages are reported for the previous year, we subtract one from the age reported in the CPS. In principle, there is some degree of measurement error here, but sorting workers into multiyear brackets should reduce this. Freeman tries several approaches to selecting age brackets, with his key model using the 45- to 54-year-old bracket and the 25- to 34- year-old bracket. The ratio of men in these brackets is the main explanatory variable. Later work in the literature favored using experience brackets rather than age.

Table 1 reports relative cohort sizes for the male workforce since 1965. We can see a few trends over time. The current age distribution is more uniform than in the early periods. We can also see the baby boom pass through the age brackets, ballooning cohort sizes by as much as 35% in some cases. The generations that followed tended also to be large, but the relative cohort sizes are much more stable. Note the sizes of cohorts two brackets behind the peak.

Freeman uses a few other variables in the CPS to restrict the samples he uses to estimate his models. As the degree of substitutability between workers differs by education level, Freeman does estimation on high school and college graduates separately. The CPS provides detailed data on education levels, but some caution is needed as prior to 1992 the education variable did not distinguish between spending 4+ years in college and receiving a bachelor's degree. Another problem is the lack of education data in the 1963 survey. It also had a "12th grade, diploma unclear" value. Following Freeman, these observations are included as graduates, which seems reasonable given that superseniority was much less common at the time. Freeman also restricts some samples to year-round full-time workers. Which variables were used to do this are unstated, as Freeman draws values for some years from summaries of CPS data in Current Population Reports (CPR). Here, I use the FULLPART variable, used to indicate full-time or part-time work status. Using the interval weeks worked variable is also possible.

In addition to the variables obtained in the CPS, Freeman uses detrended log GNP (residuals around a time trend estimated by OLS) as a control variable. He does not mention the source of GNP data, but it can be easily obtained through FRED or directly from the BEA. A greater concern is that while a linear trend may be a sensible approximation in the period Freeman considered, it is less appropriate for a longer interval. Detrending from a linear approximation of GNP from 1961 to 2017 will give negative values at the beginning and end of the period and positive values in the middle, in no way capturing periods of recession and growth as intended. I use separate linear detrending for the pre- and post-1975 period in an effort to maintain comparability.

Figure 4 shows the natural log of GNP for the early years of the sample. Here, the linear trend is a good fit, although the residuals show a clear pattern. Our goal is not white noise errors, but this measure suggests that 1969 to 1976 were all weak years. Longer time periods increase the degree of the problem. Other work tends to use unemployment rates to capture business cycle effects. As an alternative, the cyclical component of log GNP derived from a Hodrick-Prescott filter is show below. This method is far less sensitive to the time period of the sample and shows more plausible variation over time.

Assessment

When replicating a result, carefully defining the criteria for success or failure in advance is critical. I attempt both to replicate Freeman's results for the period he examined and to estimate the model on an extended time period to see if the results change. Because of data limitations, we should not expect results to match Freeman's estimates exactly in either case. Recall that many of Freeman's results are based on

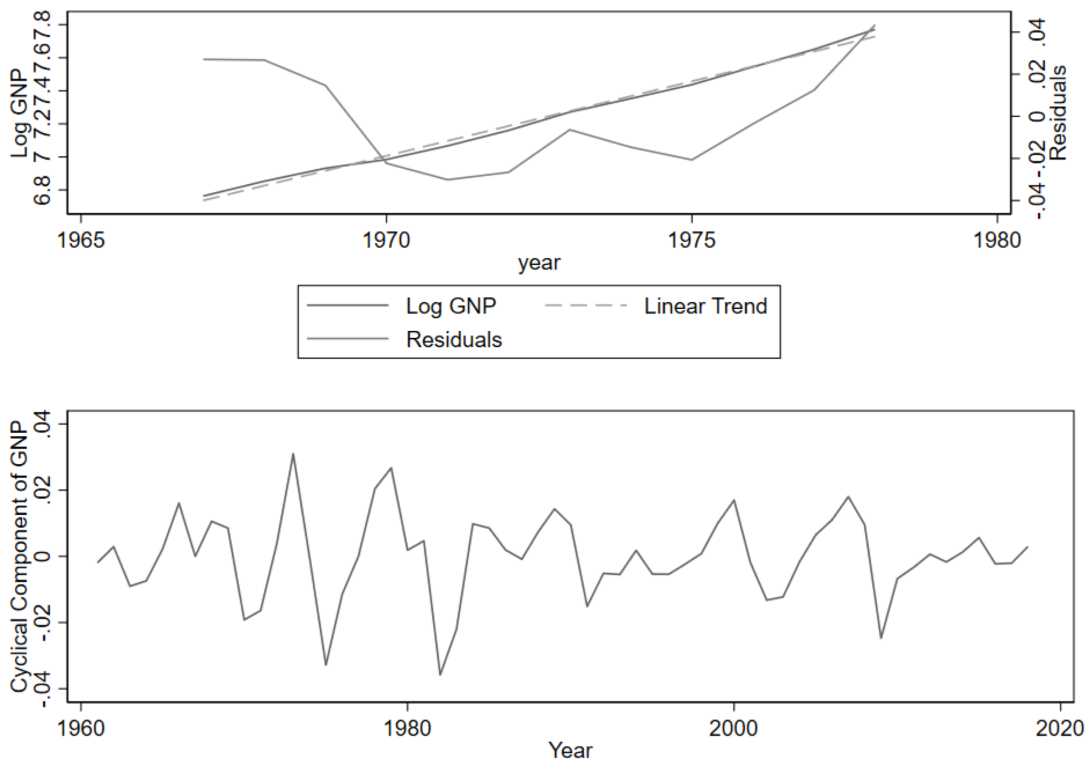

Figure 4. Detrended GNP

summaries in the Consumer Survey Reports and that the source of some of his variables (e.g. GNP) were not clearly reported.

Freeman reports his key results in table 4 of his paper, which contains coefficient estimates from a regression on time series data from 1955 to 1974[7]. We are concerned with the coefficient on the log ratio of workers in younger and older age brackets, and to a lesser extent with the time trend. The other variables are controls, not of direct interest. If older and younger workers are gross substitutes, in all cases the elasticity of

[7]Some models are estimated from 1947 to 1974, but these use different sample restrictions on workers

substitution is greater than 1. Thus, our first criterion is that the coefficients remain between -1 and 0.

A second key result is that the coefficient for college graduates is of greater magnitude than the coefficient for workers with only a high school degree. This is what theory predicts and Freeman spends some time interpreting it. So, our second criterion is that $\beta_{1,col} < \beta_{1,hs}$.

A third key result is that the demographic shift is responsible for a large portion of the total variation in relative wages. For the period Freeman considers, he finds roughly half of the change in relative wages can be attributed to changes in relative cohort size. In the replication, this result should hold for Freeman's sample period. In later periods, relative cohort size may explain less of the variation in relative wages, but should account for a similar proportion of the variance in the fitted values. Failure to meet any one of the three criteria will indicate a failed replication attempt.

Assuming the coefficient values match, we need also to establish the statistical significance of the results. Freeman reports standard errors but does not mention whether they are classic OLS or something more robust. He also estimates many coefficients, so the fact that some are significant can be overinterpreted. Additionally, he cannot calculate Durbin Watson statistics for his two key models. By using the CPS data directly in our estimations and robust standard error estimates, these issues can be remedied. If the results of our estimation meet the three criteria above but are not statistically significant, this should be taken as a partial failure to replicate.

Another concern is that Freeman's results may be sensitive to his somewhat arbitrary choice of age brackets. Freeman seems to base his choice of age brackets on what the CPR and other sources used in their summaries rather than having a theory-based argument. With the CPS data available, we can shift age brackets a small amount to test the sensitivity of the results. If moving the age brackets by a year or two leads to large changes in the coefficient estimates, that may indicate that Freeman's results are driven by some cohort specific effects rather than cohort size, which again would represent a failed replication.

Results

The estimates for the key coefficient are reported in two tables. Table 2 follows Freeman's model closely. The results in the pre-1975 period are similar to Freeman's, insofar as that the signs match and the absolute value of the coefficient for college graduates is larger than the coefficient for high school graduates. The results for the youngest cohort of full-time workers are noticeably different, though Freeman's estimate may be imprecise. While not visible in the table, it should be noted that robust standard errors for these coefficients are smaller than the classic OLS standard errors, so standard errors may have been overstated. All told, the first and second criteria for replication are satisfied, as all estimates are between 0 and -1 and college graduates have a more negative coefficient. The third criterion is more debatable, but it appears that variation in cohort size is still the primary driver of changes in predicted relative wages, rather than the other independent variables, with youngest cohort as an exception. By the standards above, the replication is successful.

Table 2. Elasticity of Complementarity Estimates, Basic Model

Sample	Freeman	Basic Model Replication Pre 1975	1975 to 1996	1996 to 2017	p-value (=)	D-W
Full-time Workers, 25 to 35	-0.14** (0.04)	-0.13 (0.24)	-0.09 (0.05)	-0.13* (0.07)	0.86	1.10
Four-year Degree Workers	-0.51* (0.28)	-0.30 (0.21)	-0.04 (0.09)	0.21** (0.08)	0.03	1.87
High School Graduate Workers	-0.07 (.11)	-0.15 (0.20)	-0.20** (0.04)	0.12** (0.05)	0.00	1.54
Full-time Workers, 20 to 24	-0.41** (0.19)	-0.49** (0.15)	-0.06 (0.07)	0.37 (0.24)	0.01	1.01

Notes: *-5% significance, **-1% significance. Replicated results were estimated jointly, interacting with a dummy variable for the post 1974 period. Standard errors are heteroskedasticity robust. Lower critical value on DW is ~0.94, upper critical value is ~1.95. The test is inconclusive in all cases.

Comparing the pre- and post-1976 trends, the coefficients are statistically significantly different for both high school and college graduates. Recall that baby boomers began entering the 25 to 34 age bracket in 1975 and began to enter the 45 to 54 age bracket in 1995. If the estimates are true, the elasticity of substitution between young and old college graduates rose following the period Freeman examines while it went down for high school graduates. I should additionally point out that positive values should be disquieting. Recall that the specification implies $\sigma = -(1/\beta_1)\%$. This makes 0 a vertical asymptote, and a positive coefficient is not possible in the CES framework. For now, I suppose the true coefficient is nonpositive when interpreting.

Also worth mentioning is the value of the Durbin-Watson statistics. Freeman finds values near 2 in most of his models, but estimating over a longer time period gives results that more strongly suggest either autocorrelated errors or some form of endogeneity. The results suggest that ARMA type models should be explored. Estimates using cohort bins shifted by one year in either direction did not yield statistically significantly different results. Results are sensitive to the choice of detrending method of GNP. This should not discredit the model, but is concerning and suggests that the model would benefit from more robust controls for labor market conditions. Overall goodness of fit, in terms of R^2, is similar to Freeman's estimates. Based on the results in Table 2, I consider two adjustments to the model: labor market controls and addressing autocorrelation in the errors.

Table 3 shows elasticity estimates for the two extensions to the model in two periods. First, alternative labor market controls are added in the form of GNP and unemployment, both detrended using a Hodrick-Prescott (H-P) filter for annual data. This yields insignificant results for both high school and college workers in both periods. The values are not greatly changed, so this may indicate that earlier results were false positives. The next model again uses H-P detrended labor market controls, and adds an AR(1) term. This model yields significant results in the pre-1976 period and for high school only workers in the post-1976 period. Note that the standard errors are similar to the previous model, so significance is not driven by increased precision. The elasticity for college graduates is more negative than for high school graduates in the early period, and the difference is not significant in the later period. Overall, this is weakly supportive of the claim that Freeman's results were accurate for the period, but that the relationship changed.

Table 3. Elasticity of Complementarity Estimates, Extended Model

Sample	Basic Replication	Extended Model Replication: H-P Detrending, Pre `74	H-P Detrending, `74 to `96	H-P Detrending, AR(1), Pre `74	H-P Detrending, AR(1), `74 to `96
College	-0.30	-0.29	-0.03	-0.77**	-0.04
	(0.21)	(0.21)	(0.08)	(0.22)	(0.10)
High	-0.15	-.03	-0.02	-0.17**	-0.18**
School	(0.20)	(0.16)	(0.05)	(0.07)	(0.06)

Notes: Replicated results were estimated jointly, interacting with a dummy variable for the post 1974 period. Standard errors are heteroskedasticity robust.

Conclusion

What we have seen in this chapter is that the relationship between relative wages and relative labor quantities can reveal both the degree of substitutability and the relative level of productivity between types of workers. Estimating this relationship is possible with existing data and techniques. A key limitation of the approach seen here is that labor is not well differentiated in the data. Workers with college degrees do high skill work and workers with a high school degree or less to low skill work. In reality, there are more than two types of work and a worker's education level does not perfectly predict what type or types of labor a worker provides.

The remainder of this dissertation moves away from the concept of high skill and low skill as types of labor and towards a framework in which labor is decomposed into tasks. Tasks are what workers do, while skill are something workers have. Different tasks have their own task-specific human capital. We can measure the quantity of tasks a worker does separately for the skill the worker has at performing a task. Once I obtain task quantities and shadow prices, we can analyze their relationship in a framework similar to the one used in this chapter, but we will be able to distinguish changes in wages across education groups within a task from changes in wages within education groups across tasks.

CHAPTER IV

WHO DOES WHAT? MEASURED TASK INTENSITY FROM 1980 TO 2015

In this chapter I make two extensions to prior findings in the task-based literature. First, I apply the measurement technique introduced in Peri and Sparber (2009) to a broader suite of tasks. Their method has an advantage that has yet to be fully appreciated, in that the method allows for task quantities to be aggregated across workers and can be applied to more than a few tasks at once. This aggregability allows for the estimation of task-labor demand curves and elasticities of price and substitution that until now have not been viable to estimate for many task categories. Second, I introduce computers as a task category.

The bulk of the chapter describes the construction of task quantity measures from the raw data in the March Current Population Survey (CPS) and Occupational Information Network (O*NET) datasets. I start with a discussion of the O*NET dataset and potential data quality issues, then describe the process of merging the datasets. The remainder of the chapter is an extended discussion of how tasks are distributed within the workforce and how the distribution of tasks has changed over the sample period. The general finding is that, within education-experience groups, mean task intensities have been roughly stable aside from computer tasks, which is somewhat surprising if computer tasks substitute for routine tasks. If relative task intensities are stable within groups and wages are a function of tasks, changing compensation for tasks must explain the changes wages between groups, a topic I explore in chapter 5. I also document the tendency of local labor markets to specialize into groupings of tasks e.g., many states will increase mean intensity in communication, nonroutine analytic, and computer tasks while decreasing manual task intensity. Routine analytic tasks avert this trend with growth largely unrelated to growth in other tasks.

Conceptually, I take the hundreds of occupations reported in the CPS and reduce them to a set of a few interpretable task variables observable at the individual worker level. I measure six tasks simultaneously, including, for the first time, computer tasks. The task measurements here are continuous at the worker level and, under conditions explained below, permit aggregation across workers. I allow variation in task intensity within occupations over time, both by using O*NET data from multiple time periods and accounting for changes in the composition of detailed occupations within census occupation codes. The description of the distribution of tasks within the workforce produced here is consequently richer than in other available sources.

O*NET Database History and Mission

O*NET is a project founded in 1998 to replace the Dictionary of Occupational Titles. Conducted by RTI International at the behest of the United States Department of Labor, O*NET's mission is to provide up-to-date information on occupational requirements to help workers make decisions about which occupations to enter. Note that producing longitudinal or panel datasets is not part of O*NET's core mission. Consequently, O*NET has historically not been averse to changing methodologies from year to year in ways that can affect both the interpretation and statistical properties of the variables they record. The existing research that uses O*NET data almost uniformly skirts this issue by applying one cross section of O*NET variables to the entire sample period, which is reasonable for short time periods or for tasks that do not substantially change within occupations over time. Here, I make use of O*NET data from multiple time periods, in what I believe is a useful extension to previous methods.

O*NET methodology

O*NET's taxonomy for occupations is an extension of the Standard Occupation Classification (SOC) system with additional breakdowns for some occupations. Values reported below are based on 882 SOC code occupations. The general methodology of O*NET is to survey incumbents. A set of questionnaires goes out to businesses expected to have workers in the selected occupations. Each questionnaire contains a set of questions on an area of O*NET's content model, such as skills, work activities, work context, etc. Respondents are asked to rate the importance or level of job requirements or job tasks, on a 1 to 5 or 1 to 7 scale. Mean values within occupations are reported. In some cases, these results are supplemented with input from occupational analysts. Since the first survey dataset was collected, about 100 occupations per year have been updated, meaning a dataset for a given year is based on a mix of older and newer survey results. I use O*NET dataset release versions 5.0, 13.0, 18.0, and 23.0 for the years 2000, 2005, 2010, and 2015 respectively.

Variable Selection

A brief discussion of the task-based framework is necessary to explain how variables from O*NET should be selected. Each job entails a mix of some number of tasks. Workers differ in the composition of tasks that they perform. We can think of workers as providing labor that they apply to producing a mix of intermediate inputs, tasks, that are then combined into a final output good or service. Tasks are imperfectly substitutable, so the marginal product of a worker doing a specific task depends on the worker's skill at that task and on the aggregate quantities of other tasks, both from the worker in question and other workers, potentially in different groups.

How to divide tasks into reasonable categories is well covered in the literature; see Acemoglu and Autor (2011) for an extended review of the topic. There are two commonly used schemes that may be combined. Tasks are sorted into routine and non-routine categories or into analytic (abstract), manual, and interactive (communication). When these are combined, interactive is typically treated as non-routine, leading to a framework of five categories. For each category of tasks, a set of several related variables is selected and combined through one of a handful of methods covered in more detail below. These methods have not been applied to computer tasks, however, and I will explain shortly how the framework is applicable.

My selection of variables is based on the methods in Acemoglu and Autor (2011) and Peri and Sparber (2009). The O*NET variables for each task category are shown in Table 4. I will refer to the listed O*NET variables as the "raw" variables as necessary to avoid confusion with the task measures I produce later. I use Peri and Sparber's (2009) communication tasks rather than Acemoglu and Autor's (2011) interaction tasks because the latter are focused on the question of offshoring and constructed their variable to be sensitive to the need for face-to-face interaction, which is less of a focus here.

Table 4. Variable Selection for Task Categories

Task Measure	O*NET Variables
Computer	Interacting with computers; Programming
Communication	Oral comprehension; Oral Expression; Written Comprehension; Written Expression
Routine Manual	Pace determined by speed of equipment; Controlling machines and processes; Spend time making repetitive motions
Nonroutine Manual	Operating vehicles, mechanized devices, or equipment; Spend time using hands to handle, control or feel objects, tools, or controls; Manual dexterity; Spatial orientation
Routine Analytic	Repeating the same tasks; Being exact or accurate; Structured vs unstructured work
Nonroutine Analytic	Analyzing information; Thinking creatively; Interpreting information for others

All of these questions are of the form "rate the importance of X on a scale of 1 to 5." Figure 5 plots the year 2000 values of O*NET variables within each task category against their values in 2015. Taken at face value, over that time many occupations have seen substantial change in task intensity. The predominant trend is a broad increase in reported task intensity for low intensity occupations. This pattern is visible for all task categories except for nonroutine manual. How much of this is real is questionable. The result hinges largely on using the year 2000 as the base year. Comparing other years to 2015 shows occupations evenly scattered around a 45-degree line, though the fluctuations are substantial. For most tasks, there is a noticeable but less pronounced drop in task intensity for high intensity occupations, suggesting that some degree of measurement error is causing strength of the relationship to attenuate. This is a potential issue, but the methods used to convert these variables into task measures should reduce it.

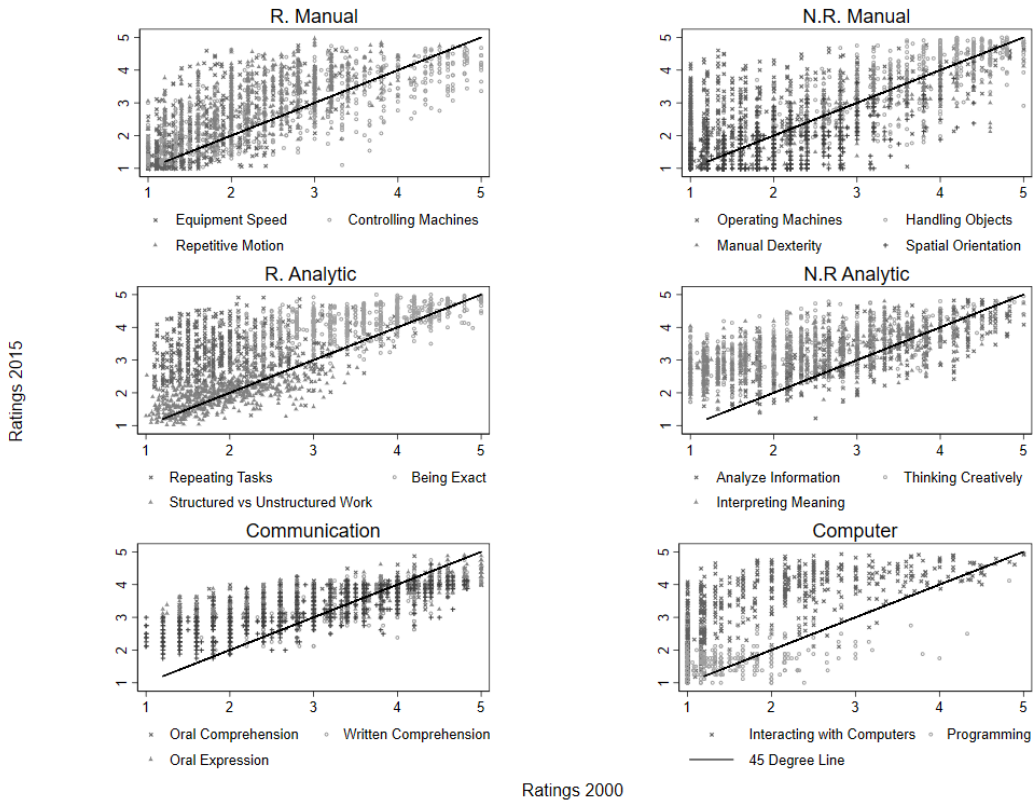

Figure 5. Within Occupation Changes in Task Intensity, 2000 to 2015

The other takeaway from Figure 5 is that, within task categories, the changes in values over time are in most cases strongly correlated. This is especially true for variables in the communications category, in which changes have correlation coefficients between 0.45 and 0.81, while correlation coefficients are weaker for variables in the routine manual category at between 0.09 and 0.25. In no case is the correlation between tasks within a category negative and significant. In short, values within task categories move in the same direction. While not conclusive, this is supportive of the claim that these variables are capturing features of the same types of tasks. Recall that prior work combining these variables has used O*NET data from single time periods, so this finding is encouraging, since I require a common method of combining variables in different time periods.

The Case for a Computer Tasks Category

One of these task categories is not so well established as the others, so I take a moment here to discuss how computer tasks should be defined and interpreted. I must establish both that computers are a valid task category and that we can measure computer tasks empirically using the same general methods used for other tasks. Starting with the latter, my computer task measurement is based on two items on the O*NET surveys: rate the importance of "Writing computer programs for various purposes" and "Using computers and computer systems to program, write software, set up functions, enter data, or process information" on a scale of 1 to 5. These are sufficiently computer related and similar in format to commonly used O*NET questions for empirical work.

