Employer Learning and the "Importance" of Skills

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Abstract: We ask whether the role of employer learning in the wage-setting process depends on skill *type* and skill *importance* to productivity. Combining data from the NLSY79 with O*NET data, we use Armed Services Vocational Aptitude Battery scores to measure seven distinct types of pre-market skills that employers cannot readily observe, and O*NET importance scores to measure the importance of each skill for the worker's three-digit occupation. Before bringing importance measures into the analysis, we find evidence of employer learning for each skill type, for college and high school graduates, and for blue and white collar workers. Moreover, we find that the extent of employer learning—which we demonstrate to be directly identified by magnitudes of parameter estimates after simple manipulation of the data—does not vary significantly across skill type or worker type. When we allow parameters identifying employer learning and screening to vary by skill importance, we identify distinct tradeoffs between learning and screening for some (but not all) skills. Our evidence points to heterogeneity in the degree of employer learning that is masked by disaggregation based on schooling attainment or broad occupational categories.

I. Introduction

The term "employer learning" is typically associated with a class of empirically testable models in which employers learn the productivity of workers over time. In these models, employers are assumed to use schooling attainment and other readily-observed signals to predict productivity and set wages at the start of the career; as workers' careers evolve, true productivity is revealed and the role of schooling in the wage-setting process declines. Building on the work of Spence (1973) and others, Farber and Gibbons (1996) and Altonji and Pierret (2001) were the first to demonstrate that the relationship between a test score and wages is expected to increase with experience in the face of employer learning—where the test score can, in principle, be any measure that is correlated with pre-market productivity but unobserved by employers. Variants of this test have been used by Lange (2007) to assess the speed of employer learning, by Pinkston (2009) and Schönberg (2007) to study asymmetric employer learning, and by Arcidiacono *et al.* (2010), Bauer and Haisken-DeNew (2001) and Mansour (2012) to investigate differences in employer learning across schooling levels, occupational type (blue versus white collar) and initial occupations, respectively.

In the current study, we ask whether the role of employer learning in the wage-setting process depends on the *type* of skill potentially being learned over time as well as the skill's *importance*, by which we mean its occupation-specific contribution to productivity. Basic language skills might be readily signaled to potential employers via the job interview process, while other skill types such as "coding speed" (the ability to find patterns of numbers quickly and accurately) might only be revealed over time in the absence of job applicant testing. Moreover, a skill's importance is likely to affect both *ex ante* screening technologies and the extent to which employers learn about the skill over time. If the ability to solve arithmetic

problems is irrelevant to the work performed by dancers and truck drivers, for example, then the true productivity their employers learn over time should be uncorrelated with a measure of arithmetic skill. Stated differently, the relationship between arithmetic test scores and wages should not increase with experience for dancers and truck drivers. In reverse situations where a particular skill is essential to job performance, it is unclear whether signaling or learning will dominate the wage-setting process. Given that arithmetic skill is critical to accountants' job performance, for example, should we expect arithmetic ability to be a key component of what their employers learn over time? Or do employers customize their screening methods to ensure that the most critical skills are accurately assessed *ex ante*?

To address these questions, we begin by identifying the channels through which skill importance enters a standard model of public employer learning. Using the omitted variable bias strategy of Altonji and Pierret (2001), we demonstrate that by using the portion of a test score (referred to as Z^*) that is orthogonal to schooling and other regressors, the Z^*-experience gradient in a log-wage model is expected to depend solely on the test score's correlation with performance signals that lead to learning, while the coefficient for Z^* is expected to depend both on skill importance and the extent to which the skill is signaled *ex ante*. These derivations motivate our empirical strategy: First, we empirically assess the role of employer learning for alternative skills by inserting skill-specific test scores (Z^*) into a log-wage model and comparing the magnitudes of their estimated experience gradients. Second, we allow coefficients for Z^* and Z^*-experience interactions to depend on skill importance (which we measure directly) to determine whether learning and screening depend on the skill's importance to productivity.