To create a computer task index within the existing framework we need to consider both what computers do and how they are used by typical workers. Computers manipulate data using operations that have been unambiguously defined in a specialized format. In that sense, computers substitute for routine tasks, as argued in Autor, Levy, and Murnane (2003). There, the authors approach computers as physical capital that substitutes for routine tasks done by human workers, leading to decreased demand for the low education workers that tend to do routine tasks and higher demand for college educated workers that tend to do nonroutine tasks. They acknowledge computer skills as a type of human capital, but do not explore computers tasks as a distinct type of labor.

Someone must induce the computer to produce the desired output, and doing that takes dedicated time and effort the same as other tasks. Workers must produce the unambiguous instructions that the computer requires to operate. In that sense, computers perform work that substitutes for routine tasks done by workers but require non-routine labor input to do so, and my definition of computer tasks encompasses the latter. While in principle a routine category of computer tasks can be defined, in practice we should expect computer tasks that are routine to be done by computers. That is, routine computer tasks, once identified, are incorporated into software after a brief potential lag. This is really all that software is. Consequently, in equilibrium there are no routine computer tasks performed by human workers, keeping in mind that under the definition of computer tasks used here typing and data entry are not included.

We might think of computer tasks as a second category of interactive or communication tasks, where the worker "communicates" with a non-human, not truly intelligent device. My definition of computer tasks includes programming, but also other activities such as organizing files, customizing settings, and actively using software. It does not include passive uses of computers, where the user is watching or reading off the screen. The task category definition in this paper can be summarized as "an activity that produces instructions a computer can use." Fortunately, this is quite close to the phrasing used in the O*NET questionnaires.

Limitations of O*NET Data

I have alluded to the limitations of the O*NET dataset before, but before continuing I will state a few explicitly. First, we cannot observe differences within occupations that are frequently of interest, such as by education, experience, or location. This is probably not too serious. If an occupation had large task differences between workers, we would typically not think of it as a single occupation. Occupations are defined such that their internal task compositions are mostly consistent across time, location, and worker type. This is especially true when occupations are highly detailed, as they are in O*NET's 1,000 plus detailed occupations, most of which are usable here. Additionally, the usual case is that a firm decides on a position and what the position requires, rather than finding an employee and setting job requirements to match, so job requirements should generally not differ based on the type of worker that eventually fills the position.

Other concerns include the difficulty of interpreting the self-reported values and converting the many variables from O*NET into a small number of task variables. These issues I address with methods drawn from Peri and Sparber (2009) and to a lesser extent Acemoglu and Autor (2011). A final issue is that changing methodology can lead to changing statistical relationships between variables. A case in point is that early in the sample periods, the "interacting with computers" variable and the "programming" variable mentioned above are questions on separate surveys, while late in the sample period the "programming" variable is updated by analysts based on the survey results for the "interacting with computers" variable. This is ignorable for this chapter, but could be a minor concern in chapter 6.

Current Population Survey Data

Looking at the O*NET datasets alone can only take us so far. We need to see the changes in the occupational composition of the workforce in addition to shifts in task intensity within occupations to get a clear view of how the distribution of tasks in the workforce has changed in recent decades. To accomplish this, I link the task variables in O*NET to workers in the March CPS. Essentially, I take the high dimensionality census occupation data in the CPS and reduce it to a 6-dimensional task intensity space.

The CPS is a representative survey of 60,000 U.S. households. I make use of the March survey because it has additional questions related to employment and the job market, though many of these variables are more relevant in later chapters. To increase my sample size, I pool 3-year intervals as cross sections for the years 1980, 1985, 1990, 1995, 2000, 2005, 2010, and 2015. This pooling should also reduce the influence of recession years on occupation counts. Since I need to sort workers into experience groups, I restrict the sample to employed men, for whom potential experience is a better proxy for actual job market experience. After these restrictions, the sample discussed here contains 212,561 observations with workers in 291 census occupations.

Merger Process and Task Measure Construction

Since the CPS uses census occupation codes rather than SOC codes, some crosswalk is needed, preferably one that allows changing occupational composition over time. To account for changing SOC occupational composition within the less detailed census occupations, I use the American Community Survey (ACS) to produce a crosswalk. This is similar to the method used in Acemoglu and Autor (2011), though they use the Occupational Employment Statistics Survey for their labor supply weights. I merge the O*NET datasets to the ACS for the same year by SOC code, then collapse to census occupation codes.

Now that occupation counts are linked to task variables from O*NET, I apply the method of constructing task quantities from Peri and Sparber (2009). Each task variable is converted into a percentile based in the population, i.e. everyone in the highest rated occupation for that year gets a 1, everyone in the lowest rated occupation gets a 0, and everyone in the median occupation gets a 0.5. The mean value of variables within each task category becomes the measured task quantity for that occupation in that year.

Since the process described above can be difficult to picture, I will give an example of the calculation. In the year 2015, accountants have scores of 3.75, 4, 3.88, and 3.75 for the O*NET variables Oral Comprehension, Written Comprehension, Oral Expression, and Written Expression respectively. Accountants are coded as “13-2011” under the SOC taxonomy and “800” under the census occupation taxonomy. Since the taxonomies have a one-to-one correspondence for this occupation, the O*NET values for accountant are applied to any workers labeled “800” under the census occupation code in the CPS. In cases where multiple SOC codes fall under one census code, I take a mean weighted by total workers in each of the SOC occupations based on the ACS for that year. The are 650,785 workers in the ACS sample for that year, and accountants are at about the 45th percentile for Oral Comprehension. After converting to percentiles, the rating for the four variables in the communications category are 0.4478, 0.9564, 0.6067, and 0.8672. I take the mean of these four values and get a communications task quantity of 0.7195 for accountants in the year 2015. Differences in measured task quantity within occupations over time come from changing values of the O*NET variables and changes in occupation counts in the ACS.

What features the resulting task measure has depends on a few underlying conditions. Suppose each raw variable is the sum of the real task variable and an idiosyncratic error term arising from measurement error. Then the mean of several raw variables will have reduced measurement error. This plausibly arises when the raw variables are based on results from separate surveys and the questions are closely related. Now suppose that task quantities follow a uniform distribution in the population, though the method can be adjusted for other assumed distributions. Under this second condition, the task quantity can be aggregated across workers.

How easily we can determine the true distribution of tasks in the population is debatable, as is whether tasks should follow the same marginal distribution. That said, we can safely assume that tasks quantities for individual workers are bounded below by zero and bounded above by a finite number, so the uniform distribution may be the most defensible guess. Verifying the population distribution of tasks is infeasible with existing publicly available datasets, and would likely require detailed time use data relating to work activities. A benefit of the method here is that all we would need to check alternative distributions is to apply the inverse CDF of the distribution in question.

Task Composition Changes within Occupations over Time

Figure 6 plots year 2000 task intensities within occupations against their year 2015 values. What we see here is a moderate improvement over the relative chaos of Figure 5. Within occupation changes are much more similar over time, while allowing for moderate fluctuations. Changes over short time periods are much smaller, so what we have here are mostly believable changes over a tumultuous 15-year timespan. We also fail to see the seeming attenuation that was visible in the relationships in Figure 5.

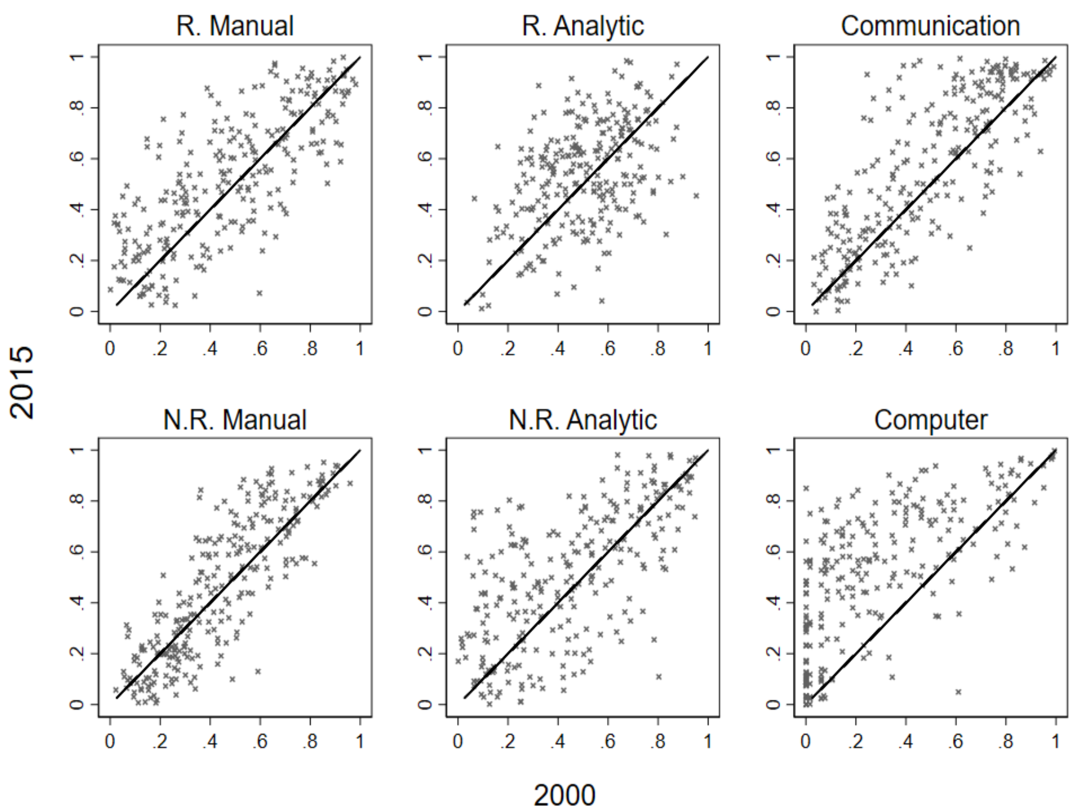

Figure 6. Scatterplots of Measured Task Intensity
Note: Based on ACS occupation counts.

A concern about this method is that if the distributional assumptions are wrong, the value obtained will be purely relative. This would lead measured task intensity to fall simply because more task intensive occupations saw higher employment growth, even if workers in each occupation are doing as much of the task as before. In that case, most of the values in this chapter can still be usefully interpreted, though some methods used in later chapters would be invalidated. Note that if the measured task intensities in this chapter are purely relative, we would expect the relative increase in college graduates over the sample period to drive down the measured intensity in analytic and nonroutine tasks for high school degree workers.

Data Differences Pre- and Post-2000

Recall that the CPS data used here goes back to the year 1980, while O*NET and ACS data only goes back to the year 2000. This necessitates caution when comparing results in the late and early sample period, since the results stem from different types of variation. Post-2000, results are influenced by changing values within SOC occupations in O*NET, changing SOC composition within census occupations from the ACS, and changing census occupation composition within the CPS. For the pre-2000 period, both task values and SOC occupation labor weights are constant at year 2000 values. Shifts in the means within groups reflect only changing census occupation composition in the CPS, with one exception.

Adjustment for Pre-2000 Computer Tasks

I make one additional adjustment to the merged dataset. While most tasks have plausibly been, to a first approximation, stable over time within occupations, this is clearly not the case for computer tasks, as computers were gradually adopted over the sample period. I use computer usage rates reported in the 1984, 1989, 1993, 1997, and 2001 October CPS to adjust computer task quantities downwards. I take the mean usage rate within occupations for each year, interpolate between years where necessary, and divide by the 2001 rate to obtain the adjustment factor for each occupation, which I apply for the pre-2000 period. For example, if 40% of accountants used computers in 1984 and 80% used computers in 2001, the computer task intensity for accountants in 1985 would be half of the year 2000 value of computer task intensity for accountants, ignoring interpolation between years.

Description of Measured Task Quantities

Having merged the datasets, I have detailed task measurements at the individual level. The remainder of this chapter discuses patterns in the task distribution of the workforce over time. I find that the apparent stability at the national level hides substantial shifts at the level of local labor markets. I also find moderate shifts in the relationships between tasks.

With occupation counts and a smaller number of task quantities to work with, we can better see the relationships between tasks. Figure 7 plots the relationships between task intensities within occupations. We can see that the relationships are similar in both the year 2000 and the year 2015, though the strength of the relationships vary. We can also see a positive relationship between computer, nonroutine analytic, and communications tasks. Routine analytic tasks are weakly correlated with other tasks within these cross sections. Meanwhile, routine and nonroutine manual tasks are strongly correlated, while both are negatively correlated with computer, communications, and nonroutine analytic tasks.

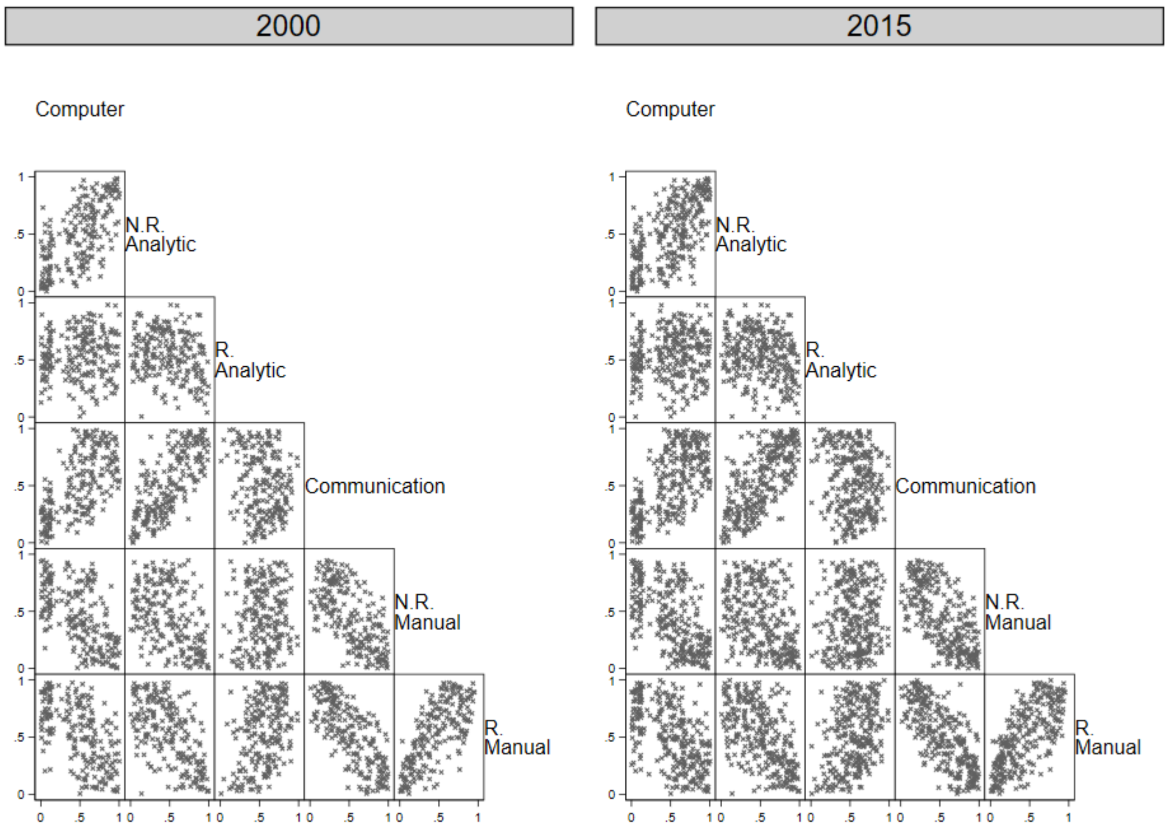

Figure 7. Task Intensity Changes within States.

In short, Figure 7 suggests that jobs cluster into groups that focus on nonroutine analytic, communication, and computer tasks or on manual tasks, while routine analytic tasks are dispersed across occupations regardless of specialization into other tasks. I will turn to the question of why shortly. While the results in Figure 7 are not surprising, they are encouraging. We have prior reason to expect correlation between routine and nonroutine manual tasks and for nonroutine analytic tasks to be correlated with computer and communications tasks. The method for constructing task quantities used here has not been applied in quite this way before, so establishing that the method works consistently over time and preserves known relationships is important. While the O*NET data showed fluctuations in task variables over time, the constructed task intensity measures here do not show major shifts in the relationships between occupations over time.

Mean Task Composition by Experience Group

While we cannot observe differences between education and experience groups within occupations, we can now examine mean task levels within those groups. Figure 8 shows the mean task intensity within education-experience groups over time at the national level. The margin of error on these means is generally less than 0.01, so most differences large enough to see in the graphs are real. Means here are weighted by hours worked.

The first feature to note is that one of these categories is much different than the others. Computer tasks saw a massive increase while other tasks were roughly stable. This is largely because computer task intensity was adjusted in the pre-2000 period, which was not possible for other tasks. For other tasks, recall that all pre-2000 task quantity shifts are based on changes in the occupational composition of the workforce as observed in the CPS. Keep in mind that this should not be taken as evidence that computer tasks changed more dramatically than other task categories, but rather this is an estimate of the degree of the change in computer tasks given we know a major change occurred over the period and were able to make an adjustment. None of this is likely to have had a major effect on the results, given that we know computers were being adopted in workforce over this period and we should expect other tasks to be approximately stable within occupations over time.

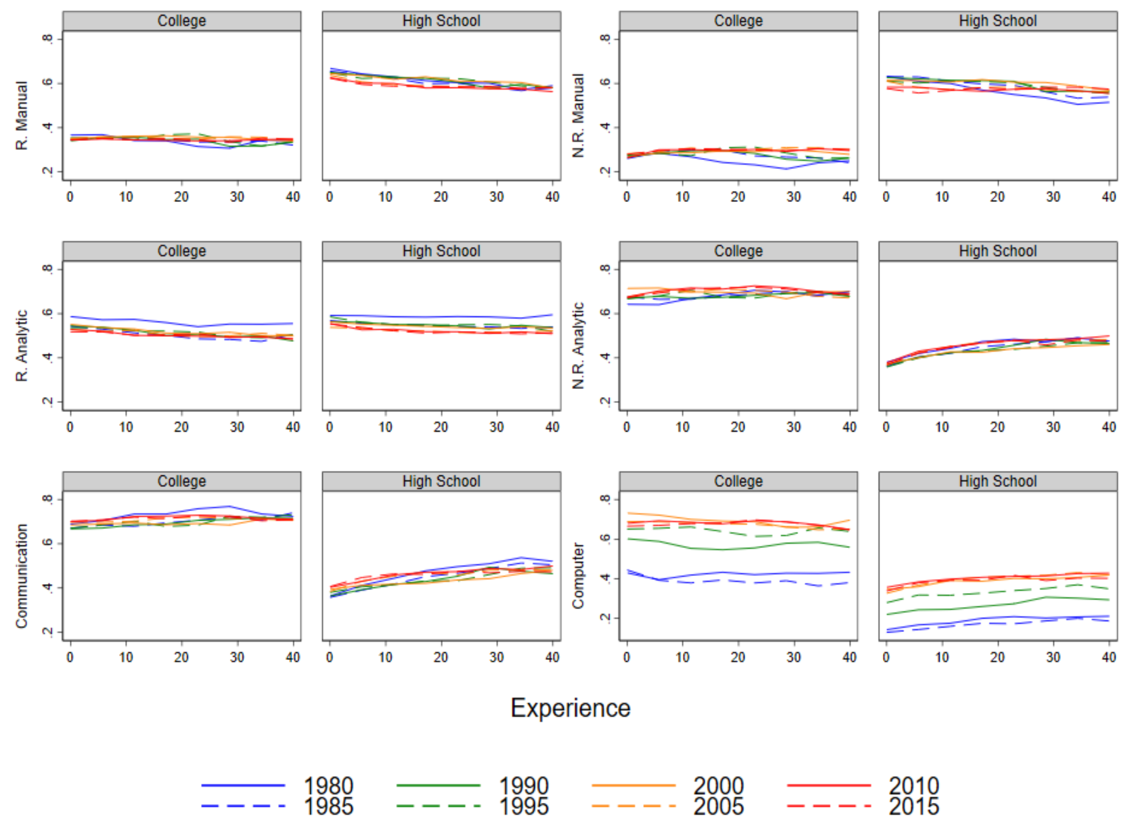

Figure 8. Mean Task Intensity by Year, Education, and Potential Experience.
Sources: O*NET, March CPS.

We can see several patterns in Figure 8 that match known facts about the task composition of the workforce. High school only workers are more intensive in both routine and nonroutine manual task than college degree workers. College degree workers are more intensive at nonroutine analytic, communications, and computer tasks than high school only workers. We can also see that college degree workers vary less in their task composition by experience group, while experienced high school only workers engage less in manual tasks and more in nonroutine analytic, communications, and computer tasks than their low experience counterparts. That this holds for computer tasks, both before and after the year 2000, is a positive sign. Though not covered in the task-based literature, a similar finding appears in Weinberg (2002), who found that experienced high school only workers used computers at a higher rate than their inexperienced counterparts.

In a few cases, we can see changes in task intensity within groups over time. Since 1980, there has been a modest increase in communications tasks by new high school only and college degree workers, a decrease in routine and nonroutine manual tasks by new high school only workers, and a sizable decrease in communications and routine analytic tasks by experienced high school only workers. The steepness of the profiles of manual tasks for high school only workers has decreased over time, such that late in the sample period low and high experienced high school workers provide more similar levels of manual tasks. Given that their profile for communication and computer tasks have also flattened, the mean task intensities for high and low experience high school degree workers are noticeably more similar at the end of the sample period than at the beginning. This may suggest that high school only workers are more substitutable across experience groups now than in the past.

Aside from computer tasks, communication tasks exhibit the most noticeable shifts. In general, communications tasks shifted from high experience workers to low experience workers. This is surprising, given that we would expect communications skill to increase with experience and to be more valuable for workers doing more skill sensitive tasks such as nonroutine analytic tasks. This pattern partially reversed in the later sample period. We may be seeing a response to the introduction of computers in the workplace, in which communication with young workers comfortable with the new technology became more important.

All that said, Figure 8 primarily shows a story of a stable distribution of tasks between worker groups over time. This is surprising in light of both technological change and the dramatic relative increase in college educated workers over time. When interpreting this result, we should recall that task quantities here are based entirely on occupations, with no within occupation differences by education being observable.

Task Specialization within States

In addition to national trends, linking the O*NET data to the CPS allows us to look at the geographic variation in the task composition of the labor force. This allows us to see trends within states as well as differences between states at a given time. In particular, we can see differential specialization into tasks or groups of tasks. Note that due to methodological differences, we should consider the pre-2000 and post-2000 sample periods separately.

Table 5 shows correlation coefficients between mean task intensity growth at the state level in the early and late sample periods. That is, a positive coefficient indicates that growth in two task categories was correlated across states. Cells bolded indicate that correlation is higher relative to the other time period. The first notable feature is that growth in computer tasks is strongly positively correlated with growth in analytic and communications tasks, negatively correlated with growth in manual tasks, and that the relationship is stronger late in the sample period. This is an interesting finding. We may have expected computer task growth to be weakly correlated with growth in other tasks, simply because growth in computer tasks was so widespread. The ability to observe this pattern stems largely from the richer, continuous measure of computer tasks introduced here, compared to the binary indicators that the early research on computerization was forced to use. Now we see that areas that specialized in computer tasks more aggressively specialized in nonroutine analytic and communication tasks. We may be concerned that the relationships between variables appears stronger later in the sample period due to more accurate measurement, but we also observe some relationships that are less correlated in the late period.