We implement these extensions of the Altonji and Pierret (2001) model using data from the 1979 National Longitudinal Survey of Youth (NLSY79) combined with Occupational

Information Network (O*NET) data. To proxy for pre-market skills that are unobserved by employers, we use test scores for seven components of the Armed Services Vocational Aptitude Battery (ASVAB). The use of narrowly-defined test scores distinguishes our approach from the existing literature, where most analysts rely on scores for the Armed Forces Qualifications Test (AFQT)—a composite score based on four ASVAB components that we use individually.[1] By using skill-specific test scores, we can determine whether employer learning plays a different role for arithmetic ability, reading ability, coding speed, *etc.* We further extend the analysis by using O*NET data to measure the importance of each skill in the three-digit occupation associated with each job. These additional variables enable us to determine whether skill-specific screening and skill-specific employer learning are themselves functions of skill importance.

By allowing employer learning to vary with skill type and skill importance, we contribute to an emerging literature that explores heterogeneity in employer learning along various dimensions: Bauer and Haisken-DeNew (2001) find evidence of employer learning for men in low-wage, blue collar jobs but not for other male workers; Arcidiacono *et al*. (2010) find evidence of employer learning for men with 12 years of schooling but not for men with 16 years of schooling; and Mansour (2012) finds that employer learning differs across workers' initial, two-digit occupations. Our approach incorporates three innovations. First, we allow for a richer form of heterogeneity than is permitted with a high school/college or blue collar/white collar dichotomy and, in fact, we demonstrate that employer learning does *not* differ across categories

[1]We also use AFQT scores in our log-wage models for comparison with existing studies. To our knowledge, no prior study reports estimates based on a cognitive test score other than the AFQT, although Pinkston (2006) notes (p. 279, footnote 23) that he used two ASVAB test scores and obtained results that "resembled" his AFQT-based estimates. As alternatives to test scores, analysts have used parental schooling attainment (Altonji and Pierret 2001; Arcidiacono *et al*. 2010; Pinkston 2006; Bauer and Haisken-DeNew 2001), sibling wages (Altonji and Pierret 2001; Pinkston 2006) or the presence of library cards in the household at age 14 (Farber and Gibbons 1996; Altonji and Pierret 2001).

used by Bauer and Haisken-DeNew (2001) and Arcidiacono *et al*. (2010). Second, while we incorporate Mansour's (2012) notion that detailed measures of "job type" are needed to capture true heterogeneity in employer learning, we define "job type" in terms of the skills needed to perform a specific task rather than occupational categories. We do so in light of existing evidence that jobs are better described in terms of tasks or skills than by orthodox occupational taxonomies (Bacolod and Blum 2010; Gathmann and Schonberg 2010; Lazear 2009; Phelan 2011; Poletaev and Robinson 2008; Yamaguchi 2012). Mansour (2012) allows employer learning to differ with the long-term wage dispersion associated with initial two-digit occupational groups. While this is an innovative measure of job-specific heterogeneity, it raises questions about why employer learning is identical for school teachers and truck drivers (who perform different types of work, but fall into occupational categories with identical wage dispersion) and dramatically different for physicians and medical scientists (whose work is similar, but whose occupational groups are at opposite ends of the wage dispersion distribution). Third, in contrast to the existing literature, we explicitly examine the tradeoff between employer learning and screening, rather than simply infer that any absence of learning must be due to increased screening.

Prior to bringing importance scores into the analysis, we find evidence of employer learning for all seven skill types. We also find that differences across skill types in the degree of learning are uniformly insignificant, as are differences across "worker type" (12 vs. 16 years of schooling, or blue collar vs. white collar) for most skills; in contrast to Arcidiacano *et al*. (2010) and Bauer and Haisken-DeNew (2001), this initial evidence points to little heterogeneity across skills or workers in the role of employer learning. Once we incorporate measures of skill importance, our findings change dramatically. We identify distinct tradeoffs between screening

and employer learning, and we find that the effect of skill importance on screening and learning differs by skill type. For coding speed, mathematical knowledge, mechanical comprehension, screening increases and learning decreases in skill importance; for word knowledge, paragraph comprehension and numerical operations screening is decreasing in skill importance. These patterns suggest that the role of employer screening in wage determination depends intrinsically on the type of skill being assessed and the nature of the job being performed.

II. Model

In this section, we review the test for public (symmetric) employer learning proposed by Altonji and Pierret (2001) (hereafter referred to as AP), and we demonstrate how it can be extended to assess the role of employer learning for a range of skills that differ across jobs in their productivity-enhancing "importance." Our extension of the AP framework enables us to identify both screening and employer learning through sample covariances. As a result, we can assess tradeoffs between learning and screening for "job types" that are richly-defined on the basis of skill type and skill importance. As we discuss, we rely on the AP model rather than Lange's (2007) speed of learning model because the latter cannot distinguish between screening and learning. We also detail how we are able to identify employer learning and screening in the presence of initial occupational sorting as well as subsequent sorting and job mobility.

A. The Altonji and Pierret (AP) Employer Learning Model

A.1. Productivity

Following AP we decompose the true log-productivity of worker i at time t, y_{it}, into its components:

$$y_{it} = rS_i + \alpha_1 q_i + \lambda^z Z_i + N_i + H(X_{it}) \tag{1}$$

where S_i represents time-constant factors such as schooling attainment that are observed at labor market entry by employers and are also observed by the econometrician; q_i represents time-

constant factors such as references that are observed *ex ante* by employers, but are unobserved by the econometrician; Z_i are time-constant factors such as test scores that the econometrician observes but employers do not; N_i are time-constant factors that neither party observes; and $H(X_{it})$ are time-varying factors such as work experience that both parties observe over time. In a departure from AP, we explicitly define λ^z as the *importance* of the uni-dimensional, pre-market skill represented by Z_i—*i.e.*, the weight placed on each test score in determining true log-productivity.[2]

Employers form prior expectations of factors they *cannot* observe (Z_i and N_i) on the basis of factors they *can* observe (S_i and q_i):

$$E(Z_i|S_i, q_i) + \nu_i = \gamma_1 q_i + \gamma_2 S_i + \nu_i$$

$$E(N_i|S_i, q_i) + e_i = \alpha_2 S_i + e_i$$

where, following AP, q_i can be excluded from one expectation. Over time, employers receive new information about productivity—which we refer to as D_{it}—that they use to update their expectations about Z_i and N_i. With this new information in hand, employers' beliefs about productivity at time *t* are:

$$E(y_{it}|S_i, q_i, D_{it}) = (r + \lambda^z\gamma_2 + \alpha_2)S_i + (\lambda^z\gamma_1 + \alpha_1)q_i + H(X_{it}) + E(\lambda^z\nu_i + e_i|D_{it}) \qquad (2)$$

where $\lambda^z\nu_i + e_i$ is the initial error in the employers' assessment of productivity. Following AP, we assume that new information is public and, as a result, learning across firms is symmetric.

A.2. Wages and Omitted Variable Bias

Given AP's assumption (used throughout the employer learning literature) that workers' log-wages equal their expected log-productivity, we obtain the log-wage equation used by employers directly from 2. In a departure from AP's notation, we write the log-wage equation as:

[2]Equation 1 imposes the restriction that λ^z (and all coefficients) is uniform across employers and occupations; we discuss the implications of relaxing this restriction in II.C.

$$w_{it} = \beta_1 S_i + \beta_2 q_i \ + g_{it} + \zeta_{it} \tag{3}$$

where $\beta_1 = r + \lambda^z \gamma_2 + \alpha_2$, $\beta_2 = \lambda^z \gamma_1 + \alpha_1$, $g_{it} = E(\lambda^z v_i + e_i | D_{it})$, ζ_{it} represents factors used by the firm that are outside the model, and $H(X_{it})$ is omitted for simplicity. The econometrician cannot estimate (3) because q_i and g_{it} are unobserved. Instead, we use productivity components for which data *are* available to estimate

$$w_{it} = b_{s1} S_i + b_{z3} Z_i + \epsilon_{it}. \tag{4}$$

AP's test of employer learning is based on an assessment of the expected values of estimators obtained with "misspecified" equation 4. Ignoring work experience and other variables included in the econometrician's log-wage model (which we revisit in II.C), these expected values are:

$$E(\hat{b}_{s1}) = \beta_1 + \beta_2 \delta_{qs} + \theta_t \delta_{vs} = \beta_1 + \beta_2 \frac{S_{zz} S_{sq} - S_{zs} S_{zq}}{S_{ss} S_{zz} - S_{zs}^2} - \frac{S_{zs} S_{zg}}{S_{ss} S_{zz} - S_{zs}^2} \tag{5a}$$

$$E(\hat{b}_{z3}) = \beta_2 \delta_{qz} + \theta_t \delta_{vz} = \beta_2 \frac{S_{ss} S_{zq} - S_{zs} S_{sq}}{S_{ss} S_{zz} - S_{zs}^2} + \frac{S_{ss} S_{zg}}{S_{ss} S_{zz} - S_{zs}^2}. \tag{5b}$$