Table 5. Pairwise Correlation in Task Intensity Growth within States

2000 to 2015	Computer	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual
Computer	1.00				
N.R. Analytic	0.60*	1.00			
R. Analytic	**0.12***	-0.08*	1.00		
N.R. Manual	**-0.57***	-0.29*	0.07*	1.00	
R. Manual	**-0.56***	-0.50*	0.21*	0.78*	1.00
Communication	**0.53***	0.61*	-0.09*	-0.72*	**-0.82***
1980 to 2000	**Computer**	**N.R. Analytic**	**R. Analytic**	**N.R. Manual**	**R. Manual**
Computer	1.00				
N.R. Analytic	0.66*	1.00			
R. Analytic	-0.05	-0.13*	1.00		
N.R. Manual	-0.48*	**-0.52***	**0.20***	1.00	
R. Manual	-0.30*	-0.49*	0.22*	0.80*	1.00
Communication	0.37*	0.68*	-0.05	-0.68*	-0.73*
*-denotes significance at the 1% level. Bold indicates significant change in correlation between periods.					

The next most notable feature is a weakening relationship between nonroutine manual tasks and analytic tasks. This suggests that nonroutine manual tasks are more commonly paired with nonroutine analytic tasks than previously, though the relationship is still negative. The overall picture we should take from Table 5 is that areas are specializing either into manual tasks or into computer, communications, and nonroutine analytic tasks, and this process has been occurring since at least 1980.

Over the sample period, there have been substantial changes to both the age profile and the education level in the population. We may be concerned that the results in

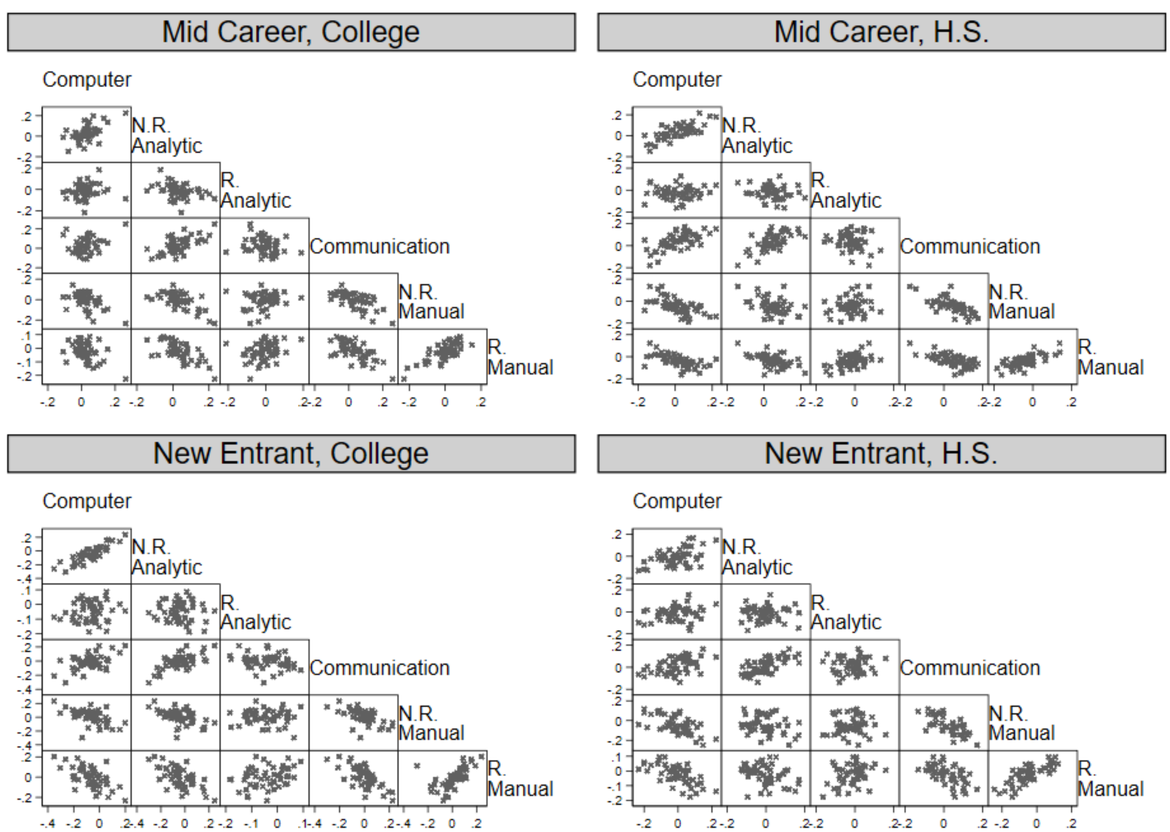

Figure 9. Within State Mean Changes in Task Intensity, 2000 to 2015
Note: Midcareer 20 to 25 and New Entrant 0 to 5 Years Potential Experience.

Table 5 are influenced by these shifts. If we look at task growth by education-experience groups, the findings are broadly similar, with minor variations between groups. Figure 9 gives scatterplots of growth in mean task intensities for the later sample period. We see here the changes in how states' workforces have specialized since 2000. Overall, states have become more polarized between those that focus on manual tasks and those that focus on other task categories. In fact, the correlations suggest that states have specialized into three broad groups: Nonroutine analytic and communications tasks, routine analytic tasks, or manual tasks. The change in routine and nonroutine manual tasks are strongly correlated within states, as are the changes between communication and computer tasks.

In some cases, differences between groups are of interest. For example, routine and nonroutine analytic mean task intensity is negatively correlated for mid-career college graduates, suggesting that states have specialized for one type of analytic task or the other over the sample period. For other groups, the relationship is weak. Also, changes in computer and routine analytic tasks are positively correlated for new entrants with a high school degree. This may indicate that routine tasks previously bundled with nonroutine analytic task oriented occupations have shifted to new entrants.

A somewhat surprising finding is that growth in computer task intensity is strongly linked to growth in communication task intensity, except for midcareer college graduates. This suggests that midcareer college graduates in an area tend to specialize into nonroutine analytic and computer tasks or nonroutine analytic and communications tasks, but not combine communications and computer tasks. For other groups, all three tasks go together. Also, midcareer college graduates saw a much more consistent increase in mean computer task intensity within states.

The state level breakdown of the data also suggests that task intensity rose across all groups and categories from 1980 to 2000. This is a surprising result, given that this is the period in which within occupation changes are lacking. This essentially shows us that occupations that were more task intensive in the year 2000 saw higher employment growth over the previous 20 years.

Figure 10 shows correlations between within state changes in task intensities for the early sample period, again by education-experience group. The general patterns are similar to the later sample period, suggesting that the trends in specialization seen there are part of an ongoing process that has lasted several decades. A handful of differences are worth discussing. First, changes in computer task intensity are less correlated with changes in nonroutine analytic task intensity for most groups. This suggests that as computers were adopted, they were not targeted at workers doing specific tasks. In fact, the clearest patterns are for midcareer high school only workers.

Less prominently, the slopes of the best fit lines tend to be shallower, even if the degree of correlations are similar. Higher manual tasks are associated with lower computer, nonroutine analytic, and communications tasks, but the gradient is shallow. This suggests areas were specializing more slowly in this period, with the overall trend still being for areas to focus on computers, nonroutine analytic, and communications or on manual tasks, while college graduates tending to focus on either routine or nonroutine analytic tasks.

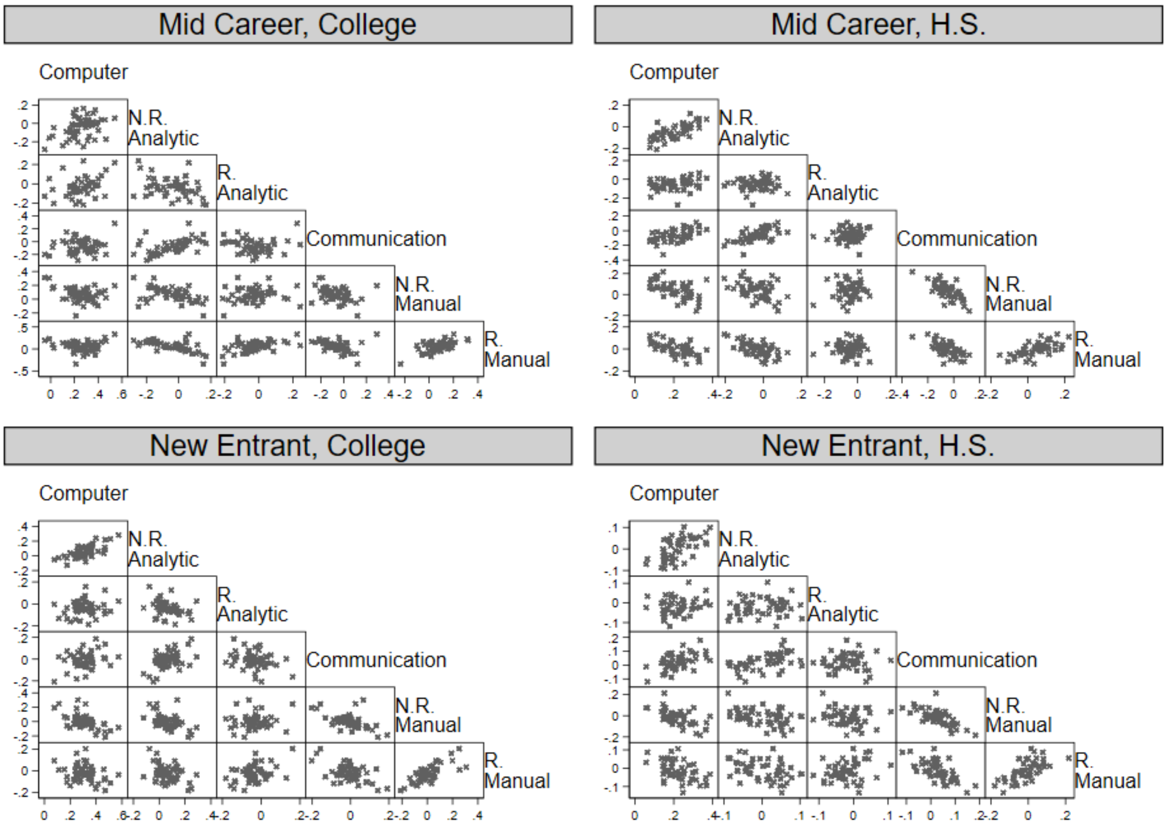

Figure 10. Within State Mean Change in Task Intensity, 1980 to 2000
Note: Midcareer 20 to 25 and New Entrant 0 to 5 years potential experience.

Summary and Comparison to Prior Findings

To summarize the changes in the task composition of the labor force revealed in this chapter, the national level means within education-experience groups is largely stable aside from computer tasks, while states have increased specialization in two directions. Where comparable, the findings here match findings in the earlier literature, especially in terms of the marginal distribution of tasks across worker types. The findings are broadly consistent with stories about polarization in the job market, but is more complicated than the stories in which computers substitute for routine tasks. What we see here is that routine analytic tasks were historically bundled with nonroutine analytic and communications tasks, but have been unbundling from them for the last few decades. Routine analytic tasks did not vanish as they would if computers directly replaced them, but are done by workers and in areas separate from the ones that do nonroutine and communications tasks. Drudgery has been redistributed. Additionally, the measured task composition of the labor force has not much changed in response to the increase in college graduates.

The task measurement here suggests that differences in outcomes, such as the college wage premium, must be driven by differences in the implicit shadow prices more so than changes in the tasks workers perform, unless we look to computers as the principal driver of the changing wage structure. This prompts the questions addressed in chapters 5 and 6. If the task composition of workers within education-experience groups has remained similar over time aside from computer tasks, changes in the wage gaps between these groups must be due to some combination of rising computer task quantities and changes in compensation related to other existing tasks, due either to substitution effects or human capital differences. In chapter 5, I estimate wage effects in the form of shadow prices for tasks. In chapter 6, I explain changes in task shadow prices.

CHAPTER V

DO TASK SHADOW PRICES EXPLAIN THE EVLUTION OF SKILL PREMIA FROM 1980 TO 2015

In the previous chapter, I constructed a measure of task quantities and showed that the relative amounts of tasks done by education-experience groups have been stable since 1980. Over the same period, skill premia changed substantially, as documented in Bound and Johnson (1992) and Goldin and Katz (2009), who both show large growth in the college degree premium. Here I seek to explain wage changes through changes in the shadow prices of tasks. Based on the observed changes in wages, shadow prices for tasks conducted by more experienced and more educated workers should have risen over the period. I find that a few task shadow prices explain most of the overall wage differences between groups. Additionally, computer task shadow prices had a mitigating effect on the increase in experience premia.

The premise here is that a worker’s overall labor can be decomposed into task labor quantities. Since tasks are bundled into jobs, we cannot observe the exchange of singular tasks for wages in the market, but given wages and task quantities at the individual worker level, shadow prices for tasks can be estimated in a hedonic framework. This approach is established in the literature, and I base the method here primarily on Peri and Sparber (2009). Chapter 4 describes how I adapted their method of measuring task quantities for individual workers, while this chapter covers how I adapt their method of estimating shadow prices, while making use of recently developed improvements to estimation techniques.

Another contribution of this chapter is a dataset of task shadow prices that can illuminate how wage premia have developed over time. A limitation of the existing literature is that the explanations for why wages grew for nonroutine analytic oriented jobs are nebulous. The dataset of task shadow prices and quantities produced here will allow a more detailed examination of the determinants of task shadow prices and how tasks substitute for each other. We will be able to see whether quantities of one task strongly affect prices for another and the extent to which workers in different education-experience groups influence the shadow prices of different tasks. We may also be able to track task-specific human capital accumulation within cohorts over time based on shadow price changes. Not all of these topics are within the scope of this dissertation, but in chapter 6 I will estimate time trends in shadow prices, searching for bias in technical change.

Since some of the task measures here are novel, checking that they fit the stylized facts of the literature is worthwhile. Figure 11 show scatterplots of median hourly wages within occupations and task intensities for occupations in the years 2000 and 2015. What we can see here is that the relationships between wages and routine manual, nonroutine manual, and routine analytic tasks have weakened over the period, such that the best fit line has a less positive (or more negative) slope. Over the same time, occupations intensive in nonroutine analytic, computer, and communication tasks pay better than they once did, and the strength of the correlation with wages is stronger than for the other task categories. This is in line with the usual stories about declining pay for a strong back and for routine work as discussed in Acemoglu and Autor (2011), Autor and Dorn (2013), and Goldin and Katz (2009), and elsewhere, but figures like Figure 11 are often misleading as the trendlines almost certainly suffer from omitted variable bias. This chapter will reveal whether the relationships between task intensity and prices remains after controlling for other factors.

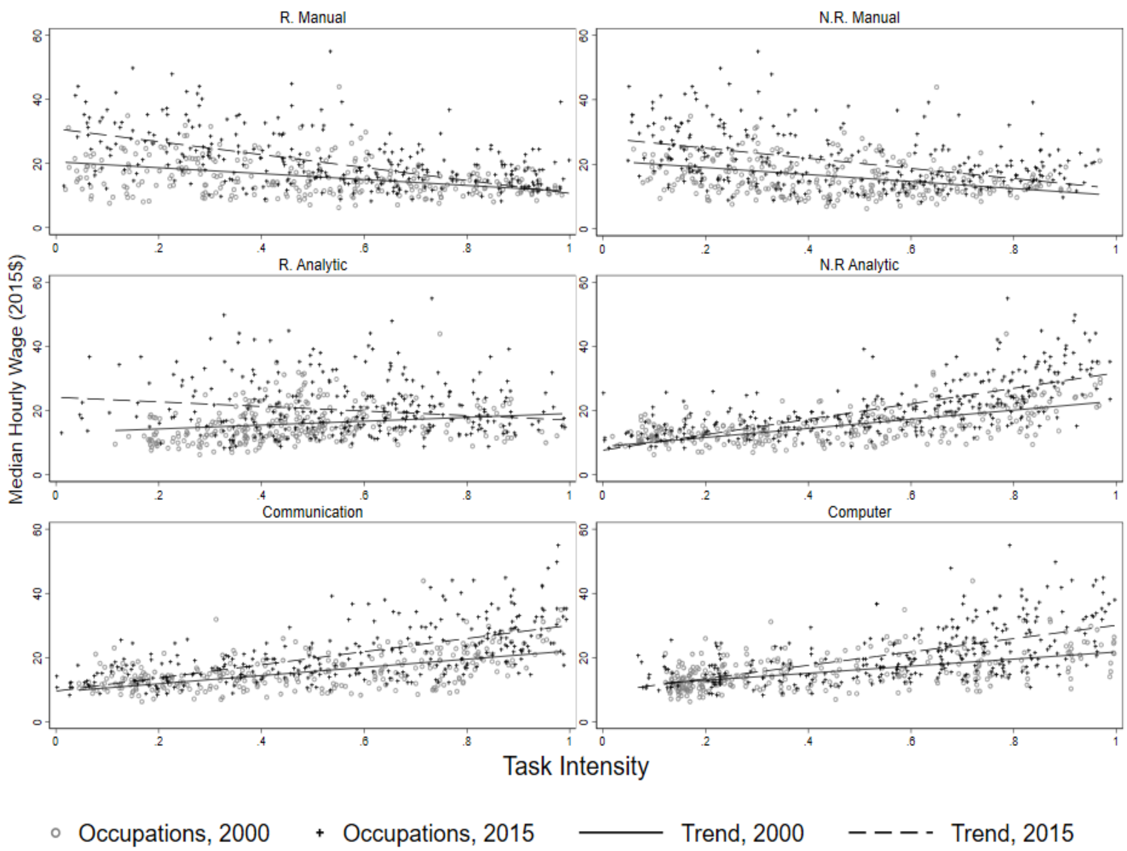

Figure 11. Scatterplots of Hourly Wages and Task Intensity, 2000 and 2015
Sources: ACS, O*NET

Data and Methodology

The dataset used here is the March CPS merged with task quantities based on the O*NET dataset, as covered in detail in chapter 4. This sample contains 212,561 observations of individual workers in 16 education experience groups over 8 periods from 1980 to 2015. Shadow price estimates are based on cross sectional variation in wages and task intensities within these cells. The model includes 6 categories of tasks, including the widely used suite of routine and nonroutine analytic, routine and nonroutine manual, and communication tasks, as well as a measure of computer tasks I introduce.

The CPS is a nationally representative survey of 60,000 households. These households are surveyed repeatedly throughout the year, with the March survey containing additional questions related to work and income. The number of individuals contained in these samples varies from year to year. To get a reasonable number of observations in each cell, I combine 3-year periods at 5-year intervals, i.e. the 1985 sample contains 1984, 1985, and 1986, etc. I restrict the sample to full-time (as defined in the CPS, at 35 hours or more per week) working males surveyed from 1980 to 2015, with reported values for annual wages, hours worked, occupation, education group, age, state, and other demographic and household variables. The March supplement is preferrable to other CPS products due to its more extensive control variable availability, such as previous year occupation and pension program participation.

I convert to real wages in 2015 dollars using GDP deflator and use usual hours worked and weeks worked to obtain hourly wages. Within education and experience groups, real median wages grew at about 0.5% per year in the sample. An exception to this pattern is a jump for low experience college graduates in the late 1990s that may have been as high as 10%. Thus, overall wage growth over the period can be explained almost entirely by rising education and an aging population. If trends in total compensation follow trends in wages, this implies anemic productivity growth over the period. Note also that the stagnant national median wage conceals moderate growth in wages in some states, which is offset by lower population growth in high wage states.

I sort workers into 5-year potential experience groups based on their age and education. By assumption, high school graduates enter the workforce at age 18, college graduates at age 22. I choose to use comparatively narrow experience groups in the expectation that cohort differences may be important, and this choice influences several other choices in constructing the sample dataset. To illustrate, suppose a cohort experiences a shock that affects their productivity at computer tasks. Perhaps the cohorts in high school just before and just after Sputnik received differently relevant educations. For example, the National Defense Education Act prompted by the Sputnik crisis greatly increased funding for education and increased the emphasis on the sciences in the public education system. If those shocks affect only a few years of students each, we lose information by sorting into wider potential experience bins. This could also occur for workers that enter the labor market in a recession, as discussed in Kahn (2010) and elsewhere. Dealing with these shocks is feasible when the experience bins are narrow and observed frequently, allowing for balanced panels. These concerns are another reason the CPS, with frequent observations in the relevant period, is preferrable to other data sources.

In terms of potential experience, the median worker in both education groups has six more years in 2015 than in 1980, with the rise following a linear trend. The distribution of experience is highly left skewed in 1980 and is symmetric by 2000 and afterwards. After 2000, the distribution also flattens out to become roughly uniform near the end of the sample period. This is interesting in light of a result from Lam (1989), which suggests that a uniform age distribution minimizes lifetime wages if workers of differing ages are imperfect substitutes. This suggests that cohorts that experienced a skewed age distribution in earlier periods of the sample benefited compared to the later cohorts that experienced a more symmetric distribution.

The great value of the March CPS over other data sources is the abundance of labor marker and demographic control variables available. Below I describe the estimation method used to obtain shadow prices in this chapter and how it permits the use of large numbers of control variables. The controls in March CPS include indicators for race, marital status, number of children, number of children under 5, number of people in the household, detailed education, age, recent changes in occupation, and inclusion in employee pension plan. Using a data driven approach to selecting controls, I use all of these and their interactions as potential control variables. To avoid controlling for too much, estimation is conducted separately by education-experience group and I use only an indicator for whether a worker switched occupations in the last year rather than full indicators for last year's occupation.

Peri and Sparber (2009) serves are the basis for the approach taken here. The authors model wage differences between low skill migrants and natives to see if immigration leads to changes in the relative task intensity for natives, and show that low skill native workers switch to communications tasks and away from manual tasks in response to rising immigration. To obtain prices, they use a variant of the partialling-out estimator by regressing wages on demographic variables, regressing task quantities on the same variables, then regressing the residuals from the first regression on the residuals from the second regression. Unusually, their method has wages in log form in the first regression. Their model is implicitly in a hedonic framework.

A little more formally, Peri and Sparber (2009) derive their specification based on a task-based nested CES production function. This is a natural extension of the familiar CES production function in which the labor inputs are themselves CES conglomerates whose inputs are specific types of labor. The details of the task-based nested CES are more relevant in chapter 6, so for the moment I will describe the general case. Suppose aggregate production is $F(X)$, where X is a 1 x n vector of total task labor quantities from all workers. Then a worker's marginal productivity is the directional derivative $D_y F = \sum_{i=1}^{n} v_i \, \partial f/\partial x_i$, where v is the vector of task labor quantities performed by the worker in question. We can interpret the $\partial f/\partial x_i$'s as task-specific wage rates if conditions hold for wages to equal marginal productivity. These derivatives are not directly observable unless firms explicitly use piece rate compensation schemes, but are reflected in the labor demand curve. These wage effects are what the hedonic model estimates using the measures (v_i) of task intensity constructed in chapter 4.

In terms of the production function, I need prices $(\partial f/\partial x_i)$ for one task not to depend on quantities of other tasks provided by the same worker, which will hold in the case of the task-based nested CES as long as the number of workers is large. This yields the expression for the basic model of wages I estimate,

$$w = w_{rm}M_r + w_{nrm}M_{nr} + w_{ra}A_r + w_{nra} + A_{nr} + w_I I + w_C C$$

Equation 7

where w is wages, M, A, I, and C indicate manual, analytic, communication (interactive), and computer tasks respectively, while r and nr denote routine and nonroutine.

The empirical approach to estimating this equation is slightly more complicated. In the decade following the publication of Peri and Sparber (2009), partialling out estimators were an active area of research and several more powerful estimation techniques were introduced. See Belloni et al (2014) and Chernozhukov et al (2018) for discussion. The key advantages of the new methods are data driven control variable selection via lasso and more accurate standard errors in the second stage regression.

The approach I adopt here is termed cross-fit partialling-out or debiased machine learning (DML). The intuition behind this approach is that when variable selection is based on the sample, what would usually be misspecification bias is a random variable, and under surprisingly plausible conditions that random variable will converge in probability to 0. To summarize DML, control variables are selected using lasso on both the dependent (outcome) and key independent (policy) variables. For those unfamiliar, lasso selects control variables by restricting the length of the coefficient vector in a regression. The outcome and policy variables are then separately regressed onto the selected controls, after which the residuals of the outcome variable are regressed on the residuals for the policy variables. This is extremely close conceptually to Peri and Sparber's (2009) approach, though with additional technical details.

The DML framework supposes that

$$y = d\alpha + g(x) + u$$

Equation 8

and

$$d = m(x) + v$$

Equation 9

where *d* is the policy variable (or vector), *y* is the outcome of interest, *g(x)* and *m(x)* are unknown functions of the control variables *x*, and *u* and *v* are error terms with expected value 0 after conditioning on *d* and *x*. Here $d\alpha$ corresponds to Equation 7, with α being $[w_{rm}\ w_{nrm}\ w_{ra}\ w_{nra}\ w_{I}\ w_{C}]$. Note that since the final stage regression is of residuals on residuals, the model contains no constant as all have mean 0. The DML approach estimates *g(x)* and *m(x)* using the large number of control variables made possible by implementing lasso. Specifically, the models of *x* and *y* is estimated on the full set of control variables, but in addition to the usual $\sum_{i=1}^{N} \varepsilon_i^2$ term in the objective function, a penalty term $\lambda \sum_{k=1}^{p} |\psi_k \beta_k|$ is included in the objective function. This effectively restricts the length of the coefficient vector. We may think of this as a control function approach, though that term is not usually applied to this method. The key conditions to estimate α consistently are that we observe sufficient control variables to approximate *g(x)* and *m(x)* and that the number of variables necessary to do so is small. We need not know which of the variables are relevant a priori.

On a side note, Stata introduced a command, xporegress, to estimate DML the year after the method was first published. This is a relevant example of the argument made in chapter 4 that computer tasks done by workers are necessarily nonroutine, as in equilibrium any routine computer task is incorporated into software.

The benefits of using DML here are several. First, Peri and Sparber (2009) make use of few control variables, selected based on their expertise. This is reasonable in the context of their work, but less so here. For example, I wish to conduct estimation separately by education-experience group, as I expect shadow prices to vary with skill. For some groups, within group age differences are surely important, such as recent labor market entrants. For the highly experienced groups, within group age differences should be irrelevant. Theory offers little guidance as to when within group differences cease to be relevant, so a data driven approach is helpful. Similar issues arise for education, and in some cases where the CPS variables are needlessly detailed. For instance, Peri and Sparber use racial indicators for both Chinese and Japanese when partialling out, even though they lack many other race categories. Similarly, whether divorced and separated individuals should be treated differently is not obvious. A final benefit to the data driven approach is that the relevant variables may vary over time. For example, divorce has become much more common over time, so divorce may not be as strongly related to other worker features as in earlier times.

A more minor benefit of the DML approach over Peri and Sparber's (2009) method is reduced sensitivity to functional form. For example, Peri and Sparber (2009) have wages in log form in their first step regression, meaning they have to convert residuals back to levels for their second stage regression. How sensitive their results are to the details of this process are unclear. They also lack a method of adjusting the standard errors in their final regression to account for this process.

There are a few details to keep in mind when interpreting the coefficients. The resulting estimate of α is the vector of shadow prices, or wage effects. Taken literally, going from doing no computer tasks to doing computer tasks at peak intensity would raise hourly wages by w_C. A coefficient of 10 indicates that moving to an occupation with task intensity 0.1 higher would raise the worker's hourly wages by $1. Since the task quantities used here are based on occupation, it is more defensible to think of this as the wage effect of a worker switching to an occupation with differing task intensity, keeping in mind that the coefficients are estimated off of cross-sectional variation within education-experience groups. This potentially differs from the effect of a change in task intensity within an occupation over time.

Shadow Price Estimates Assuming Convergence

I first consider the case in which shadow prices are assumed constant across states. This should occur if workers face no cost to relocating and have perfect information of wages in all states so that shadow prices converge via competition. The estimates here are based on the model above, with no interaction with task quantities included, though state indicators are possible control variables. Generally, the results are in line with which tasks we would expect to pay well or poorly, and at what tasks more skilled workers should be more highly compensated.

The precision of the shadow price estimates leaves a little to be desired. Standard errors range from about $0.50 to as high as $20, with most being less than $3. This level of precision is similar for all tasks. This leaves most estimates statistically significant, but for cells with few observations the shadow prices remain uncertain. Given that we expect experience-wage profiles to be roughly quadratic, I fit a quadratic expression to the estimates to visualize the experience-wage effect profiles for tasks. Note that since these are cross-sectional profiles rather than longitudinal, the shape may be irregular due to differences between cohorts, especially if the entering and exiting cohorts are well compensated.

Figure 12 shows quadratic fits in experience of shadow price estimates for each task by education group over time. Overall, Figure 12 suggests that a few changes in task prices are driving skill premia, while for many tasks the shadow prices are fairly low and stable. Several notable results are visible. First, we can see a rise in nonroutine analytic task prices over time in both education groups, more prominently for workers with college degrees and later in the sample period. We can also see a drop in routine manual task prices for both groups. Given that workers with college degrees are more intensive at nonroutine analytic tasks and less at routine manual tasks, these trends work to increase

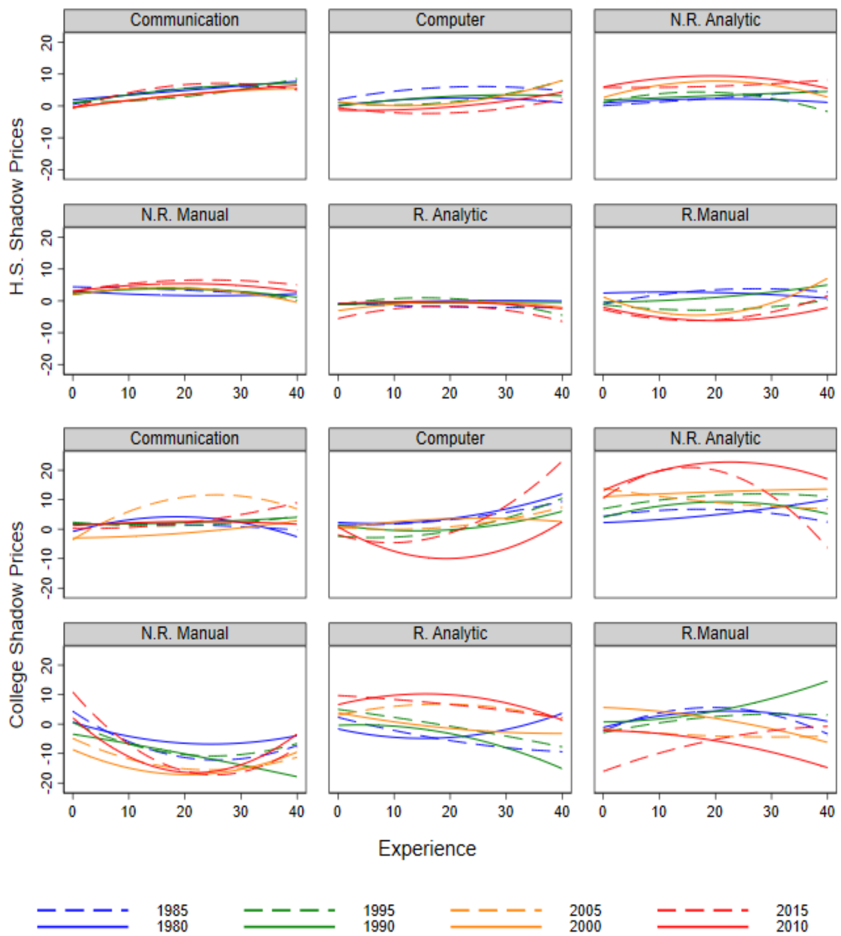

Figure 12. Task Shadow Prices from 1980 to 2015

the college degree premium, especially in the post 2000 period. Additionally, routine analytic prices rose for college graduates but were flat for high school only workers, while computer task prices fell for high school only workers.

Also notable are which tasks have shadow prices that rise with experience. For high school only workers, only computer tasks have a clear upward trend with experience, though this was more prominent in the early sample period. Worth noting is that this result matches the findings of Weinberg (2002), though the author there used substantially different data and methodology than the one used here. For college graduates, an upward trend in experience is evident both for computer tasks and for nonroutine analytic tasks. This is sensible given that these are tasks at which workers are likely to learn from experience. Also visible for college graduates is a downward trend in experience for nonroutine manual tasks, and for routine analytic tasks after the early career stage. This may indicate the workers with college degrees have an initial advantage at these tasks, but their abilities diminish with age.

To give an idea of the degree of smoothing in Figure 12, Figure 13 shows the quadratic fits and the shadow price estimated for nonroutine analytic tasks. The degree of

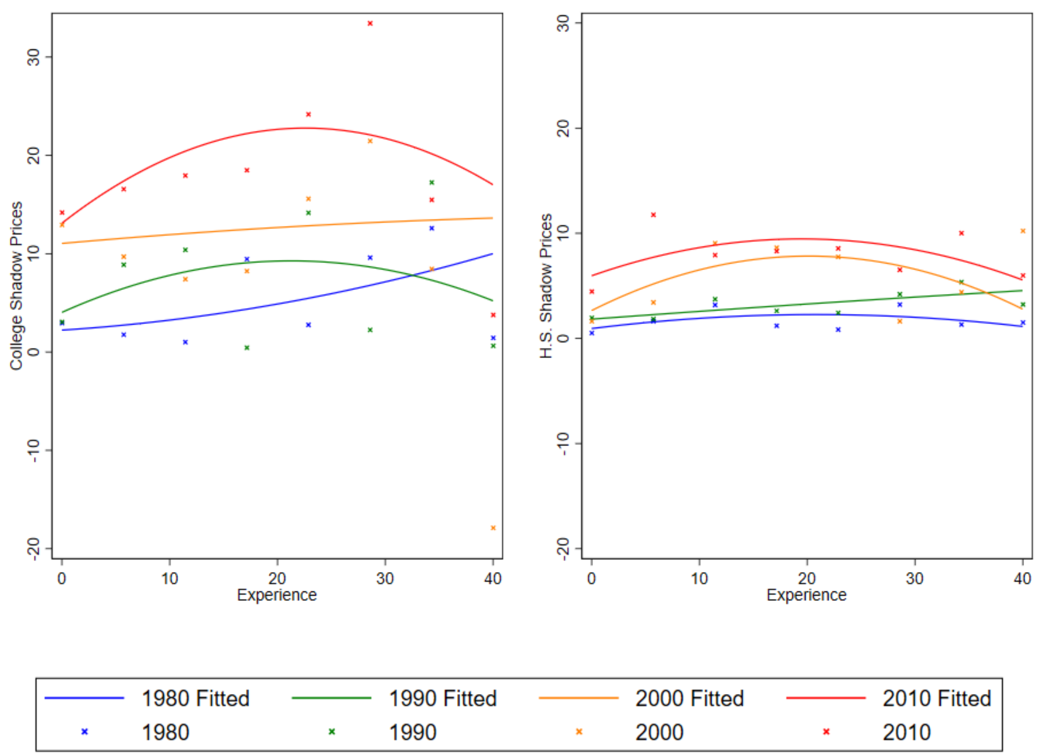

Figure 13. Quadratic Fits vs Shadow Price Estimates

smoothing is moderate. The fitted curves are generally a good representation of the pattern of task shadow prices at a given time and the features discussed above are visible in a scatterplot as long as the graph is large and includes fewer years. Issues such as the outlier in the lower right of the college shadow prices plot are mitigated by using the inverse square of their standard errors as weights. That particular estimate is for the cohort that reached retirement age as the dot com bubble burst, and the shadow price estimate is imprecise, possible due to the unusual conditions of the time.

To confirm my interpretation of Figure 12, I run an Oaxaca decomposition to find the portion of the experience premium attributable to each task price. Table 6 shows partial results from an Oaxaca decomposition of hourly wages as a function of task quantities. This will reveal how much of the difference in wages between experience groups stems from price differences as opposed to task quantities. Overall, the hourly wage premium of experience for college graduates rose from about $3.50 to $4.50 over the sample period, while for high school only workers the premium went from about $1.20 to $1.80. As anticipated, task price differences explain most of the experience wage gap. A surprising finding is that computer task prices consistently served to decrease the overall experience premium.

Aside from computer task prices, the results of the Oaxaca decomposition suggest that communications task prices and routine analytic task prices drove the experience wage premium over the entire sample period, while nonroutine analytic task price increases led to an increase in the experience premium in the post-2000 period. These trends were similar for both high school only and college degree workers. Routine and nonroutine manual task tended to have a modest effect and slightly favored younger workers.

Table 6: Oaxaca Decompositions of Hourly Wages by Experience

	College Degree			High School Degree		
Year	Total Difference	From Prices	From Computer Task Price	Total Difference	From Prices	From Computer Task Price
1980	3.49 (0.19)	3.31 (0.17)	-0.33 (0.54)	1.22 (0.10)	1.10 (0.09)	-0.01 (0.01)
1990	3.30 (0.36)	3.23 (0.36)	-1.16 (0.85)	1.94 (0.14)	1.86 (0.14)	-0.05 (0.21)
2000	4.28 (0.40)	4.45 (0.40)	-1.78 (1.66)	2.11 (0.22)	2.09 (0.21)	-0.67 (0.50)
2010	4.47 (0.39)	4.26 (0.37)	-1.02 (1.96)	1.83 (0.20)	1.79 (0.18)	-0.36 (0.61)

Notes: Standard errors in parentheses. Reference groups are workers with less than 10 years of experience. Comparison groups are workers with more than 20 years of experience. Five other task categories included in the model of wages.

Shadow Price Estimates Allowing State Differences

In practice, we should expect that workers are not perfectly free to move. This may be due to the material costs of moving or coordination problems with moving. Information costs relating to wages in distant labor markets may also make moving less appealing. The fact that geographic mobility has declined in the U.S. in recent decades is well established and known to affect labor market outcomes. See Karahan and Rhee (2014) for discussion. Consequently, differences in task shadow prices between states is entirely plausible.

How to account for these differences is not a trivial question. The obvious approach here is to interact task quantities with state indicators, but should these all be included as policy variables or should these interactions be treated as potential control variables in the DML framework? The answer to this question depends on how the estimates are to be used. Here in chapter 5, we wish to interpret the estimates, so noisy shadow price estimates are a problem. In chapter 6, the estimates are to be used as a dependent variable, and measurement error in the dependent variable is not necessarily a major problem. Random, classical measurement error in the dependent variable makes estimation less precise, but does not bias the results. For the purposes of chapter 5, I will treat the state task quantity interactions as potential controls. This means in effect that most states will have the same task prices, but we will detect differences in task prices for selected states in some cells.

Figure 14 reports the (national) shadow price estimates obtained in the framework discussed above. The results are broadly similar to those seen in Figure 12, but are generally better behaved, even though the degree of precision is nearly identical. We again see a rise in nonroutine analytic task prices for both groups over time and a similar decline in routine manual task prices. College graduates have also seen an increase in wages from routine analytic tasks, which again is a little surprising if we expect computers to substitute for routine analytic tasks. The trends in experience are the same as we saw in Figure 12, though we can also see an upward trend with experience in communication task prices for high school only workers.

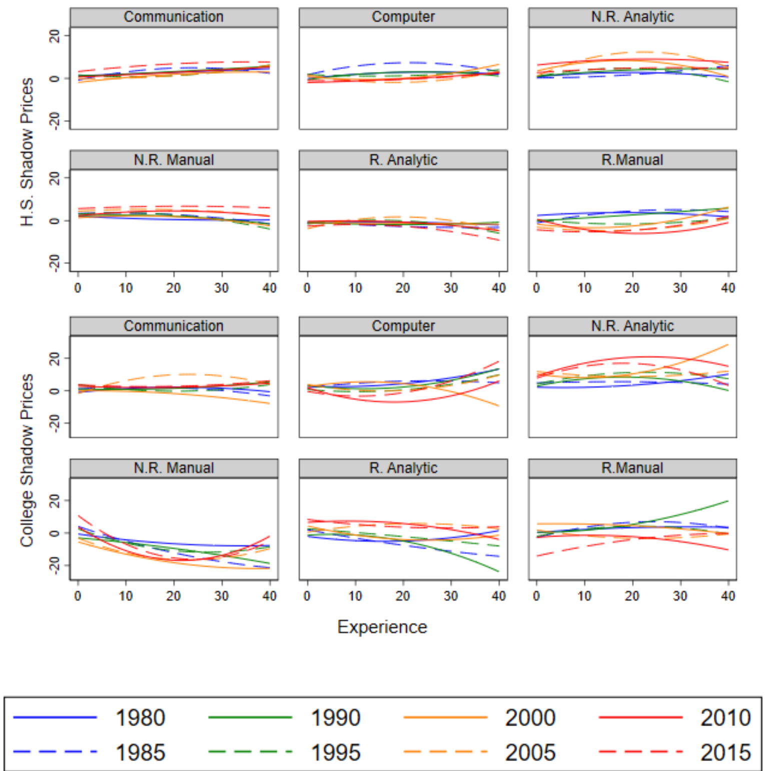

Figure 14. Task Shadow Prices with Controls for State Heterogeneity

We may be interested to see which states had clear deviations from the national prices for tasks. In total, DML suggests there were 1,557 cases in which a state deviated from the national levels of prices, out of 39,168 potential cases. Table 7 lists the top 5 states in terms of how often task prices differed from the national level. These tend to be large states, so lasso may have an easier time detecting price differences when there are many workers observed in a state. This is probably not too serious, as even lightly populated Wyoming occasionally has measurable price deviations from the rest of the country. California was the clear leader, with New York as a distant second. Their task shadow price levels tend to be higher than the rest of the nation, as we might expect. Texas, Pennsylvania, and Florida have smaller price differences and tend to have task prices below other states.

Table 7: State Level Shadow Price Deviations

State	Price Deviation Count	Mean Price Deviation (2015$)
California	414	1.03
New York	237	1.37
Texas	193	-0.19
Pennsylvania	121	-0.12
Florida	108	-0.55

The fairly small mean shadow price differences can conceal some cases where the differences are quite large. There are rare cases of price differences estimated at $40, which we can reasonably assume are estimation errors, but a comparison of Figure 12 and Figure 14 suggest that shadow price differences in a handful of states are having a nontrivial effect on the national level shadow price estimates obtained under the assumption that the shadow prices have converged.

Discussion

Overall, the estimates obtained here should be an improvement over previous work. That said, the interpretation and in some cases the reliability of the estimate bear discussion. DML is a broadly applicable technique, and can be used in both treatment effect and structural frameworks. In this case, I believe that interpretation as a treatment effect is more defensible, but interpreting the wage effects here as marginal productivity effects is fairly reasonable. That is, increasing task intensity causes wages to adjust as described, but the claim that changing productivity is the underlying cause is a further inference.

In a perfectly competitive market, compensation equals marginal product. We lack total compensation data and use wages as the dependent variable, but in this case the control variables should be adequate to account for that, since DML allows a large number of control variables and the March CPS offers a wide selection. We observe pension plan participation, along with a range of demographic variables that should determine preference for nonwage compensation, including age, marital status, and number of children, and when using DML all of these can be interacted with each other.

A potential concern is that in a nontrivial number of cases the results are significant and negative. This could arise for several reasons. First, we should note that since tasks are bundled, the usual condition that marginal product is always positive need not hold. An employer can hire superfluous amounts of a task if the marginal worker still has net positive marginal product after accounting for other tasks. Marginal product can also be negative if the worker treats the task as an amenity. This explanation is implausible for most of the results here. While workers may treat some tasks as amenities, a marginal increase in computer task intensity, for example, is unlikely to be an amenity. The tasks workers would most plausibly treat as amenities are also the ones with consistently positive estimates, communication and nonroutine analytic. Workers may benefit from on the job learning that raises their future productivity. This would suggest that the wage profile should have a noticeable upwards slope and wouldn't explain negative wage effect late in worker's careers. If none of these are the case, then tasks may be correlated with unobserved low ability, meaning the estimates are biased downwards. This last explanation is most plausible in the case low routine manual shadow prices for mid-career college graduates.

I find the most plausible explanation to be that there is a glut of some tasks. The task composition of the workforce has, after all, been surprisingly stable in light of the degree of technological change. This is not a major problem for this chapter, but negative marginal productivity is evidence against a range of production functions, including the nested CES I wish to utilize in chapter 6.

Conclusion

This chapter's accomplishment is to obtain reasonable shadow price estimates for the standard suite of tasks, plus the computer task category introduced in chapter 4. The immediate use of these estimate is to show that task shadow prices explain the lion's share of the wage premia for experience and education. In particular, the results here show that computer task shadow prices substantially mitigated the trend of rising experience premia while rising nonroutine analytic task shadow prices drove the increase in the experience premia on the post-2000 period.

The secondary use the task shadow prices here is that we can now model the evolution of these shadow prices over time. Up to now, what drives the changes in these shadow prices has been unclear and the literature gives us an embarrassment of riches in terms of explanations. Shadow price differences may stem from economies of scale, trends in technology, unobserved human capital differences, or changes in the aggregate labor supply of other tasks. Fortunately, these explanations correspond to different patterns in task shadow prices that can be tested. Modeling the shadow prices obtained here will be the subject of chapter 6.

CHAPTER VI

BIAS IN TECHNICAL CHANGE SINCE 1980

In this chapter I use the data on task shadow prices and labor quantities from chapters 4 and 5 to estimate the bias in technical change. To recap, over the past few decades, the wage gap between high and low skill workers grew substantially. We have a mostly plausible explanation for why in the form of skill-biased technical change (SBTC), but we wish to distinguish between several cases of biased technical change, some of which may fall outside the scope of SBTC.

Technical change here means a time trend in a technology parameter of a production function. The central question is: where is technical change neutral and where is technical change biased? Common assumptions are that technology (represented by parameter A) is factor augmenting, $Y = AF(K, L)$ (Hicks neutral), labor augmenting, $Y=F(K, A_L L)$ (Herrod neutral), or capital augmenting, $Y = F(A_K K, L)$ (Solow neutral)[8]. The feature we require here is that technology is labor augmenting but not necessarily neutral, a fact which is reflected in differences in wage trends between types of labor.

Nonneutral labor augmenting technical change can be illustrated by supposing aggregate labor input L is a function of specific labor inputs, here written as the vector h. SBTC is the claim that we can sort the elements of h into high skill and low skill labor

[8]Attentive readers will notice that if and only if the production function is Cobb-Douglas, these cases are equivalent, and the trends cannot be distinguished based solely on observed output, labor, and capital.

depending on the worker and write aggregate production as $Y = F(K, L(A_s h_s, h_l))$, with A_s trending upwards over time[9]. Task-biased technical (TBTC) change is the case where the elements of *h* can be sorted into tasks, for simplicity suppose two, and for one task the technology parameter has a positive trend higher than the other. The slightly narrower claim of routine-biased technical change (RBTC) is that the task categories are routine and non-routine and that nonroutine tasks have a stronger trend in technology. See Böhm (2020) for a detailed look at the recent literature on RBTC. Note that *h* can be sorted both ways, so we may have

$$Y = F(K, L(A_s A_{nr} h_{s,nr}, A_s h_{s,r}, A_{nr} h_{us,nr}, h_{us,r})),$$

Equation 10

in which both types of bias occur, with *s* and *us* denoting skilled and unskilled, and *nr* and *r* denoting nonroutine and routine.