The δs in 5a-b are defined by auxiliary regressions $q_i = \delta_{qs} S_i + \delta_{qz} Z_i$ and $v_i = \delta_{vs} S_i + \delta_{vz} Z_i$, where v_i is now "shorthand" for initial error $\lambda^z v_i + e_i$, $\theta_t = \frac{S_{zg}}{S_{zv}}$, $S_{zg} = \sum (Z_i - \bar{Z})(g_{it} - \bar{g})$, and $S_{zv} = \sum (Z_i - \bar{Z})(v_i - \bar{v})$; the remaining variance and covariance terms in 5a-b are defined similarly.

The first term in 5a represents the true effect of S_i on log-wages, the second term represents the time-constant component of the omitted variable bias, and the third term (by virtue of its dependence on g_{it}) is the time-varying component of the omitted variable bias. Similarly, in 5b—where there is no true effect because employers do not use Z_i to set wages—the first (second) component of the omitted variable bias is constant (varying) over time.

A.3 AP's Test of Employer Learning

AP's primary test of employer learning amounts to assessing the sign of the time-varying components of the omitted variable biases in 5a-b. Given the relatively innocuous assumptions that $S_{zv} > 0$, $S_{zs} > 0$, and Z_i and S_i are scalars, it is apparent that the time-varying component of 5a is negative and the time-varying component of 5b is positive. Stated differently, the expected value of the estimated S_i coefficient in the econometrician's log-wage model declines over time, while the expected value of the estimated Z_i coefficient increases over time.

AP and subsequent contributors to the literature operationalize this test by modifying specification 4 as follows:

$$w_{it} = b_1 S_i + b_3 Z_i + b_4 S_i \cdot X_{it} + b_5 Z_i \cdot X_{it} + \epsilon_{it}, \tag{6}$$

where S_i is "highest grade completed," Z_i is a test score, X_{it} is a measure of cumulative labor market experience, and ϵ_{it} represents all omitted or mismeasured factors. A positive estimator for b_5 is evidence in support of employer learning; a negative estimator for b_4 is evidence that employers use schooling to statistically discriminate regarding the unobserved skill, Z_i.

B. Assessing Employer Learning for Different Skills and Skill Importance

B.1 Skill Type

Our first goal is to estimate specification 6 with alternative, skill-specific test scores representing Z_i, and use the set of estimators for b_3 and b_5 to compare signaling and employer learning across skills. To do so, we must assess *magnitudes* of the time-varying components of the omitted variable biases in 5a-b. This constitutes a departure from AP, whose objective simply required that they *sign* each time-varying component.

Inspection of 5a-b reveals that the time-varying components (*i.e.,* the right-most terms) depend on S_{zg}, which represents the covariance between the test score used in estimation (Z_i) and the employer's updated information about productivity ($g_{it} = E(\lambda v_i + e_i | D_{it})$), as well as S_{zs}, S_{zz}, and S_{ss}. While S_{zg} is a direct measure of employer learning, two of the remaining three

terms also vary across test scores and, therefore, can confound our ability to interpret $\hat{b}_5$ for each test score as a skill-specific indication of employer learning.