What I have documented so far is that within education-experience groups, mean task intensities have not changed since 1980 (chapter 4), and that changes in the shadow prices of tasks led to an increase in the wage gap favoring workers with a college degree (chapter 5). In particular, routine analytic task shadow prices rose for college graduates, nonroutine analytic task shadow prices rose across all groups, and routine manual task shadow prices fell across all groups. These findings are less consistent with the classical form of SBTC described in, e.g., Katz and Murphy (1992), with a separate technology

[9]For simplicity, suppose for now that only high skill workers face an upwards productivity trend. In practice we may need to distinguish between trends for each worker type.

parameter for high skilled workers, and more consistent with a task-biased technical change (TBTC) mentioned in Acemoglu and Autor (2011) and in Adermon and Gustavsson (2015), in which the authors attempt but are unable to document such a trend in task-specific productivity based on panel data of Swedish wages.

The dataset constructed in chapters 4 and 5 allows me to estimate time trends in task-specific wages by education-experience groups. I will show that time trends for task productivity are statistically and economically significant, but are usually not statistically significantly different between education groups. I take this as evidence in favor of TBTC over SBTC.

Data and Methodology

The empirical approach here uses two steps. First, I obtain shadow prices for each task, in each year, and for each education-experience group. This is similar to the process detailed in chapter 4, with two key differences. I now estimate state varying shadow prices and I forego the cross-fitting step[10] for computational efficiency. Second, I estimate time trends in these shadow prices and test whether these time trends vary across tasks and education groups. I interpret these time trends as biased technical change, and the types of labor over which the time trends vary will indicate whether recent changes in the wage structure stem from SBTC in its original proposed form or TBTC. A difference

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[10]The consequence of this is an estimator that is $\sqrt{N}$-consistent rather than N-consistent. This could mean that estimates for cells with few observations are far from their asymptotic distributions, but should not affect the asymptotic properties of the estimator unless the number of necessary control variables is rising with N, which I do not expect in this case.

in trend between high school degree only workers and college graduate workers across all tasks indicates SBTC, while a trend difference between the shadow prices of tasks across both education groups indicates TBTC.

Shadow Price Estimation

The state varying shadow prices are estimates from the same dataset constructed in chapter 4 and used in chapter 5. The sample contains 212,561 observations of individual workers from 1980 to 2015, recording their hourly wages, intensities for each task, and a range of demographic and employment data from the CPS and O*NET. The shadow price estimates are obtained via a partialling out method. I regress wages and a set of task intensities interacted with state level indicator variables on a set of control variables selected by lasso. As before, I model wages as

$$w = d\alpha' + x\beta' + \epsilon.$$

Equation 11

The difference here is that d is a set of 51 interactions between a particular task and state level indicator variables, so α' is a vector of state specific shadow prices. Note that I allow parameters to vary by education-experience group, task, state, and year, so we may want to think of these as α'_{ijkst}, though the number of indices quickly becomes cumbersome.

A caveat here is that in this case the goal is not to interpret α' directly. These are estimates of state level shadow prices, but the statistical significance of the price in any particular state is not important. The aim here is to obtain state level variation, even if my estimates have substantial amounts of noise. Recall from chapter 5 that in a given year, few states were detected as having shadow prices that deviated from the national average. Fortunately, the imprecision in the estimate should reflect sampling error, and since the shadow prices act as the dependent variable in step two, this is a case of measurement error in the dependent variable and should be a manageable issue.

To recap the selection process, *w* and each element of *d* are separately regressed on *x*, but the objective function contains a penalty term along with the sum of squared errors. We can write the objective function as

$$Q_{Lasso} = (1/2N) \sum_{i=1}^{N} \epsilon_i^2 + \lambda \sum_{k=1}^{P} |\psi_k \beta_k|$$

Equation 12

Here, ϵ_i and β_k have their usual meanings, while λ and ψ_k are a penalty parameter and weights, respectively, selected to minimize estimation noise under the assumption of heteroskedastic error terms[11]. See Belloni and Chernozhukov (2011), Belloni et al. (2012), and Hastie et al. (2015) for technical details. The basic idea is that by restricting the size of the β's we obtain a model in which most β's are 0. If we happen to know that most of the β's in the true model are 0, this method will identify the relevant regressors, or at least a set of regressors that approximates them arbitrarily well, provided some conditions hold. Once the β's are selected, I obtain in-sample residuals from regressions of *w* and each element of *d* on the relevant elements of *x*, then regress the wage residuals

[11]Specifically, $\lambda=(1.1/\sqrt{N})\Phi^{-1}(1-0.1/(2p\ ln(max\{p,N\})))$ and $\psi_k=\sqrt{(1/N)\sum_{i=1}^{N} (x_{ij}\hat{\epsilon}_i)^2}$, where $\hat{\epsilon}_i$ is obtained by an iterative algorithm.

on the task residuals to estimate α'. See Belloni et al. (2012) for the derivation of this method.

The consistency of the partialling out estimator here depends on two conditions. The first condition is sparsity, i.e., the number of nonzero coefficients in the true model is small relative to the sample size N. The second condition is that we have observed values for all the relevant variables, or strong proxies, even if we do not know which variables these are. Because of the penalty term, the model is identifiable even if the number of control variable candidates, p[12], is much larger than N. The partialling out approach relies on stronger sparsity conditions than the cross-fit partialling out method used in chapter 5. This estimator is $\sqrt{N}$-consistent as opposed to N-consistent. In slightly simplified terms, the number of potential control variables with non-zero coefficients in the true model must be on the order of $\sqrt{N}$. In practical terms, the estimates will be inconsistent in a sample of 10,000 observations if there is no set of 100 control variables sufficient to eliminate omitted variable bias[13]. While more restrictive than the cross fit partialling out method from chapter 5, the number of control variables permitted is still large.

The product of this first step is a dataset of shadow prices and aggregate labor quantities by task, education-experience group, state, and year. For each task, there are 6,528 estimated "observations" of shadow prices, with the quality of the estimates depending on the number of observations in each cell.

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[12] Here, $p=7{,}433$.

[13] More precisely, or perhaps less precisely but more accurately, $s/(N^{-1}/ln(p))$ must be small, where s is the number of regressors in the true model, though there is no exact threshold.

Models of Shadow Prices and Time Trend Estimation

In the second step, I model the shadow prices as a function of aggregate labor quantities. The method here is analogous to the methods used to model wages seen in chapter 3 and much of the literature on SBTC. The canonical framework for this is the nested CES, so I begin with a discussion of this framework before moving to a more general, semiparametric framework.

The nested CES is a fairly simple extension of the standard CES production function model with several highly convenient properties and at least one key limitation relevant to distinguishing SBTC from TBTC. The premise is that the aggregate production function takes on the standard CES form,

$$Y = A(\theta K^{\rho} + (1-\theta) L_1^{\rho})^{1/\rho},$$

Equation 13

where Y, A, K, θ, and ρ have their usual interpretations, but now L_1 is a CES conglomerate of more detailed labor types, usually low and high skill, usually distinguished empirically by education,

$$L_1 = (\theta_1 L_{11}^{\rho_1} + (1-\theta) L_{12}^{\rho_1})^{1/\rho_1}.$$

Equation 14

We can generalize further by allowing L_1 and L_2 to themselves be CES labor conglomerates that depend on narrower types of labor, such as labor by experience group or, in rarer cases, by task. This is a flexible framework in that we can treat different many different types of labor as imperfect substitutes.

Suppose that the nesting structure is education-task-experience, indexed by *i*, *j*, and *k* respectively[14]. We can easily apply the chain rule to obtain the marginal product of labor,

$$MP_{ijk} = \theta_i \theta_{ij} \theta_{ijk} Y^{1-\rho} L_i^{\rho-\rho_i} L_{ij}^{\rho_i-\rho_{ij}} L_{ijk}^{\rho_{ij}-1},$$

Equation 15

where L_{ijk} is the aggregate task labor quantity for the relevant education-experience group. While at first glance, this formula appears complicated, we can divide by a reference type of labor within the *ij* nest and take logs to obtain

$$ln(MP_{ijk}/MP_{ij1}) = ln(\theta_{ijk}/\theta_{ij1})+(\rho_{ij}-1)ln(L_{ijk}/L_{ij1}).$$

Equation 16

This specification is convenient because it is linear, and both the coefficients and the constant have useful interpretations. The coefficient is the elasticity of complementarity, indicating the degree to which labor types are imperfect substitutes, while the constant is a difference in productivity levels. Time trends in the constant are trends in productivity (hence labor demand) that we could link to biased technical change, as pointed out in

[14]The literature on nested CES models generally supposes education is the highest nesting level. See Ottaviano and Peri (2012) for methods of testing the nesting structure. A nesting level for tasks is rarely used, though Haas, Lucht, and Schanne (2013) assume a nesting structure with tasks between education and experience.

Katz and Murphy (1992), though not in a task-based context. Note also that the constant is decomposable in other ways that may be of interest, such as allowing cohort effects or interacting it with lagged task quantities to test for learning on the job.

The key relevant limitation of the nested CES is that marginal productivity can never be negative. As discussed in previous chapters, the task-based model does not in general require the marginal product of tasks to be positive, even in a perfectly competitive labor market. Additionally, chapter 5 shows cases in which shadow prices are negative and significant. In addition to the inability to take logs, negative values mean we lose information by dividing, as we would lose an indication of whose shadow prices were negative and a time trend would be uninterpretable.

Two approaches could resolve the issue of negative shadow prices. If the negative values arise from measurement error or some random shocks, Equation 16 can still be estimated via nonlinear least squares, keeping in the nested CES framework, though this leaves many nuisance parameters in the model. Having attempted this approach, I can attest to its inappropriateness. Estimates are extremely sensitive to initial values, frequently fail to converge, and even when initial values are reasonable and convergence is achieved, the estimates not credible[15]. Alternatively, if the shadow prices for tasks are in some cases negative due to an overabundance of labor, technology trend estimates can still be obtained nonparametrically or semiparametrically. I go forward with the semiparametric approach explained below.

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[15]For example, the education-experience group indicators are not significant, except for one with $t=40$, and $R^2=-6$.

If the aggregate production function is not CES, estimation is still possible in a more generalized framework. Suppose aggregate production is of the form seen in Equation 10, then by the chain rule marginal product is

$$MP_{ijk} = \partial F(K, L(\varphi,h))/\partial h_{ijk} = \varphi_{ijk} f(L)$$

Equation 17

where h_{ijk} is the aggregate task quantity for the education-experience group and task in question, and φ is a vector of labor augmenting technology parameters. In the CES case above, φ_{ijk} could be decomposed into the product of θ_i, θ_{ij}, and θ_{ijk}, while here we have less guidance. I am interested in time trends in φ_{ijk}, here the technology parameter for education-experience group *ik* at task *j*. If *f* is an unknown function, a semiparametric approach is appropriate. As the φ_{ijk} term enters multiplicatively rather than additively, and taking logs is not an option, the relevant approach is the smoothly varying coefficient model (SVCM). Essentially, we can estimate φ_{ijk} in narrow bands of values of aggregate labor quantity[16][17]. See Rios-Avila (2020) for technical details. Centorrino and Racine (2017) use this approach in a context similar to what I do here. The authors model wages as a function of experience, allowing the coefficient to vary by educational attainment. Note that here I suppose that task labor supply is inelastic in the short term. Were this not

[16]The model supposes the coefficients $\beta_x(z)$ vary with z in the form $\beta_x(Z_i) \approx \beta_x(z)+\partial\beta_x(z)/\partial z(Z_i-z)$.
[17]Here, aggregate labor quantity is the total amount of the task for the state, year, and group. I rely on the properties of the task measure from Peri and Sparber (2009) to allow task quantities to be summed over workers.

the case, using a shift share instrument for aggregate task quantities is an alternative approach.

Since in the SVCM framework, we have a linear model at any fixed value of the smoothing variable z (here the labor quantity $f(L)$), we need to specify a linear expression for φ_{ijk}. I assume

$$\varphi_{ijk} = D_k X + a_k t + b_{ik} t * Col,$$

Equation 18

where X is a set of education-experience group indicators, t is time, and *Col* is an indicator for a four year college degree. The parameters of interest here are a_k and b_{ik}. These are the time trend in the shadow price for the task and the difference in time trend by education. I further interpret these as technology trends. A steeper positive time trend for some tasks is indicative of TBTC while a steeper time trend for college graduates than high school only workers across all tasks is indicative of SBTC in the traditional sense, the same as in the nested CES case above.

Strictly speaking, MP_{ijk} is not observed, so I require an empirical stand-in. I make the assumption that $\alpha_{ijk}=MP_{ijk}+v_{ijk}$ within each state and year, where v_{ijk} is an idiosyncratic error term. That is, employers adjust wages by α_{ijk} because that is the size of the productivity increase due to rising task intensity. This assumption leads me to estimate the equation

$$\alpha_{ijk} = (D_k X + a_k t + b_{ik} t * Col) f(L)$$

Equation 19

semiparametrically on each task.

The estimates I obtain from SVCM are coefficients that vary by quantity of labor. This heterogeneity may be interesting, but here is a byproduct of the estimation technique rather than the focus of the investigation. Note that the marginal effect of a technology trend on wage levels will depend on the aggregate labor quantity in any plausible model. The nested CES approach, and others, avoids the issue by transforming the equation in a way that is not possible here, to isolate the parameters of interest in the constant term. Recalling that *f(L)* is the derivative of the production function with regard to labor, we should expect *f(L)* to be positive and decreasing, so all else equal the coefficients should decrease in absolute value with labor quantity. Deviations from this patter may indicate heterogeneity in the marginal effects apart from what would occur in a nested CES framework.

When interpreting the results, converting the coefficient estimates into marginal effects is helpful. We can obtain marginal effects at the mean by multiplying the coefficients by the local mean task intensity for the relevant group. Table 8 reports mean task intensity by education and task for each task category except computers. As an example, suppose the coefficient on the time trend is 1 for all labor quantities and the mean task quantity is 0.7 for college graduate and 0.4 for high school only graduates across all states, experience groups, and years. Then the trend in that task raised college graduates' wages by $0.70 every 5 years and high school only graduates by $0.40 every 5 years. For most purposes, mean task intensity varies little by experience or over time. See chapter 4 if more a more detailed breakdown of mean task quantities is needed.

Table 8. Mean Task Intensity by Education

	N.R. Analytic	R. Analytic	Communication	R. Manual	N.R. Manual
College	0.69	0.52	0.70	0.34	0.28
High School	0.45	0.54	0.45	0.60	0.57

Before proceeding, a few caveats bear mentioning. First, the SVCM approach can yield imprecise estimates at extreme values of the distribution of *z*. Coupled with the imprecision of estimates for cells with few observations, this leads to a high degree of noise for small labor markets. Results shown and discussed below are restricted to the upper 80% of the distribution of the relevant *z* variables. In practice this leads to excluding small states such as Wyoming and the Dakotas. Most of the variation in *z* is driven by state populations, as opposed to differing cohort sizes and task intensities within states. Second, the commonly used SVCM method allows only one *z* variable. Additional variables increase complexity exponentially. Results below use within education-experience group aggregate labor quantities for *z*[18] Aggregating across all experience groups gives similar results. Finally, interpreting the time trends as bias in technical change assumes I am estimating labor demand. I am relying on the inelasticity of supply for the labor quantities defined here.

[18] That is, I aggregate individual task quantities across all workers in each education-experience group, state, and year, using the method discussed in chapters 4 and 5 and based on Peri and Sparber (2009).

Results

Figure 15 plots the coefficients on the time trends for each task. For the most part, we can think of the x-axis as having small states on the left while large states like California and New York are on the right and more closely reflect national averages. Note that here, a 1 indicates an upward trend in hourly wages of $1 per 5 years for all workers intensive at the task (i.e. task intensity is 1), given the indicated aggregate labor quantity. We can see that each task tends to have a clearly positive or negative trend, with

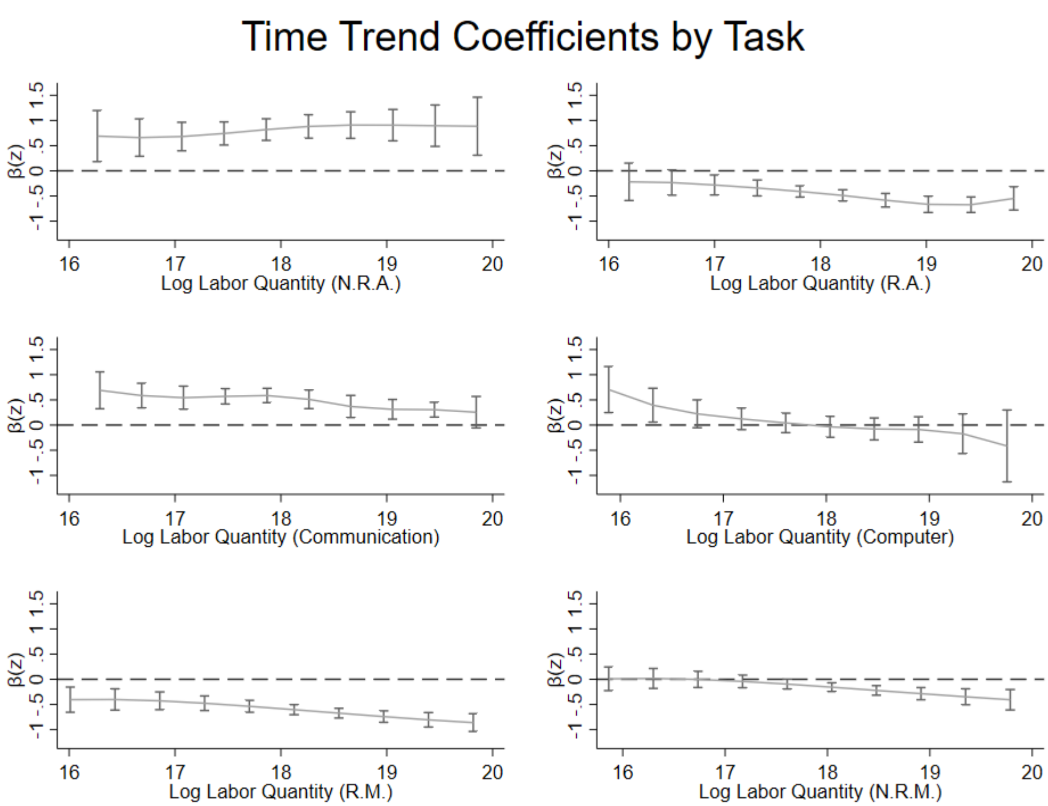

Figure 15. Time Trend Coefficients by Task
(95% Bootstrap CIs)

some variation by the aggregate quantity of labor. For the most part, the sign of the trend is positive for nonroutine tasks and negative for routine tasks, as we may expect based on the literature.

Nonroutine analytic and communication tasks exhibit a similar pattern of positive time trends across all observed labor quantities, supporting claims of TBTC. Communication tasks exhibit the pattern we would expect to see under the standard model: positive and decreasing with *f(L)*. Workers intensive in nonroutine analytic tasks saw a wage increase of about $6 per hour over the previous 4 decades, while workers intensive in communications task saw an increase of about $4.80 per hour. Variation in these effects by *f(L)* is modest. For nonroutine analytic, the effect appears larger for markets where the aggregate quantity of that task is larger, while the reverse is true for communications tasks. This is not directly of interest here, but does imply that averaged across all workers the effect of nonroutine analytic task is larger and more noticeable.

Also supporting the TBTC position are negative trends for routine analytic and routine manual tasks. Workers intensive in these tasks saw wages fall by $4 to $5 over the sample period. This decline was larger in bigger labor markets, making it especially noticeable. A negative technology trend may be confusing at first, but we must recall that technology is defined in the broad economic sense here. This may reflect issues like bureaucracy that leads labor inputs to be less productive, which has plausibly worsened for routine work and more so in larger states, though establishing this connection would require further investigation.

While the results above support both TBTC and RBTC, the remaining two task categories do not fit easily into a RBTC framework. Nonroutine manual tasks exhibit a trend similar to routine manual tasks, though less steep. For small labor markets, the trend was near zero over the sample period, but in larger labor markets, the trend was significant and negative to the tune of about $0.60 per decade. If technical bias was strictly in favor of nonroutine tasks, this should not be the case. Additionally, computer tasks have a positive trend for small labor markets, but generally the trend is not statistically significant, and any plausible effect is small compared to other tasks.

Figure 16 shows the difference in time trends by education group. Here, a 1 indicates that workers intensive in this task with a four-year college degree saw wages rise at a rate of $1 over 5 years higher than the rate for similar workers with only a high school degree. Positive values indicate a bias towards skill within a particular task category. Overall, the results here suggest that in most cases such bias is small or nonexistent, though there is a pattern of significant and positive trend differences in larger labor markets.

Routine and nonroutine analytic tasks, along with computer and communications tasks, exhibit highly similar patterns. The difference in trends is near zero, but rises with aggregate labor quantity until the difference in trends becomes positive for larger labor markets. The size of these effects is also similar, at between $0.50 and $1 per 5 years. This is a somewhat surprising result in that it supports pure SBTC, but only in some labor markets. That said, the evidence is not especially strong given the number of estimates produced here.

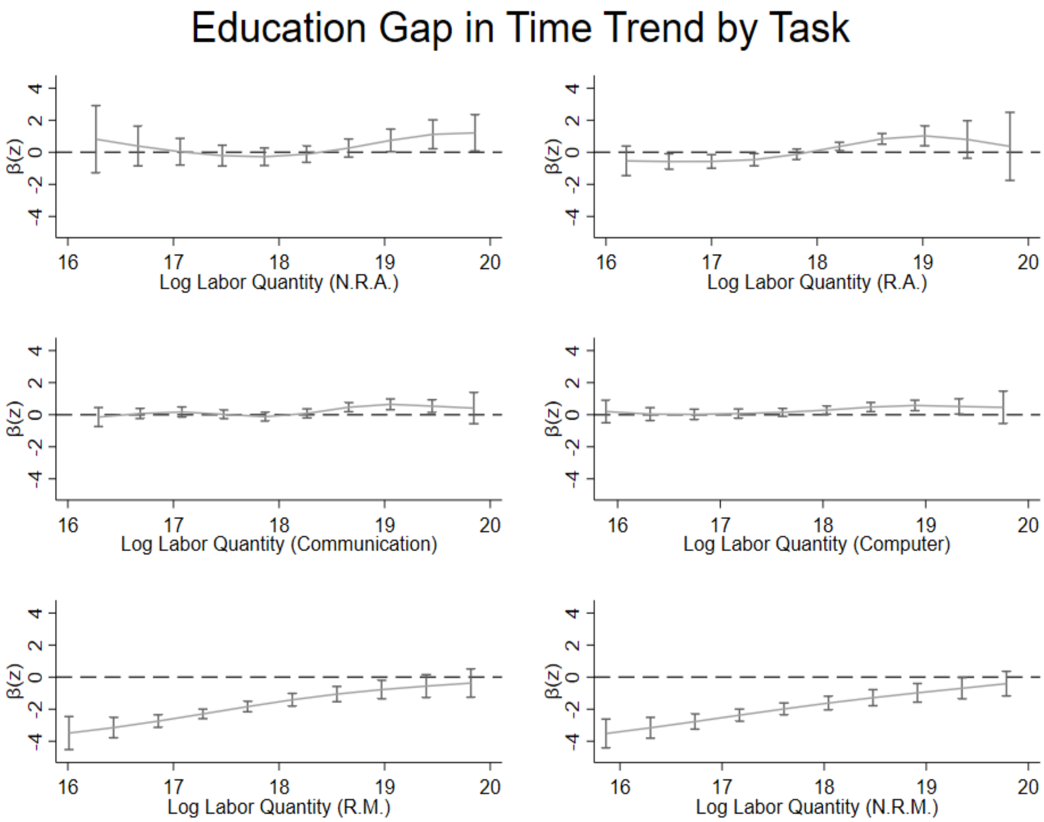

Figure 16. Education Gap in Time Trend by Task
(95% Bootstrap CIs)

The remaining task categories, routine and nonroutine manual, also show a common pattern in the difference in trends. Where the aggregate labor quantity is small, the trend is strongly negative, while for larger labor markets the trend is near zero. Before interpreting this, we should note that the practical effect here is modest, both because college graduates do these tasks with low intensity and because the large negative trend exists only in small labor markets. That is, the results imply hourly wages fell by about $1 per 5 years for typical college graduates that work in small labor markets, ignoring other factors influencing their wages.

Conclusions

In this chapter I estimated trends in the shadow prices of wages. I interpret time trends within tasks as task-biased technical change and differences in time trends by education groups as skill-biased technical change. On balance, the results here support the case for task-biased technical change in favor of nonroutine analytic and communications tasks and against routine analytic and routine and nonroutine manual tasks. The results are not supportive of skill-biased technical change in general, but may support skill-biased technical change for large labor markets.

Taken together with the findings in chapter 4 that showed that mean task intensities within education-experience groups were stable over time, the results here suggest that the rising wage gap between skilled and unskilled workers was driven largely by the positive time trend in the shadow prices of nonroutine analytic and communications tasks that are disproportionately done by workers with a college degree. Other factors were generally small or affected workers similarly across education groups.

CHAPTER VII

CONCLUSION

In this dissertation, I measured the task intensity of workers and used them first to estimate shadow prices for tasks, and then to estimate trends in technology reflecting bias in technical change. The process revealed several interesting results that warrant investigation in the future, and overall supported other findings in the literature regarding the nature of technical change. I briefly discuss these findings and their implications below.

In chapter 4, I used established techniques to obtain a measure of task intensity for a standard suite of 5 tasks, and for the first time, a computer task category. The most notable finding was that mean relative task intensities within education-experience groups saw only small fluctuations over time, except for computer tasks. This means the average college graduate and the average high school only graduate of a given experience level have about the same task profiles today as 40 years ago, aside from both using computers more extensively. This strongly suggested that changes in the wage gap between college graduates and high school only graduates are due to changes in how well compensated tasks are, rather than changes in the quantities of tasks.

Chapter 4 also showed that the task specialization in local labor markets changed substantially since 1980. Some areas specialized in nonroutine analytic and communication tasks, while others specialized in manual tasks. This was not thoroughly explored here, but suggests that the mix of task quantities in local labor markets is an important and understudied factor in labor market outcomes, and that the methodology used here could be a valuable tool in explaining local variation in labor market conditions.

In chapter 5, I estimated shadow prices for tasks using the recently developed DML estimator. The results of chapter 5 are about what we would expect. Routine manual tasks declined in price while nonroutine analytic prices rose, especially for college graduates. Less predictably, computer task prices fell, and routine analytic task prices rose for college graduates only. The results are notable in that shadow prices have not previously been reported on this large a suite of tasks.

Chapter 5 also brings a few issues to light that have yet to be fully addressed by the literature. Foremost, task shadow prices can be negative in equilibrium. This has barely been mentioned and never been a focus. Previous attempts tended to use estimation techniques that could not produce negative estimates, though theory does not indicate negative values are impossible in this framework. This result has implications for which identification strategies are appropriate in determining the nature of technical change, as seen later in chapter 6.

A few minor points also reveal themselves in chapter 5. While most states have shadow prices close to the national mean, in some cases even large states can experience a price differential. These differences do not seem to persist over time, so likely reflect temporary market imperfections. A final minor point is that the experience-shadow price profile fluctuates over time more for some tasks than others, and not all have the usual concave shape. In the future, the ability to measure this could help clarify how human capital is accumulated on the job.

In chapter 6, I estimated bias in technical change using the task labor quantities and shadow prices derived in previous chapters and a semiparametric approach, SVCM, as an alternative to the nested CES. I distinguish between SBTC, TBTC, and RBTC based on trends in shadow prices within and between groups. On balance, I find evidence in favor of TBTC, some evidence of SBTC, and evidence against general RBTC. This is notable both in that I document significant trends in shadow prices and in that the trends suggest that the shift in the literature away from SBTC as originally envisioned was warranted, but that making routineness and non-routineness the primary categories may be a misstep, though the distinction is important.

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APPENDIX A. FREEMAN REPLICATION REGRESSIONS

Freeman Model Estimated on CPS data, 1961 to 1975

1961 to 1974				
Sample	Constant	Time	Cycle	Labor Ratio
Full-time Workers, 25 to 35	0.063 (0.017)	-0.005 (0.002)	0.006 (0.242)	-0.125 (0.114)
Four-year Degree Workers	27.113 (12.475)	-0.014 (0.006)	-0.177 (0.639)	-0.298 (0.214)
High School Graduate Workers	0.066 (5.712)	0.000 (0.003)	-0.237 (0.405)	-0.150 (0.205)
Full-time Workers, 20 to 24	-0.720 (0.179)	-0.002 (0.008)	0.448 (0.338)	-0.491 (0.150)

Freeman Model Estimated on CPS data, 1975 to 1996

Sample	Constant	Time	Cycle	Labor Ratio
Full-time Workers, 25 to 35	0.015 (0.026)	-0.005 (0.002)	0.657 (0.156)	-0.088 (0.054)
Four-year Degree Workers	-42.418 (13.129)	0.008 (0.002)	-0.083 (0.289)	-0.042 (0.089)
High School Graduate Workers	9.138 (5.922)	-0.005 (0.001)	0.652 (0.162)	-0.197 (0.037)
Full-time Workers, 20 to 24	0.483 (0.182)	-0.020 (0.001)	1.518 (0.284)	-0.056 (0.070)

Freeman Model Estimated on CPS data, 1996 to 2017

Sample	Constant	Time	Cycle	Labor Ratio
Full-time Workers, 25 to 35	-0.211 (0.049)	-0.005 (0.002)	-0.283 (0.086)	-0.132 (0.079)
Four-year Degree Workers	-31.310 (13.530)	0.002 (0.003)	0.714 (0.296)	0.208 (0.081)
High School Graduate Workers	-15.732 (8.272)	0.008 (0.003)	0.738 (0.242)	0.122 (0.046)
Full-time Workers, 20 to 24	-0.269 (0.226)	0.005 (0.003)	0.655 (0.259)	0.370 (0.242)

Extensions to Freeman Model, CPS 1961 to 1996

	H-P Filter		H-P Filter, AR(1)	
	High School	College	High School	College
Constant	5.414 (3.062)	32.455 (9.006)	2.043 (1.958)	26.371 (9.111)
Labor Ratio pre '75	-0.030 (0.163)	-0.295 (0.215)	-0.170 (0.073)	-0.766 (0.215)
GNP pre '75	-0.109 (0.380)	0.438 (1.958)	-0.300 (0.305)	0.454 (1.438)
Unemployment pre '75	-0.018 (0.006)	-0.011 (0.032)	-0.018 (0.006)	-0.031 (0.039)
Time pre '75	-0.003 (0.002)	-0.017 (0.005)	-0.001 (0.001)	-0.013 (0.005)
Post '75	3.185 (3.661)	-48.617 (9.688)	5.493 (3.087)	-43.457 (10.860)
Labor Ratio post '75	-0.020 (0.049)	-0.034 (0.082)	-0.183 (0.061)	-0.043 (0.095)
GNP post '75	-0.358 (1.208)	1.399 (1.494)	-0.298 (1.192)	1.449 (1.580)
Unemployment post '75	-0.024 (0.024)	0.043 (0.032)	-0.022 (0.023)	0.045 (0.034)
Time post '75	-0.004 (0.001)	0.008 (0.002)	-0.004 (0.001)	0.008 (0.003)
Lag			0.102 (0.123)	-0.069 (0.229)
R-squared	0.9377	0.7387	0.9488	0.7666
N	34	34	32	32

APPENDIX B. SHADOW PRICE DML ESTIMATES

Shadow Price Estimates 1980

	Exp.	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	0.538 (0.896)	-1.156 (0.815)	2.435 (0.609)	2.028 (0.751)	0.311 (0.534)	1.767 (0.759)
	6-10	1.664 (0.795)	-0.744 (0.864)	3.831 (0.700)	2.709 (0.886)	-0.008 (0.803)	3.512 (0.816)
	11-15	3.206 (0.897)	0.749 (0.884)	2.128 (0.792)	4.415 (0.956)	2.348 (0.813)	1.99 (0.925)
	16-20	1.233 (1.129)	-0.804 (1.024)	1.301 (0.871)	3.253 (1.047)	3.201 (0.935)	5.558 (.952)
	21-25	0.867 (1.239)	-1.25 (1.095)	0.952 (1.041)	0.595 (1.173)	1.623 (1.081)	6.09 (1.075)
	26-30	3.25 (1.453)	0.596 (1.29)	1.582 (1.743)	1.454 (1.297)	1.944 (1.286)	5.472 (1.725)
	31-35	1.348 (1.326)	1.456 (1.198)	1.506 (1.118)	0.66 (1.25)	3.458 (1.187)	4.863 (1.233)
	36-40	1.538 (1.481)	-1.077 (1.465)	2.987 (1.202)	3.464 (1.783)	-0.277 (1.498)	10.311 (1.801)
College	0-5	2.971 (1.367)	-1.361 (1.268)	-0.671 (1.858)	0.117 (1.635)	2.547 (0.978)	-0.53 (1.246)
	6-10	1.79 (1.508)	-4.519 (1.56)	-2.025 (1.448)	0.059 (1.531)	.635 (1.157)	0.046 (1.466)
	11-15	1.041 (2.174)	-3.676 (2.224)	-0.853 (2.093)	1.008 (2.203)	5.29 (1.687)	8.461 (2.139)
	16-20	9.484 (3.055)	-4.691 (3.026)	-9.782 (3.318)	8.251 (3.006)	1.576 (2.526)	0.356 (2.515)
	21-25	2.790 (3.94)	-3.535 (4.951)	-9.997 (4.744)	2.927 (4.343)	3.541 (3.168)	7.323 (3.985)
	26-30	9.635 (3.952)	-6.05 (4.156)	-13.838 (4.426)	12.335 (3.894)	6.911 (3.308)	3.738 (4.083)
	31-35	12.626 (4.613)	9.519 (5.28)	-6.582 (5.839)	-5.85 (4.743)	2.835 (4.077)	-6.415 (4.703)
	36-40	1.465 (5.997)	-3.064 (6.562)	0.550 (4.297)	0.593 (6.594)	16.202 (4.283)	3.536 (6.022)

Shadow Price Estimates 1985

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	0.003 (0.712)	-1.765 (0.933)	3.762 (0.716)	-0.338 (0.898)	2.390 (0.816)	0.657 (0.763)
	6-10	1.102 (0.918)	0.002 (0.754)	5.240 (0.664)	-0.730 (0.921)	3.097 (0.842)	2.053 (0.743)
	11-15	1.283 (1.010)	-2.114 (1.008)	3.506 (0.948)	2.631 (1.065)	4.883 (1.167)	3.571 (1.079)
	16-20	2.342 (1.283)	-1.936 (1.103)	3.445 (0.985)	5.336 (1.258)	5.895 (1.397)	6.709 (1.128)
	21-25	1.944 (1.439)	-2.953 (1.199)	3.094 (1.169)	3.711 (1.265)	6.404 (1.313)	5.848 (1.260)
	26-30	4.093 (1.703)	-1.397 (1.639)	0.546 (1.442)	2.046 (1.676)	5.910 (1.497)	4.952 (1.360)
	31-35	4.198 (1.624)	-1.956 (1.567)	3.259 (1.337)	2.348 (1.592)	6.985 (1.893)	6.215 (1.360)
	36-40	4.760 (3.966)	-1.590 (2.601)	1.588 (1.584)	4.418 (2.018)	2.917 (2.472)	5.961 (1.762)
College	0-5	4.362 (1.688)	1.668 (1.605)	2.025 (2.456)	-0.907 (2.263)	0.655 (1.404)	1.945 (1.879)
	6-10	5.098 (1.885)	-0.043 (1.523)	-0.632 (1.727)	0.031 (1.904)	2.781 (1.369)	0.763 (1.717)
	11-15	7.600 (2.172)	-1.118 (1.884)	-5.662 (1.861)	3.651 (2.183)	2.497 (1.831)	3.492 (1.937)
	16-20	6.665 (3.142)	-6.153 (2.603)	-14.443 (2.486)	9.252 (3.028)	0.550 (2.190)	2.152 (2.707)
	21-25	9.039 (4.436)	-6.780 (4.019)	-8.779 (4.625)	1.757 (4.876)	2.618 (3.487)	1.568 (4.055)
	26-30	-1.443 (5.957)	-11.229 (5.492)	-16.279 (5.668)	6.617 (4.739)	6.301 (4.851)	-6.958 (4.787)
	31-35	-1.037 (6.508)	-15.307 (5.434)	-6.896 (6.797)	-1.114 (6.310)	7.651 (4.734)	2.030 (5.330)
	36-40	10.330 (7.540)	5.056 (7.907)	-4.723 (7.697)	-4.833 (6.924)	11.398 (6.645)	3.439 (6.363)

Shadow Price Estimates 1990

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	1.983 (1.047)	-1.287 (0.902)	1.587 (0.966)	0.261 (0.932)	-0.383 (0.886)	0.933 (0.950)
	6-10	1.868 (0.827)	-0.865 (0.920)	3.658 (0.771)	-0.206 (1.142)	1.637 (0.805)	2.955 (0.854)
	11-15	3.756 (1.130)	-0.345 (0.864)	4.233 (0.846)	-2.798 (1.198)	1.821 (0.886)	2.064 (0.974)
	16-20	2.645 (1.425)	-1.268 (1.408)	3.242 (1.074)	2.209 (1.441)	2.211 (1.035)	6.478 (1.199)
	21-25	2.462 (1.367)	-0.663 (1.780)	5.046 (1.305)	4.389 (2.066)	4.565 (1.235)	7.426 (1.445)
	26-30	4.226 (1.698)	-1.312 (1.600)	2.200 (1.300)	4.020 (1.633)	2.871 (1.481)	6.552 (1.366)
	31-35	5.384 (1.642)	0.755 (1.807)	-1.835 (1.460)	2.945 (1.754)	1.985 (1.651)	3.992 (1.614)
	36-40	3.256 (2.347)	-1.044 (2.865)	4.670 (1.568)	3.668 (2.106)	4.605 (2.010)	9.158 (1.903)
College	0-5	3.089 (1.635)	-1.622 (1.641)	-3.178 (1.698)	0.109 (2.164)	2.053 (1.386)	0.877 (1.835)
	6-10	8.911 (2.047)	1.440 (1.546)	-5.502 (1.827)	1.864 (2.221)	-2.526 (1.777)	3.650 (1.735)
	11-15	10.417 (2.419)	1.004 (2.074)	-7.727 (2.006)	2.140 (2.618)	-0.413 (1.985)	-1.166 (2.215)
	16-20	0.470 (2.831)	-8.269 (2.412)	-7.491 (2.120)	3.416 (2.520)	5.026 (2.126)	4.249 (2.276)
	21-25	14.188 (3.224)	-4.626 (3.173)	-19.462 (4.204)	7.906 (3.752)	-5.694 (2.956)	-2.732 (4.048)
	26-30	2.277 (5.920)	-4.100 (4.998)	-5.559 (4.342)	0.910 (5.478)	1.484 (3.498)	6.044 (3.822)
	31-35	17.283 (6.059)	1.950 (5.520)	-20.739 (6.292)	7.011 (5.794)	-3.733 (5.183)	-5.189 (6.894)
	36-40	0.661 (6.484)	-22.777 (5.804)	-18.843 (8.701)	21.359 (6.763)	10.438 (4.555)	6.682 (5.602)

Shadow Price Estimates 1995

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	-0.080 (1.379)	-1.165 (1.356)	0.565 (1.418)	-0.638 (1.840)	1.210 (1.063)	0.912 (1.371)
	6-10	4.228 (1.422)	-0.131 (1.484)	5.210 (1.486)	-4.150 (1.859)	0.562 (1.497)	1.000 (1.275)
	11-15	5.505 (1.594)	1.653 (1.399)	3.868 (1.077)	-1.079 (1.624)	-0.644 (1.058)	3.246 (1.337)
	16-20	3.431 (1.602)	-0.134 (1.430)	4.377 (1.102)	-2.523 (1.799)	1.851 (1.373)	2.652 (1.237)
	21-25	4.277 (2.033)	2.312 (1.854)	3.059 (1.423)	-6.805 (2.566)	2.149 (1.342)	0.973 (1.531)
	26-30	0.450 (2.586)	-4.599 (2.778)	2.587 (3.273)	1.324 (3.352)	3.240 (1.345)	5.475 (1.659)
	31-35	-2.009 (2.773)	-1.554 (1.757)	0.646 (1.546)	-0.731 (2.271)	5.371 (1.838)	7.729 (1.992)
	36-40	2.199 (2.998)	-4.822 (3.274)	2.928 (2.382)	-0.455 (3.451)	7.482 (2.508)	8.053 (2.742)
College	0-5	5.923 (2.449)	5.424 (1.897)	1.611 (2.162)	-3.752 (2.290)	-1.869 (1.999)	2.724 (1.966)
	6-10	10.000 (2.825)	2.877 (2.083)	-6.141 (2.216)	1.408 (3.036)	-3.254 (2.150)	0.752 (2.127)
	11-15	12.437 (3.287)	-0.769 (3.144)	-6.420 (2.607)	1.298 (3.233)	-3.571 (2.697)	1.850 (2.474)
	16-20	10.259 (3.033)	2.779 (3.041)	-11.54 (3.129)	0.945 (3.496)	-2.251 (2.406)	0.104 (2.819)
	21-25	10.213 (3.595)	0.765 (3.180)	-4.832 (3.264)	0.762 (3.937)	2.233 (2.627)	4.898 (3.104)
	26-30	11.479 (5.144)	-6.726 (4.170)	-17.036 (4.158)	6.979 (5.108)	2.207 (3.962)	-1.824 (4.137)
	31-35	10.792 (5.680)	-14.163 (7.251)	-7.820 (5.233)	-1.010 (6.846)	9.819 (5.135)	1.565 (5.819)
	36-40	14.802 (7.392)	-2.141 (7.294)	-6.447 (6.757)	6.889 (8.548)	3.385 (7.605)	6.671 (6.704)

Shadow Price Estimates 2000

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	1.656 (1.628)	-4.120 (2.297)	-0.384 (3.261)	2.476 (3.478)	1.011 (1.355)	-0.360 (2.628)
	6-10	3.454 (2.845)	-0.454 (1.941)	7.383 (2.934)	-4.747 (3.919)	5.017 (2.365)	1.682 (2.352)
	11-15	9.082 (1.496)	-0.521 (1.493)	3.372 (1.584)	-3.004 (2.050)	-1.323 (1.434)	1.403 (1.623)
	16-20	8.653 (1.570)	-2.274 (1.670)	4.118 (1.367)	-6.452 (2.288)	-0.002 (1.402)	2.746 (1.472)
	21-25	7.778 (1.792)	1.507 (1.661)	4.047 (1.422)	-3.290 (1.909)	1.042 (1.170)	4.827 (1.671)
	26-30	1.660 (2.078)	-1.770 (1.965)	2.864 (1.383)	-1.058 (2.008)	4.145 (1.421)	4.765 (1.694)
	31-35	4.427 (2.051)	-0.480 (2.067)	1.088 (1.620)	2.953 (1.973)	5.559 (1.501)	5.443 (1.687)
	36-40	10.254 (3.480)	-2.159 (3.621)	0.332 (2.511)	5.670 (2.977)	6.524 (2.346)	4.241 (3.451)
College	0-5	12.963 (3.362)	5.406 (2.832)	-11.211 (3.367)	6.319 (3.728)	-1.530 (3.121)	-8.055 (3.869)
	6-10	9.730 (4.207)	-1.127 (2.412)	-11.90 (2.534)	6.379 (3.558)	4.091 (3.040)	2.309 (3.681)
	11-15	7.434 (5.412)	4.975 (3.112)	-11.02 (2.903)	-1.529 (4.104)	2.841 (3.623)	1.314 (3.581)
	16-20	8.260 (4.852)	-2.719 (4.150)	-19.638 (3.044)	3.386 (3.940)	4.029 (3.584)	-2.639 (3.720)
	21-25	15.618 (5.191)	-2.791 (4.559)	-19.748 (3.657)	4.122 (5.236)	1.527 (3.798)	-4.955 (4.383)
	26-30	21.491 (4.894)	-1.799 (4.864)	-16.43 (3.786)	2.062 (5.557)	3.408 (3.641)	-5.809 (4.720)
	31-35	8.477 (9.030)	-3.918 (6.967)	-9.989 (7.826)	-6.075 (9.454)	0.263 (7.255)	9.739 (7.140)
	36-40	-17.86 (16.287)	-3.166 (9.926)	-3.248 (10.621)	-16.803 (14.702)	8.138 (8.182)	9.066 (9.581)

Shadow Price Estimates 2005

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	1.322 (1.520)	-2.391 (1.823)	5.499 (1.642)	-2.667 (2.020)	1.549 (1.428)	2.321 (1.896)
	6-10	6.837 (1.924)	-0.374 (2.085)	0.881 (2.128)	-3.097 (3.138)	0.487 (1.708)	-3.543 (2.003)
	11-15	12.943 (3.262)	2.692 (2.594)	6.112 (3.547)	-7.603 (3.672)	-2.219 (1.659)	-1.651 (3.426)
	16-20	13.027 (1.472)	4.637 (1.634)	3.898 (1.395)	-7.236 (1.881)	-3.342 (1.310)	0.466 (1.283)
	21-25	10.394 (1.787)	1.296 (1.933)	5.914 (1.218)	-4.966 (1.755)	0.362 (1.210)	2.962 (1.361)
	26-30	10.17 (1.686)	0.235 (1.839)	4.898 (1.427)	-0.678 (1.820)	0.716 (1.396)	6.694 (1.361)
	31-35	8.618 (2.684)	-0.573 (2.330)	4.958 (1.616)	-1.595 (3.269)	2.066 (1.710)	7.359 (2.276)
	36-40	10.705 (3.812)	0.449 (3.217)	4.349 (2.268)	-4.385 (3.309)	2.018 (3.698)	5.957 (2.530)
College	0-5	14.442 (2.411)	6.134 (2.890)	-5.224 (2.277)	-2.348 (3.483)	0.040 (2.252)	-3.683 (3.509)
	6-10	5.651 (5.194)	1.330 (2.662)	-6.700 (2.904)	-1.884 (4.431)	4.148 (3.337)	1.666 (4.159)
	11-15	12.348 (4.372)	5.017 (2.679)	-14.75 (2.597)	-1.573 (4.546)	-2.363 (3.401)	6.608 (4.016)
	16-20	11.891 (5.812)	10.986 (3.805)	-11.142 (3.782)	-4.141 (5.546)	-1.181 (4.378)	16.28 (4.759)
	21-25	13.049 (7.180)	3.506 (4.006)	-23.081 (4.009)	0.748 (6.132)	-2.254 (5.193)	9.702 (5.351)
	26-30	5.819 (6.364)	10.116 (3.534)	-12.002 (3.374)	-12.224 (4.864)	5.488 (4.303)	4.879 (3.976)
	31-35	0.193 (7.850)	3.928 (4.482)	-9.422 (4.333)	3.415 (7.471)	8.713 (4.791)	25.82 (6.766)
	36-40	14.005 (10.13)	-5.620 (6.765)	-21.458 (7.367)	-3.027 (7.377)	1.346 (6.941)	3.895 (6.493)