To address this issue, we follow Farber and Gibbons (1996) by constructing skill-specific test scores that are orthogonal to schooling. We define Z_i^* as the residual from a regression of Z_i on S_i and a vector of other characteristics (R_i):[3]

$$Z_i^* = Z_i - E^*(Z_i|S_i, R_i). \tag{7}$$

If we normalize each Z_i^* to have unit-variance ($S_{zz} = 1$) and replace Z_i with this standardized residual in specification 6, then because $S_{zs} = 0$ by construction the time-varying components of the omitted variable biases in 5a-b reduce to:

$$B_{1t} = -\frac{S_{zs}S_{zg}}{S_{ss}S_{zz} - S_{zs}^2} = 0 \text{ and } B_{3t} = \frac{S_{ss}S_{zg}}{S_{ss}S_{zz} - S_{zs}^2} = S_{zg} \tag{8}$$

where B_{1t} and B_{3t} represent the right-most terms in 5a-b ($\theta_t\delta_{vs}$ and $\theta_t\delta_{vz}$). By using standardized, residual test scores, the *Z-X* slope in specification 6 is determined entirely by employer learning.[4] This suggests that if we use a Z_i^* about which the performance history is particularly revealing, then we can expect the coefficient for $Z_i^* \cdot X_{it}$ identified by 6 to be particularly large. To summarize our first extension of AP's test: we use alternative measures of Z_i^* in specification 6 and compare the magnitudes of $\hat{b}_5$ to judge which skills employers learn more about.[5]

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[3]We defer discussion of the "other" characteristics (R_i) to II.C and III.B.

[4]S_{zg} in 8 now refers to the covariance between Z_i^* (not Z_i) and productivity signals. We use Z_i^* (standardized, residual test scores) throughout our empirical analysis, but in the remainder of this section we often leave implicit that Z_i is, in practice, transformed into Z_i^*.

[5]The expression for B_{1t} in 8 indicates that once we replace Z_i with Z_i^*, we should expect $\hat{b}_4$ in specification 6 to be zero because S_i does not serve as a signal for the portion of Z_i that is orthogonal to schooling. This testable hypothesis originates with Farber and Gibbons (1996) who, in contrast to AP, also used Z_i^* rather than Z_i as a regressor.

The time-constant components of the omitted variable biases in 5a-b are also of interest, given that these terms represent the extent to which Z_i is tied to initial wages via signaling. After replacing Z_i by Z_i^* and standardizing, the time-constant components of the omitted variable biases ($\beta_2\delta_{qs}$ and $\beta_2\delta_{qz}$ in 5a-b) are given by:

$$B_{10} = \beta_2 \frac{S_{zz}S_{sq} - S_{zs}S_{zq}}{S_{ss}S_{zz} - S_{zs}^2} = \beta_2 \frac{S_{sq}}{S_{ss}} \text{ and } B_{30} = \beta_2 \frac{S_{ss}S_{zq} - S_{zs}S_{sq}}{S_{ss}S_{zz} - S_{zs}^2} = \beta_2 S_{zq}. \quad (9)$$

The expression for B_{30} reveals that the time-invariant relationship between Z_i^* and log-wages increases in S_{zq}, the covariance between the skill and productivity signals (q) observed *ex ante* by the employer but not the econometrician. All else equal, we expect the estimated coefficient for Z_i^* in specification 6 to be larger for test scores that are relatively easy to assess *ex ante* via their correlation with signals other than S_i; unsurprisingly, the skills measured by such test scores would contribute relatively more to initial wages under these circumstances.

However, we cannot apply this argument to our interpretation of $\hat{b}_3$ because "all else" is not held constant as we substitute alternative test scores into the regression. In particular, B_{30} depends on β_2 which, in turn, depends on structural parameters α_1, γ_1, and λ^z. If $\hat{b}_3$ changes magnitude as we substitute alternative test scores into specification 6, we cannot determine whether the change reflects cross-skill differences in signaling (S_{zq}) or skill importance (λ^z). As explained below, in select circumstances we can make this distinction by using data on skill importance. More generally, we simply view the combined effect of S_{zq} and λ^z (what employers learn via screening combined with how they weight that information) as the screening effect.

B.2 Skill Importance

Building on the preceding discussion, we consider three avenues through which skill importance can affect the wage-generating process and, therefore, the omitted variable biases shown in 8-9.