Shadow Price Estimates 2010

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	4.488 (1.415)	-0.839 (1.933)	4.026 (1.668)	-2.228 (3.030)	-0.817 (1.452)	1.368 (2.177)
	6-10	11.781 (1.776)	-1.256 (2.130)	2.454 (1.417)	-4.127 (2.182)	-2.786 (1.517)	-1.337 (1.869)
	11-15	7.950 (1.907)	-0.073 (2.591)	6.350 (1.434)	-4.936 (3.258)	1.403 (2.413)	1.835 (1.850)
	16-20	8.324 (4.150)	1.498 (2.994)	2.145 (2.554)	-2.087 (3.821)	0.224 (2.397)	4.252 (2.335)
	21-25	8.589 (1.998)	1.184 (1.881)	6.544 (1.449)	-9.640 (2.662)	-0.630 (1.528)	5.060 (1.720)
	26-30	6.551 (2.207)	-4.003 (2.348)	3.930 (2.126)	-5.589 (1.903)	1.515 (1.682)	3.097 (2.441)
	31-35	10.043 (2.626)	-3.961 (3.087)	4.322 (1.742)	0.343 (3.274)	3.199 (2.623)	6.128 (2.351)
	36-40	6.011 (5.500)	2.067 (3.994)	2.834 (1.947)	-3.493 (2.711)	4.076 (2.302)	6.251 (2.759)
College	0-5	14.214 (2.624)	6.701 (2.094)	1.383 (2.570)	-3.070 (3.429)	-0.827 (2.677)	0.541 (2.398)
	6-10	16.597 (3.730)	8.638 (2.293)	-4.834 (2.287)	-4.192 (4.324)	-2.885 (3.044)	1.940 (3.198)
	11-15	17.984 (3.562)	9.775 (2.997)	-14.02 (3.006)	2.293 (4.783)	-4.917 (3.989)	5.983 (4.079)
	16-20	18.518 (4.080)	10.19 (2.919)	-15.122 (3.058)	-0.321 (5.078)	-8.169 (3.893)	4.777 (4.645)
	21-25	24.209 (5.222)	10.618 (4.038)	-17.537 (3.192)	-11.465 (5.397)	-11.799 (4.655)	0.415 (3.619)
	26-30	33.465 (4.610)	6.644 (4.122)	-14.56 (4.052)	-14.769 (5.615)	-16.717 (4.653)	-0.599 (4.470)
	31-35	15.509 (5.147)	5.799 (4.223)	-8.727 (3.712)	-12.977 (5.787)	-0.920 (3.933)	1.998 (4.445)
	36-40	3.804 (11.324)	0.730 (5.869)	-7.300 (8.657)	-2.488 (9.784)	7.507 (7.356)	8.746 (8.360)

Shadow Price Estimates 2015

	Exp	N.R. Analytic	R. Analytic	N.R. Manual	R. Manual	Computer	Comm.
High School	0-5	4.487 (2.297)	-5.948 (2.913)	2.339 (2.154)	-3.247 (2.820)	-0.217 (2.086)	-1.612 (2.314)
	6-10	8.675 (2.396)	-3.993 (2.138)	5.589 (3.038)	0.379 (4.490)	-0.416 (2.095)	4.236 (3.206)
	11-15	4.390 (2.811)	1.184 (4.386)	7.588 (2.611)	-10.355 (4.178)	-4.795 (2.677)	5.092 (2.702)
	16-20	6.759 (2.439)	-0.952 (2.913)	5.237 (2.347)	-5.403 (3.494)	-3.924 (1.738)	6.980 (2.268)
	21-25	4.366 (2.932)	-0.272 (2.950)	7.096 (2.636)	-8.118 (4.215)	0.122 (2.477)	6.320 (2.843)
	26-30	6.182 (3.191)	-4.547 (2.603)	3.567 (2.527)	-1.160 (3.144)	0.193 (2.226)	5.743 (3.497)
	31-35	9.186 (3.106)	-8.450 (4.420)	9.675 (2.946)	-3.828 (3.976)	3.188 (3.110)	4.527 (2.950)
	36-40	7.773 (3.390)	-4.252 (3.117)	4.168 (3.001)	1.635 (3.085)	0.143 (2.473)	6.900 (2.987)
College	0-5	5.944 (4.968)	8.592 (2.882)	11.057 (6.740)	-13.539 (6.929)	1.066 (4.381)	5.910 (4.440)
	6-10	18.246 (4.759)	11.704 (3.533)	4.091 (3.760)	-16.057 (5.112)	-5.726 (4.627)	-7.267 (5.004)
	11-15	27.924 (4.369)	9.906 (4.006)	-15.259 (3.410)	-4.063 (5.269)	-9.820 (4.112)	-3.328 (4.366)
	16-20	13.726 (10.389)	2.115 (4.702)	-15.17 (3.786)	-12.302 (6.609)	0.769 (5.429)	-0.324 (4.396)
	21-25	14.867 (4.814)	5.998 (4.722)	-11.769 (3.410)	-6.934 (5.717)	0.474 (4.287)	8.632 (4.090)
	26-30	0.935 (7.856)	0.932 (5.746)	-17.951 (4.512)	9.894 (6.659)	16.077 (5.396)	11.607 (6.304)
	31-35	7.095 (7.498)	9.534 (4.737)	-14.571 (4.811)	-9.873 (6.611)	8.509 (5.476)	2.281 (5.542)
	36-40	3.819 (10.103)	-3.766 (7.557)	-9.572 (6.295)	2.587 (9.320)	21.658 (6.644)	6.528 (6.296)

APPENDIX C. OAXACA DECOMPOSITION OF WAGES BY TASK

College Graduates, 1980

Overall	High Experience	21.580 (0.264)	Coefficients	Computer	-0.333 (0.543)
	Low Experience	18.088 (0.271)		Communication	1.851 (0.939)
	Difference	3.492 (0.194)		N.R. Manual	-1.717 (0.323)
	Endowments	0.056 (0.064)		R. Manual	2.618 (0.578)
	Coefficients	3.314 (0.180)		N.R. Analytic	2.034 (1.040)
	Interactions	0.121 (0.038)		R. Analytic	-1.138 (0.845)
Endowments	Computer	0.013 (0.010)	Interactions	Computer	0.006 (0.010)
	Communication	0.037 (0.033)		Communication	0.013 (0.013)
	N.R. Manual	0.065 (0.018)		N.R. Manual	0.152 (0.038)
	R. Manual	-0.103 (0.034)		R. Manual	-0.071 (0.028)
	N.R. Analytic	0.0599 (0.047)		N.R. Analytic	0.015 (0.014)
	R. Analytic	-0.015 (0.016)		R. Analytic	0.007 (0.010)
N		11,404			

College Graduates, 1990

Overall	High Experience	24.215 (0.312)	Coefficients	Computer	-1.163 (0.846)
	Low Experience	20.916 (0.443)		Communication	1.355 (0.859)
	Difference	3.299 (0.362)		N.R. Manual	-2.588 (0.429)
	Endowments	-0.032 (0.059)		R. Manual	2.828 (0.756)
	Coefficients	3.225 (0.357)		N.R. Analytic	3.134 (1.155)
	Interactions	0.106 (0.034)		R. Analytic	-0.341 (1.035)
Endowments	Computer	-0.002 (0.005)	Interactions	Computer	0.002 (0.007)
	Communication	0.005 (0.040)		Communication	0.001 (0.007)
	N.R. Manual	-0.002 (0.014)		N.R. Manual	0.117 (0.035)
	R. Manual	-0.050 (0.026)		R. Manual	-0.035 (0.020)
	N.R. Analytic	0.034 (0.034)		N.R. Analytic	0.016 (0.017)
	R. Analytic	-0.019 (0.014)		R. Analytic	0.006 (0.017)
N		12,838			

College Graduates, 2000

Overall	High Experience	30.440 (0.559)	Coefficients	Computer	-1.778 (1.664)
	Low Experience	26.161 (0.650)		Communication	2.693 (1.192)
	Difference	4.279 (0.399)		N.R. Manual	-2.273 (0.612)
	Endowments	-0.195 (0.073)		R. Manual	1.182 (0.816)
	Coefficients	4.453 (0.397)		N.R. Analytic	3.666 (1.796)
	Interactions	0.020 (0.049)		R. Analytic	0.963 (1.139)
Endowments	Computer	0.012 (0.027)	Interactions	Computer	0.048 (0.046)
	Communication	-0.057 (0.045)		Communication	-0.014 (0.013)
	N.R. Manual	0.049 (0.021)		N.R. Manual	0.068 (0.027)
	R. Manual	-0.001 (0.018)		R. Manual	0.000 (0.007)
	N.R. Analytic	-0.232 (0.050)		N.R. Analytic	-0.089 (0.046)
	R. Analytic	0.033 (0.027)		R. Analytic	0.007 (0.010)
N		20,894			

College Graduates, 2010

Overall	High Experience	33.544 (0.544)	Coefficients	Computer	-1.017 (1.963)
	Low Experience	29.071 (0.642)		Communication	2.444 (1.217)
	Difference	4.473 (0.385)		N.R. Manual	-1.918 (0.689)
	Endowments	0.107 (0.097)		R. Manual	-0.297 (1.086)
	Coefficients	4.264 (0.365)		N.R. Analytic	3.431 (2.417)
	Interactions	0.102 (0.044)		R. Analytic	1.621 (1.380)
Endowments	Computer	0.009 (0.014)	Interactions	Computer	0.005 (0.012)
	Communication	0.097 (0.055)		Communication	0.020 (0.015)
	N.R. Manual	0.058 (0.025)		N.R. Manual	0.074 (0.031)
	R. Manual	-0.052 (0.028)		R. Manual	0.004 (0.013)
	N.R. Analytic	0.007 (0.057)		N.R. Analytic	0.002 (0.019)
	R. Analytic	-0.013 (0.029)		R. Analytic	-0.003 (0.008)
N		22,029			

High School Only, 1980

Overall	High Experience	15.363 (0.190)	Coefficients	Computer	-0.074 (0.105)
	Low Experience	14.139 (0.204)		Communication	0.022 (0.212)
	Difference	1.224 (0.095)		N.R. Manual	-1.524 (0.242)
	Endowments	-0.079 (0.029)		R. Manual	-0.136 (0.341)
	Coefficients	1.100 (0.089)		N.R. Analytic	1.644 (0.248)
	Interactions	0.202 (0.023)		R. Analytic	1.168 (0.334)
Endowments	Computer	0.029 (0.009)	Interactions	Computer	-0.007 (0.010)
	Communication	0.357 (0.033)		Communication	0.002 (0.021)
	N.R. Manual	-0.227 (0.023)		N.R. Manual	0.141 (0.025)
	R. Manual	-0.277 (0.025)		R. Manual	0.008 (0.020)
	N.R. Analytic	0.058 (0.011)		N.R. Analytic	0.072 (0.014)
	R. Analytic	-0.020 (0.007)		R. Analytic	-0.014 (0.006)
N		30,857			

High School Only, 1990

Overall	High Experience	16.185 (0.258)	Coefficients	Computer	-0.045 (0.212)
	Low Experience	14.247 (0.262)		Communication	0.201 (0.289)
	Difference	1.938 (0.141)		N.R. Manual	-1.677 (0.371)
	Endowments	-0.007 (0.033)		R. Manual	0.565 (0.522)
	Coefficients	1.859 (0.142)		N.R. Analytic	1.354 (0.352)
	Interactions	0.086 (0.022)		R. Analytic	1.462 (0.401)
Endowments	Computer	0.069 (0.018)	Interactions	Computer	-0.003 (0.015)
	Communication	0.236 (0.038)		Communication	0.012 (0.018)
	N.R. Manual	-0.205 (0.032)		N.R. Manual	0.081 (0.021)
	R. Manual	-0.134 (0.023)		R. Manual	-0.023 (0.022)
	N.R. Analytic	0.040 (0.011)		N.R. Analytic	0.044 (0.014)
	R. Analytic	-0.013 (0.005)		R. Analytic	-0.025 (0.009)
N		26,257			

High School Only, 2000

Overall	High Experience	18.735 (0.244)	Coefficients	Computer	-1.778 (1.664)
	Low Experience	16.622 (0.230)		Communication	0.649 (0.483)
	Difference	2.114 (0.221)		N.R. Manual	-2.077 (0.636)
	Endowments	-0.015 (0.049)		R. Manual	1.011 (1.056)
	Coefficients	2.091 (0.213)		N.R. Analytic	2.450 (0.556)
	Interactions	0.038 (0.027)		R. Analytic	0.743 (0.823)
Endowments	Computer	0.014 (0.009)	Interactions	Computer	-0.012 (0.010)
	Communication	0.085 (0.037)		Communication	0.013 (0.011)
	N.R. Manual	-0.079 (0.030)		N.R. Manual	0.035 (0.017)
	R. Manual	-0.042 (0.019)		R. Manual	-0.019 (0.021)
	N.R. Analytic	0.023 (0.020)		N.R. Analytic	0.026 (0.022)
	R. Analytic	-0.017 (0.011)		R. Analytic	-0.005 (0.006)
N		29,969			

High School Only, 2010

Overall	High Experience	19.144 (0.203)	Coefficients	Computer	-0.366 (0.614)
	Low Experience	17.308 (0.240)		Communication	0.812 (0.503)
	Difference	1.836 (0.195)		N.R. Manual	-1.765 (0.740)
	Endowments	-0.004 (0.053)		R. Manual	0.247 (0.892)
	Coefficients	1.791 (0.185)		N.R. Analytic	1.495 (0.785)
	Interactions	0.049 (0.028)		R. Analytic	1.367 (0.907)
Endowments	Computer	0.008 (0.009)	Interactions	Computer	-0.006 (0.011)
	Communication	0.116 (0.056)		Communication	0.019 (0.015)
	N.R. Manual	-0.107 (0.038)		N.R. Manual	0.030 (0.016)
	R. Manual	-0.042 (0.018)		R. Manual	-0.005 (0.018)
	N.R. Analytic	0.039 (0.021)		N.R. Analytic	0.029 (0.020)
	R. Analytic	-0.019 (0.012)		R. Analytic	-0.017 (0.013)
N		26,220			

APPENDIX D. SMOOTHLY VARYING COEFFICIENTS MODEL ESTIMATES

The following tables report the coefficients and bootstrap standard errors for the smoothly varying coefficients model discussed in chapter 6. The dependent variable is the task shadow price. The left-hand column is the level of labor quantity at which the coefficients are estimated.

SVCM Nonroutine Analytic

Log Labor Quantity	Constant	Trend	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.266	-0.606 (1.237)	0.690 (0.344)	1.364 (1.344)	-0.626 (1.393)	0.437 (1.695)
16.665	-1.329 (0.917)	0.660 (0.263)	1.826 (0.944)	0.302 (1.442)	1.577 (1.144)
17.064	-1.861 (0.655)	0.681 (0.197)	2.187 (0.613)	1.274 (1.393)	2.709 (0.795)
17.463	-2.140 (0.491)	0.742 (0.144)	2.436 (0.403)	2.313 (1.206)	3.684 (0.655)
17.862	-2.133 (0.459)	0.819 (0.117)	2.579 (0.392)	3.349 (0.998)	4.413 (0.655)
18.260	-1.816 (0.506)	0.881 (0.123)	2.600 (0.485)	4.190 (0.908)	4.861 (0.698)
18.659	-1.211 (0.561)	0.909 (0.149)	2.461 (0.567)	4.598 (0.957)	5.070 (0.742)
19.058	-0.412 (0.604)	0.908 (0.187)	2.161 (0.619)	4.446 (1.071)	5.188 (0.789)
19.457	0.452 (0.668)	0.897 (0.235)	1.760 (0.655)	3.799 (1.235)	5.428 (0.909)
19.785	1.287 (0.819)	0.885 (0.301)	1.365 (0.782)	2.867 (1.505)	6.013 (1.239)
R-squared			0.06304		

SVCM Nonroutine Analytic Continued

Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.266	2.069 (1.145)	-0.044 (2.039)	-0.898 (1.372)	4.628 (2.294)
16.665	3.195 (0.967)	1.491 (1.440)	1.051 (1.011)	5.116 (1.525)
17.064	4.090 (0.745)	2.907 (0.981)	2.748 (0.784)	5.695 (1.032)
17.463	4.747 (0.606)	4.172 (0.706)	4.175 (0.706)	6.264 (0.849)
17.862	5.188 (0.647)	5.258 (0.664)	5.388 (0.805)	6.723 (0.864)
18.260	5.447 (0.780)	6.096 (0.772)	6.498 (1.029)	6.955 (0.920)
18.659	5.548 (0.901)	6.583 (0.948)	7.647 (1.341)	6.857 (1.045)
19.058	5.512 (0.984)	6.662 (1.214)	9.018 (1.815)	6.379 (1.323)
19.457	5.375 (1.052)	6.390 (1.623)	10.813 (2.654)	5.518 (1.732)
19.785	5.204 (1.189)	5.955 (2.239)	13.203 (4.066)	4.324 (2.232)

SVCM Nonroutine Analytic

	Interacted with College Degree				
Log Labor Quantity	Trend	0 to 5-Years Experience	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.266	0.816 (1.337)	-3.820 4.908)	-6.335 (4.535)	-1.840 (4.990)	1.678 (5.747)
16.665	0.391 (0.733)	-1.259 (2.749)	-2.238 (2.697)	-0.052 (3.132)	3.323 (3.480)
17.064	0.036 (0.423)	1.000 (1.612)	1.079 (1.754)	1.769 (2.369)	5.142 (2.142)
17.463	-0.207 (0.338)	2.825 (1.253)	3.655 (1.358)	3.725 (2.175)	6.954 (1.542)
17.862	-0.277 (0.336)	4.055 (1.141)	5.482 (1.142)	5.838 (2.037)	8.634 (1.495)
18.260	-0.118 (0.334)	4.528 (1.003)	6.536 (0.974)	7.977 (1.847)	10.111 (1.716)
18.659	0.263 (0.340)	4.210 (0.997)	6.918 (0.993)	9.862 (1.717)	11.472 (2.038)
19.058	0.745 (0.368)	3.397 (1.277)	6.995 (1.229)	11.261 (1.701)	13.068 (2.391)
19.457	1.120 (0.425)	2.718 (1.735)	7.337 (1.509)	12.327 (1.748)	15.343 (2.817)
19.785	1.213 (0.546)	2.847 (2.382)	8.469 (1.808)	13.755 (2.058)	18.460 (3.639)

SVCM Nonroutine Analytic Continued

	Interacted with College Degree			
Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.266	-2.429 (5.181)	0.063 (5.231)	5.725 (11.561)	-32.498 (33.626)
16.665	3.421 (2.981)	0.741 (4.093)	7.869 (8.164)	-14.992 (17.871)
17.064	8.337 (1.973)	3.471 (4.413)	9.178 (5.676)	-2.128 (8.772)
17.463	12.253 (1.592)	7.612 (4.045)	9.998 (3.895)	6.803 (4.465)
17.862	15.158 (1.418)	12.278 (2.888)	10.854 (2.985)	12.569 (3.139)
18.260	17.109 (1.398)	16.503 (1.890)	12.281 (3.102)	15.970 (2.996)
18.659	18.363 (1.747)	19.575 (2.329)	14.617 (3.722)	17.915 (3.247)
19.058	19.479 (2.593)	21.423 (3.239)	17.897 (4.444)	19.512 (4.100)
19.457	21.136 (3.923)	22.673 (3.893)	21.854 (5.257)	21.931 (5.741)
19.785	23.741 (5.867)	24.246 (4.473)	26.051 (6.349)	25.992 (8.055)

SVCM Routine Analytic

Log Labor Quantity	Constant	Trend	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.192	-0.648 (0.817)	-0.218 (0.200)	1.021 (1.275)	-0.112 (1.712)	0.726 (0.864)
16.596	-1.008 (0.417)	-0.234 (0.105)	0.695 (0.653)	-0.759 (0.724)	-0.768 (0.648)
17.000	-1.213 (0.465)	-0.281 (0.101)	0.526 (0.390)	-0.375 (0.553)	-0.987 (0.666)
17.403	-1.133 (0.485)	-0.341 (0.095)	0.400 (0.413)	-0.066 (0.544)	-0.915 (0.556)
17.807	-0.669 (0.331)	-0.411 (0.062)	0.240 (0.384)	-0.383 (0.444)	-0.974 (0.449)
18.211	-0.036 (0.280)	-0.489 (0.055)	0.052 (0.266)	-0.749 (0.361)	-1.011 (0.367)
18.615	0.422 (0.344)	-0.584 (0.076)	0.056 (0.227)	-0.610 (0.330)	-0.754 (0.282)
19.018	0.437 (0.360)	-0.666 (0.086)	0.373 (0.309)	-0.015 (0.294)	-0.156 (0.343)
19.422	0.266 (0.491)	-0.674 (0.097)	0.539 (0.368)	0.484 (0.411)	0.293 (0.520)
19.826	0.161 (0.610)	-0.549 (0.124)	0.418 (0.468)	0.823 (0.839)	0.427 (1.137)
R-squared			0.09398		

SVCM Routine Analytic Continued

Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.192	-1.462 (1.827)	-4.118 (1.294)	-5.032 (0.977)	-2.845 (1.968)
16.596	-1.430 (0.913)	-2.868 (0.777)	-3.974 (0.643)	-2.983 (1.129)
17.000	-1.056 (0.661)	-2.107 (0.603)	-3.187 (0.407)	-2.812 (0.666)
17.403	-0.658 (0.450)	-1.948 (0.475)	-2.503 (0.421)	-2.165 (0.604)
17.807	-0.669 (0.355)	-1.988 (0.456)	-2.197 (0.422)	-1.428 (0.544)
18.211	-0.889 (0.314)	-2.031 (0.425)	-2.137 (0.419)	-1.377 (0.375)
18.615	-0.669 (0.387)	-2.026 (0.421)	-1.716 (0.502)	-1.552 (0.446)
19.018	-0.006 (0.400)	-1.688 (0.467)	-0.891 (0.496)	-1.097 (0.508)
19.422	0.486 (0.379)	-1.293 (0.598)	-0.331 (0.785)	-0.498 (0.539)
19.826	0.822 (1.022)	-0.795 (1.259)	-1.171 (1.367)	-1.011 (1.477)