First, importance affects B_{30} directly through β_2, which is a function of λ^z, so the estimated coefficient for Z_i^* in 6 depends in part on the skill's importance. Second, importance affects B_{30} indirectly if employers' ability to screen for a particular skill is itself a function of importance (*i.e.,* if S_{zq} depends on λ^z). This channel—which is consistent with Riley's (1979) argument that screening is more important in some occupations than in others—might exist because employers screen more intensively (or efficiently) for those pre-market skills that matter the most. For example, dancing skill is critical for a dancer while arithmetic skill is not, so dancers' employers are likely to hold dance auditions (a component of q) prior to hiring but not administer an arithmetic test. Third, importance affects B_{3t} directly because S_{zg} (the covariance between skill and time-varying productivity signals that give rise to learning) depends on skill importance, and not just the skill itself. This latter channel implies that the estimated coefficient for $Z_i^* \cdot X_{it}$ $(\hat{b}_5)$ in specification 6 depends on skill importance.[6]

In light of these arguments, we augment specification 6 to allow b_3 and b_5 to depend on skill importance:

$$w_{it} = a_1 S_i + a_3 Z_i + a_4 S_i \cdot X_{it} + a_5 Z_i \cdot X_{it} + a_6 Z_i \cdot IS_{it}^z + a_7 Z_i \cdot X_{it} \cdot IS_{it}^z + a_8 IS_{it}^z + \epsilon_{it}, \quad (10)$$

where IS_{it}^z is an "importance score" representing the importance of skill Z_i^* for the occupation held by worker *i* at time *t;* we view IS_{it}^z as a direct measure of λ^z. While specification (10) illustrates a parsimonious way to allow the coefficients for Z_i^* and $Z_i^* \cdot X_{it}$ to depend on IS_{it}^z, we also experiment with more flexible specifications that interact IS_{it}^z with additional variables and/or allow IS_{it}^z to have nonlinear effects on the parameters of interest.

Because the time-varying component of the relationship between Z_i^* and w_{it} (per

[6]Altonji (2005) proposes a model in which the rate at which employers learn is related to the overall level of skill importance in an occupation. He does not pursue this extension empirically.

equation 8) is a function only of S_{zg}, we can interpret any IS_{it}^{z}-pattern in the Z_i^*-X_{it} slope as representing the effect of skill importance on employer learning. In contrast, the time-constant component of the relationship between Z_i^* and w_{it} (per equation 9) is a function of both S_{zq} and λ^z, so estimates for a_6 in 10 are potentially more difficult to interpret. Given that λ^z tautologically increases in its empirical analog IS_{it}^{z}, a finding that the estimated Z_i^* coefficient *declines* in skill importance is unequivocal evidence that screening (S_{zq}) declines in skill importance. If the estimated Z_i^* coefficient *increases* in skill importance, we cannot determine whether S_{zq} increases or decreases in importance. We illustrate this ambiguity by considering the case where Z_i^* measures arithmetic ability and "increased importance" corresponds to contrasting a dance company to an accounting firm. The scenario where S_{zq} *increases* in importance corresponds to accounting firms screening for arithmetic skill more effectively than dance companies; in addition, accounting firms necessarily put more weight on arithmetic skill in the initial wage-setting process, so a given amount of arithmetic skill translates into higher initial log-wages for accountants than for dancers because both S_{zq} and λ^z are larger. In the alternative scenario, accounting firms screen *less* effectively than dance companies for arithmetic skill but place a greater weight (λ^z) on whatever arithmetic skill they are able to identify *ex ante*; a given amount of skill continues to translate into a higher initial log-wage for accountants than for dancers because the smaller S_{zq} is offset by a larger λ^z. We cannot distinguish empirically between the two scenarios, but in interpreting our estimates we view the "total" effect of IS_{it}^{z} on the estimated Z_i^* coefficient as the screening effect of interest.

C. Additional Considerations

In this subsection, we consider several factors that potentially affect our ability to relate the magnitude of estimated *Z·X* coefficients in specifications 6 and 10 to the extent of employer

learning associated with skill *Z*. We consider the role of initial occupational sorting, subsequent job mobility, on-the-job training, and the simple fact that we include more regressors in specifications 6 and 10 than were brought to bear in deriving expected parameter values. In addition, we clarify why we assess employer learning by directly identifying S_{zg}, rather than by estimating Lange's (2007) speed of employer learning model.