SVCM Routine Analytic Continued

	Interacted with College Degree				
Log Labor Quantity	Trend	0 to 5-Years Experience	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.192	-0.535 (0.461)	2.952 (2.109)	1.747 (2.431)	-4.115 (2.157)	-2.850 (2.719)
16.596	-0.576 (0.225)	2.620 (0.964)	3.449 (1.270)	-1.724 (1.311)	-1.424 (1.324)
17.000	-0.569 (0.196)	2.939 (0.707)	3.999 (1.149)	0.046 (1.083)	0.244 (1.087)
17.403	-0.463 (0.177)	2.977 (0.785)	3.620 (0.927)	1.053 (0.992)	0.875 (0.922)
17.807	-0.124 (0.183)	2.234 (0.796)	2.313 (0.651)	1.348 (0.987)	0.505 (0.864)
18.211	0.363 (0.144)	1.159 (0.625)	0.909 (0.545)	1.053 (0.854)	0.106 (1.042)
18.615	0.837 (0.195)	0.298 (0.756)	0.171 (0.765)	-0.023 (0.836)	-0.057 (1.306)
19.018	1.025 (0.289)	0.583 (1.353)	0.539 (1.150)	0.089 (0.975)	0.855 (1.797)
19.422	0.801 (0.493)	1.963 (2.711)	2.210 (1.851)	3.294 (1.675)	2.636 (2.479)
19.826	0.368 (0.798)	3.156 (5.057)	5.435 (3.140)	8.491 (2.827)	4.417 (3.391)

SVCM Routine Analytic Continued

	Interacted with College Degree			
Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.192	-4.001 (5.149)	-1.451 (2.952)	1.204 (3.691)	8.917 (2.971)
16.596	-0.890 (2.411)	-0.969 (1.703)	3.291 (2.143)	5.268 (1.632)
17.000	0.221 (1.366)	1.435 (1.301)	3.621 (1.523)	5.015 (1.210)
17.403	1.293 (1.212)	2.826 (1.222)	4.360 (1.079)	4.832 (1.648)
17.807	1.019 (1.017)	2.088 (1.299)	4.415 (1.085)	3.256 (2.366)
18.211	-0.731 (1.061)	0.295 (1.122)	2.403 (1.407)	1.559 (2.028)
18.615	-1.841 (1.369)	-1.145 (0.998)	0.298 (1.949)	0.402 (2.051)
19.018	-1.432 (1.623)	-1.390 (1.307)	1.624 (2.322)	0.856 (2.650)
19.422	-0.063 (1.937)	-0.061 (2.328)	5.118 (5.836)	3.445 (3.939)
19.826	2.576 (3.371)	3.914 (6.841)	7.726 (12.590)	6.747 (5.383)

SVCM Communication

Log Labor Quantity	Constant	Trend	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.290	-1.635 (1.084)	0.690 (0.140)	1.562 (1.650)	0.615 (1.573)	1.420 (1.776)
16.685	-2.548 (0.552)	0.586 (0.107)	1.515 (0.615)	2.065 (0.586)	2.821 (0.699)
17.080	-2.937 (0.500)	0.544 (0.098)	1.609 (0.351)	2.998 (0.520)	3.500 (0.438)
17.475	-2.867 (0.339)	0.570 (0.072)	1.848 (0.397)	2.890 (0.505)	3.593 (0.533)
17.870	-2.571 (0.289)	0.587 (0.069)	2.005 (0.282)	2.498 (0.485)	3.668 (0.514)
18.265	-1.965 (0.464)	0.511 (0.096)	1.862 (0.375)	2.742 (0.570)	3.550 (0.441)
18.660	-1.387 (0.618)	0.369 (0.117)	1.918 (0.391)	3.085 (0.571)	3.720 (0.534)
19.055	-1.257 (0.460)	0.312 (0.095)	2.009 (0.442)	3.241 (0.334)	4.092 (0.431)
19.450	-1.085 (0.248)	0.305 (0.088)	1.751 (0.764)	3.392 (0.975)	4.634 (0.993)
19.845	-0.548 (0.474)	0.255 (0.141)	2.104 (1.620)	3.932 (2.244)	6.181 (2.512)
R-squared			0.20636		

SVCM Communication Continued

Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.290	2.074 (1.905)	1.188 (1.648)	2.721 (1.705)	5.793 (1.776)
16.685	2.936 (0.896)	3.638 (0.910)	4.128 (0.551)	5.459 (0.716)
17.080	3.494 (0.513)	4.763 (0.684)	4.933 (0.599)	5.586 (0.618)
17.475	4.023 (0.382)	4.814 (0.595)	5.295 (0.582)	6.224 (0.743)
17.870	4.264 (0.348)	4.926 (0.508)	5.538 (0.464)	6.715 (0.719)
18.265	4.314 (0.450)	5.129 (0.481)	5.570 (0.582)	6.136 (0.684)
18.660	4.738 (0.528)	5.104 (0.551)	5.989 (0.596)	5.572 (0.737)
19.055	5.092 (0.528)	4.980 (0.525)	7.064 (0.701)	5.171 (0.792)
19.450	5.091 (1.040)	5.206 (1.157)	8.737 (1.358)	4.386 (1.467)
19.845	5.829 (2.511)	6.583 (2.460)	9.173 (1.116)	3.545 (2.236)

SVCM Communication Continued

	Interacted with College Degree				
Log Labor Quantity	Trend	0 to 5-Years Experience	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.290	-0.145 (0.420)	2.074 (1.889)	1.688 (1.981)	1.448 (1.587)	6.443 (3.051)
16.685	0.075 (0.200)	2.410 (0.836)	3.714 (1.076)	2.409 (0.978)	5.328 (1.182)
17.080	0.169 (0.158)	2.413 (0.688)	4.165 (1.024)	3.827 (0.989)	5.337 (0.830)
17.475	0.024 (0.141)	2.587 (0.514)	4.442 (0.795)	4.947 (0.844)	6.460 (0.673)
17.870	-0.122 (0.138)	2.854 (0.508)	4.520 (0.576)	5.963 (0.794)	7.877 (0.645)
18.265	0.082 (0.145)	2.578 (0.639)	3.530 (0.509)	6.263 (0.742)	8.019 (0.860)
18.660	0.469 (0.124)	1.601 (0.586)	2.627 (0.735)	6.103 (0.698)	7.827 (0.978)
19.055	0.649 (0.182)	1.325 (0.513)	3.016 (0.761)	5.758 (0.584)	8.288 (0.975)
19.450	0.536 (0.220)	2.652 (0.912)	4.185 (0.562)	6.474 (0.866)	10.054 (1.510)
19.845	0.412 (0.497)	3.515 (2.702)	5.591 (1.352)	9.303 (1.312)	10.684 (2.276)

SVCM Communication Continued

	Interacted with College Degree			
Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.290	4.112 (2.787)	3.722 (2.269)	v17	7.416 (3.322)
16.685	6.255 (1.124)	4.457 (1.362)	5.191 (2.334)	6.381 (1.623)
17.080	7.801 (0.961)	6.027 (1.183)	6.885 (1.785)	6.015 (1.191)
17.475	8.989 (0.775)	8.599 (0.929)	7.710 (1.772)	7.376 (1.051)
17.870	10.444 (0.757)	10.208 (1.067)	7.941 (0.982)	8.169 (0.868)
18.265	9.684 (0.719)	9.957 (1.240)	9.402 (0.781)	8.136 (1.271)
18.660	8.370 (0.771)	9.350 (0.875)	10.794 (1.090)	7.598 (1.620)
19.055	9.030 (0.892)	9.638 (0.886)	10.953 (0.984)	7.218 (1.287)
19.450	9.969 (1.026)	10.900 (1.100)	10.965 (1.782)	8.572 (1.534)
19.845	10.864 (1.058)	10.990 (2.209)	11.682 (2.250)	10.495 (3.048)

SVCM Computer

Log Labor Quantity	Constant	Trend	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
15.887	-3.924 (1.048)	0.705 (0.252)	4.468 (1.462)	1.487 (1.356)	1.276 (2.015)
16.316	-2.557 (0.771)	0.395 (0.167)	3.300 (0.851)	1.292 (0.897)	1.766 (1.116)
16.746	-1.788 (0.609)	0.223 (0.128)	2.361 (0.602)	1.436 (0.672)	2.430 (0.707)
17.176	-1.132 (0.514)	0.123 (0.099)	1.737 (0.465)	1.518 (0.597)	2.860 (0.585)
17.606	-0.376 (0.496)	0.042 (0.100)	1.493 (0.385)	1.499 (0.558)	2.977 (0.524)
18.036	0.248 (0.579)	-0.035 (0.109)	1.604 (0.400)	1.694 (0.534)	3.015 (0.485)
18.466	0.422 (0.732)	-0.078 (0.110)	1.957 (0.454)	2.100 (0.519)	3.194 (0.519)
18.896	0.380 (0.975)	-0.088 (0.145)	2.196 (0.572)	2.401 (0.639)	3.556 (0.622)
19.325	1.012 (1.555)	-0.172 (0.233)	2.008 (0.809)	2.490 (1.068)	3.861 (1.032)
19.755	2.878 (2.559)	-0.415 (0.391)	1.636 (1.083)	2.572 (1.647)	3.751 (1.950)
R-squared			0.17720		

SVCM Computer Continued

Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
15.887	4.609 (1.870)	3.471 (2.075)	2.968 (1.132)	5.756 (1.393)
16.316	4.278 (1.115)	4.031 (1.263)	4.309 (0.683)	5.485 (0.908)
16.746	4.026 (0.629)	4.495 (0.804)	4.816 (0.524)	5.689 (0.838)
17.176	3.957 (0.481)	4.502 (0.624)	5.021 (0.530)	6.121 (0.842)
17.606	4.002 (0.459)	4.286 (0.597)	5.120 (0.478)	6.363 (0.781)
18.036	4.093 (0.561)	4.108 (0.652)	5.278 (0.447)	6.023 (0.761)
18.466	4.263 (0.714)	3.892 (0.743)	5.761 (0.674)	5.177 (0.895)
18.896	4.456 (0.649)	3.757 (0.798)	6.741 (1.002)	4.192 (1.083)
19.325	4.453 (0.648)	3.949 (0.899)	8.068 (1.286)	3.153 (1.356)
19.755	4.273 (1.151)	4.632 (1.169)	9.313 (1.498)	1.906 (1.872)

SVCM Computer Continued

	Interacted with College Degree				
Log Labor Quantity	Trend	0 to 5-Years Experience	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
15.887	0.199 (0.395)	4.204 (1.564)	3.377 (1.678)	3.847 (2.160)	4.574 (1.809)
16.316	0.036 (0.253)	4.763 (0.926)	4.178 (1.173)	4.178 (1.221)	5.353 (0.938)
16.746	0.020 (0.184)	4.995 (0.667)	4.810 (0.946)	4.752 (0.933)	6.096 (0.801)
17.176	0.069 (0.161)	4.755 (0.617)	4.676 (0.786)	4.872 (0.970)	6.640 (0.780)
17.606	0.145 (0.154)	4.085 (0.600)	3.962 (0.660)	4.728 (0.917)	6.897 (0.723)
18.036	0.288 (0.148)	3.386 (0.689)	3.264 (0.691)	4.818 (0.874)	7.039 (0.871)
18.466	0.478 (0.131)	3.145 (0.696)	3.100 (0.789)	5.282 (0.974)	7.530 (1.012)
18.896	0.578 (0.184)	3.583 (0.826)	3.726 (0.979)	5.877 (1.010)	8.846 (1.234)
19.325	0.518 (0.323)	4.476 (1.763)	4.745 (1.636)	6.644 (1.375)	10.843 (1.944)
19.755	0.458 (0.602)	4.970 (3.803)	5.294 (3.128)	7.693 (2.525)	12.250 (3.542)

SVCM Computer Continued

	Interacted with College Degree			
Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
15.887	3.331 (2.863)	7.699 (3.232)	10.861 (2.921)	8.706 (3.987)
16.316	5.862 (1.768)	8.607 (1.745)	9.832 (1.670)	9.665 (2.169)
16.746	7.085 (1.241)	9.101 (1.148)	9.311 (1.266)	9.664 (1.475)
17.176	7.550 (1.008)	9.860 (1.132)	9.175 (1.102)	9.362 (1.146)
17.606	7.902 (0.833)	10.688 (1.204)	9.536 (0.780)	9.082 (1.002)
18.036	8.053 (0.813)	11.025 (1.210)	10.409 (0.747)	8.880 (1.152)
18.466	8.071 (0.953)	11.136 (1.008)	11.423 (0.961)	9.140 (1.230)
18.896	8.674 (1.382)	11.791 (1.020)	12.523 (1.586)	10.071 (1.441)
19.325	9.956 (2.048)	12.849 (1.664)	13.920 (2.854)	11.506 (1.772)
19.755	10.670 (3.230)	13.241 (2.730)	14.572 (5.537)	13.088 (2.460)

SVCM Nonroutine Manual

Log Labor Quantity	Constant	Trend	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
15.863	0.839 (0.633)	0.009 (0.116)	-0.827 (0.661)	-1.087 (1.167)	-3.744 (1.015)
16.299	0.408 (0.483)	0.016 (0.101)	-0.517 (0.522)	-0.911 (0.838)	-3.315 (0.772)
16.735	0.215 (0.380)	-0.003 (0.086)	-0.340 (0.412)	-0.790 (0.586)	-3.017 (0.591)
17.170	0.218 (0.303)	-0.042 (0.071)	-0.263 (0.324)	-0.729 (0.414)	-2.812 (0.459)
17.606	0.369 (0.240)	-0.096 (0.057)	-0.247 (0.257)	-0.720 (0.322)	-2.664 (0.362)
18.042	0.617 (0.196)	-0.157 (0.049)	-0.253 (0.220)	-0.741 (0.295)	-2.540 (0.296)
18.478	0.914 (0.191)	-0.222 (0.048)	-0.244 (0.219)	-0.757 (0.302)	-2.406 (0.264)
18.914	1.214 (0.236)	-0.286 (0.056)	-0.197 (0.248)	-0.725 (0.323)	-2.229 (0.266)
19.349	1.482 (0.317)	-0.348 (0.073)	-0.105 (0.289)	-0.602 (0.358)	-1.984 (0.298)
19.785	1.701 (0.421)	-0.407 (0.097)	0.019 (0.333)	-0.360 (0.426)	-1.660 (0.364)
R-squared			0.374		

SVCM Nonroutine Manual Continued

Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
15.863	-3.550 (1.084)	-5.850 (0.793)	-6.277 (0.857)	-5.884 (1.073)
16.299	-3.287 (0.794)	-4.973 (0.712)	-6.073 (0.629)	-5.506 (0.840)
16.735	-3.102 (0.575)	-4.331 (0.627)	-5.802 (0.446)	-5.143 (0.647)
17.170	-2.980 (0.421)	-3.891 (0.528)	-5.468 (0.319)	-4.786 (0.499)
17.606	-2.894 (0.319)	-3.614 (0.431)	-5.075 (0.266)	-4.429 (0.397)
18.042	-2.809 (0.257)	-3.459 (0.355)	-4.627 (0.289)	-4.068 (0.351)
18.478	-2.682 (0.227)	-3.387 (0.315)	-4.119 (0.361)	-3.699 (0.395)
18.914	-2.471 (0.232)	-3.357 (0.322)	-3.542 (0.458)	-3.319 (0.539)
19.349	-2.143 (0.274)	-3.330 (0.396)	-2.885 (0.570)	-2.921 (0.749)
19.785	-1.681 (0.358)	-3.275 (0.554)	-2.145 (0.703)	-2.501 (0.986)

SVCM Nonroutine Manual Continued

	Interacted with College Degree				
Log Labor Quantity	Trend	0 to 5-Years Experience	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
15.863	-3.516 (0.360)	2.538 (1.840)	2.798 (2.781)	-9.638 (1.878)	-11.508 (2.105)
16.299	-3.160 (0.258)	2.504 (1.321)	1.582 (1.924)	-8.642 (1.380)	-10.286 (1.476)
16.735	-2.772 (0.192)	2.375 (0.984)	0.343 (1.304)	-7.955 (1.026)	-9.504 (1.059)
17.170	-2.372 (0.154)	2.131 (0.769)	-0.792 (0.889)	-7.512 (0.802)	-9.110 (0.889)
17.606	-1.982 (0.149)	1.774 (0.677)	-1.714 (0.728)	-7.244 (0.720)	-9.049 (0.953)
18.042	-1.617 (0.176)	1.333 (0.765)	-2.341 (0.859)	-7.079 (0.799)	-9.269 (1.171)
18.478	-1.283 (0.223)	0.847 (1.043)	-2.635 (1.150)	-6.945 (1.012)	-9.720 (1.497)
18.914	-0.979 (0.280)	0.351 (1.493)	-2.607 (1.492)	-6.775 (1.329)	-10.347 (1.920)
19.349	-0.694 (0.348)	-0.136 (2.134)	-2.324 (1.853)	-6.519 (1.759)	-11.096 (2.446)
19.785	-0.408 (0.433)	-0.633 (3.024)	-1.899 (2.260)	-6.164 (2.322)	-11.922 (3.082)

SVCM Nonroutine Manual Continued

	Interacted with College Degree			
Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
15.863	-9.132 (2.280)	-7.435 (3.435)	-6.401 (4.352)	-0.980 (4.482)
16.299	-9.069 (1.583)	-7.548 (2.288)	-7.656 (2.965)	-1.315 (3.197)
16.735	-9.414 (1.129)	-8.095 (1.529)	-8.665 (1.966)	-1.890 (2.308)
17.170	-10.059 (0.909)	-9.007 (1.162)	-9.487 (1.510)	-2.700 (1.748)
17.606	-10.912 (0.932)	-10.216 (1.110)	-10.207 (1.575)	-3.735 (1.530)
18.042	-11.906 (1.132)	-11.652 (1.274)	-10.924 (1.839)	-4.959 (1.710)
18.478	-12.998 (1.438)	-13.243 (1.641)	-11.722 (2.114)	-6.310 (2.221)
18.914	-14.172 (1.840)	-14.911 (2.251)	-12.661 (2.390)	-7.708 (2.957)
19.349	-15.420 (2.368)	-16.577 (3.147)	-13.783 (2.692)	-9.062 (3.876)
19.785	-16.738 (3.045)	-18.169 (4.390)	-15.133 (3.015)	-10.293 (4.999)

Log Labor Quantity	Constant	Trend	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.011	1.154 (0.442)	-0.405 (0.115)	-1.004 (0.546)	-2.236 (1.099)	-3.190 (1.275)
16.433	0.797 (0.363)	-0.402 (0.093)	-0.851 (0.449)	-2.002 (0.729)	-3.040 (0.885)
16.856	0.676 (0.345)	-0.428 (0.081)	-0.765 (0.393)	-1.821 (0.516)	-2.916 (0.653)
17.279	0.752 (0.331)	-0.476 (0.069)	-0.743 (0.349)	-1.713 (0.390)	-2.816 (0.511)
17.702	0.967 (0.305)	-0.537 (0.058)	-0.763 (0.310)	-1.676 (0.311)	-2.728 (0.409)
18.125	1.252 (0.282)	-0.604 (0.049)	-0.786 (0.270)	-1.671 (0.268)	-2.624 (0.325)
18.548	1.530 (0.281)	-0.673 (0.049)	-0.770 (0.225)	-1.625 (0.248)	-2.459 (0.258)
18.971	1.734 (0.302)	-0.741 (0.060)	-0.682 (0.189)	-1.460 (0.236)	-2.189 (0.231)
19.394	1.827 (0.337)	-0.806 (0.079)	-0.512 (0.194)	-1.133 (0.248)	-1.802 (0.282)
19.817	1.808 (0.405)	-0.861 (0.100)	-0.272 (0.251)	-0.659 (0.341)	-1.326 (0.428)
R-squared			0.338		

SVCM Routine Manual Continued

Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.011	-3.727 (1.185)	-5.941 (0.876)	-5.643 (0.830)	-4.913 (1.147)
16.433	-3.576 (0.898)	-4.911 (0.811)	-5.362 (0.598)	-4.487 (0.945)
16.856	-3.457 (0.696)	-4.161 (0.726)	-5.045 (0.419)	-4.121 (0.742)
17.279	-3.368 (0.535)	-3.681 (0.623)	-4.678 (0.300)	-3.787 (0.565)
17.702	-3.288 (0.404)	-3.421 (0.527)	-4.251 (0.262)	-3.478 (0.443)
18.125	-3.171 (0.308)	-3.308 (0.441)	-3.753 (0.313)	-3.194 (0.401)
18.548	-2.955 (0.252)	-3.251 (0.360)	-3.164 (0.419)	-2.923 (0.450)
18.971	-2.587 (0.240)	-3.153 (0.328)	-2.466 (0.535)	-2.636 (0.566)
19.394	-2.051 (0.282)	-2.943 (0.428)	-1.675 (0.627)	-2.307 (0.704)
19.817	-1.371 (0.394)	-2.601 (0.643)	-0.851 (0.689)	-1.936 (0.828)

SVCM Routine Manual Continued

	Interacted with College Degree				
Log Labor Quantity	Trend	0 to 5-Years Experience	6 to 10-Years Experience	11 to 15-Years Experience	16 to 20-Years Experience
16.011	0.816 (1.337)	4.404 (2.152)	5.640 (1.752)	-4.368 (2.145)	-5.243 (2.850)
16.433	0.391 (0.733)	3.716 (1.366)	4.170 (1.389)	-3.671 (1.573)	-4.968 (1.774)
16.856	0.036 (0.423)	3.139 (0.871)	2.764 (1.113)	-3.162 (1.205)	-4.948 (1.228)
17.279	-0.207 (0.338)	2.601 (0.642)	1.471 (0.853)	-2.900 (0.949)	-5.183 (1.051)
17.702	-0.277 (0.336)	2.057 (0.645)	0.388 (0.690)	-2.883 (0.750)	-5.605 (1.047)
18.125	-0.118 (0.334)	1.513 (0.785)	-0.358 (0.769)	-3.031 (0.688)	-6.098 (1.145)
18.548	0.263 (0.340)	1.012 (0.999)	-0.656 (1.053)	-3.155 (0.888)	-6.552 (1.373)
18.971	0.745 (0.368)	0.593 (1.290)	-0.455 (1.424)	-2.971 (1.316)	-6.914 (1.744)
19.394	1.120 (0.425)	0.252 (1.730)	0.197 (1.864)	-2.210 (1.932)	-7.201 (2.232)
19.817	1.213 (0.546)	-0.107 (2.495)	1.167 (2.477)	-0.757 (2.704)	-7.496 (2.859)

SVCM Routine Manual Continued

	Interacted with College Degree			
Log Labor Quantity	21 to 25-Years Experience	26 to 30-Years Experience	31 to 35-Years Experience	36 to 40-Years Experience
16.011	-3.913 (2.835)	-1.699 (3.225)	-1.316 (5.181)	5.229 (4.828)
16.433	-4.125 (1.999)	-1.714 (2.382)	-2.048 (3.247)	4.764 (2.983)
16.856	-4.541 (1.479)	-2.383 (1.750)	-3.040 (1.863)	3.900 (1.933)
17.279	-5.195 (1.183)	-3.612 (1.296)	-4.273 (1.360)	2.788 (1.872)
17.702	-6.061 (1.122)	-5.239 (1.127)	-5.756 (1.701)	1.584 (2.216)
18.125	-7.066 (1.269)	-7.047 (1.258)	-7.450 (2.158)	0.484 (2.464)
18.548	-8.123 (1.499)	-8.803 (1.612)	-9.216 (2.479)	-0.285 (2.653)
18.971	-9.143 (1.716)	-10.317 (2.280)	-10.874 (2.652)	-0.511 (3.112)
19.394	-10.019 (1.884)	-11.480 (3.357)	-12.347 (2.710)	-0.036 (4.140)
19.817	-10.643 (2.030)	-12.254 (4.840)	-13.749 (2.799)	1.265 (5.812)