C.1 Omitted Variable Bias in the Presence of Additional Regressors

Following AP, we derived the omitted variable biases in equations 8-9 for specification 4, which ignores regressors other than S_i and Z_i. When we estimate specifications 6 and 10, however, we control for additional factors, including race and ethnicity, cumulative labor market experience (X_{it}), and skill importance (IS_{it}^z). In order to draw inferences based on the notion that S_{zg} is the *sole* determinant of estimated *Z-X* slopes, we must recognize that variances and covariances involving all remaining regressors not only affect those estimates, but can contribute (along with S_{zg}) to differences *across* test scores. We address this problem by replacing Z_i in each log-wage model with a residual test score (Z_i^*) that is orthogonal by construction to S_i, X_{it}, IS_{it}^z, and every other regressor (race, urban status, occupation, *etc.*).[7]

C.2 Occupational Sorting and Job Mobility

Initial occupational sorting can directly influence employers' inferences about workers' unobserved skills. As noted by Mansour (2012), a finding that occupation A has more (or faster)

[7]In equation 7 we use initial and final values of time-varying regressors, and assume that this effectively reduces their covariances with Z_i to zero. Letting *X* represent a single component of *R* for illustration, once we construct residual test scores in this fashion, $S_{zx} = 0$ and the expected value of b_5 is reduced to a function of S_{zg} and sample moments $S_{z,z*x}$, $S_{s,z*x}$, $S_{z,s*x}$, $S_{z*x,x}$, *etc.*, as well other sample moments that do not involve Z_i (and therefore do not vary across *Z*). We can use the law of iterated expectations to show that each population covariance involving Z_i equals zero or equals a value that does not vary with Z_i; *e.g.*, $S_{s,z*x}$=0 and $S_{z,z*x} = E(X)$. The sample moments will not be exactly zero because we have a finite and unbalanced sample, but we expect them to contribute little to cross-Z variation in estimated slope parameters.

employer learning than occupation B can reflect the fact that workers are more homogenous with respect to skill in A than in B as a result of occupational sorting. More precisely, his argument is that systematic occupational sorting on the basis of skill reduces the variance of employers' initial expectation errors ($\lambda^z v_i + e_i$ in equation 2), thus reducing the amount to be learned. While we agree with Mansour's argument, intensive screening (perhaps for "important" skills) will also reduce the variance of employers' initial expectation errors, and hence the amount left to be learned. In principle, a finding that occupation A has more employer learning than occupation B can reflect occupational sorting *o*r differences across occupations in screening intensity. As noted in II.C.1, we eliminate the role played by initial occupational sorting in our analysis by using residual test scores (Z_i^*) that are orthogonal to initial (and final) occupation-specific importance scores.

Over the course of workers' careers, additional occupational sorting can also influence employer learning. For example, an employer for whom mechanical comprehension is very important might infer that among workers with two years of experience, workers with insufficient mechanical comprehension have migrated to other occupations. We cannot distinguish empirically between learning due to (non-initial) sorting and learning due to performance histories: employer learning in our model reflects S_{zg}, which is the sample analog of $\text{cov}(Z_i, E(\lambda^z v_i + e_i | D_{it}))$, and the information contained in D_{it} includes how long the worker has been in the labor market (*t*) as well as his performance history. We do not model the learning process per se and thus do not distinguish between changes in employers' inferences resulting from awareness of sorting and changes in employers' inferences resulting from observing a worker's performance history. The fact that employers' inferences can change over time due to sorting is not problematic for identifying employer learning; in our application, it simply

suggests another reason to expect employer learning to be related to skill importance insofar as importance might be systematically related to sorting patterns.

Log-productivity equation 1 imbeds AP's assumption that the marginal productivity of a given skill is constant over time and across occupations. As a result, when we derive the expected value of the estimated coefficient for Z_i in specification 4, the only source of time-variation in the omitted variable bias (per equation 8) is S_{zg}, which represents employer learning. In contrast to this simplified assumption, the marginal productivity of a given skill is expected to vary across jobs (Burdett 1978; Jovanovic 1979; Mortensen 1986). Life-cycle job mobility, therefore, introduces an additional source of time variation in the relationship between Z_i and log-wages that cannot be distinguished from employer learning within the AP framework. If workers tend to move to jobs that place more (less) importance on a given skill than did their previous jobs, then we will over-estimate (under-estimate) employer learning. However, if workers change jobs but the relative *importance* of skill does not change over time in the sample, then our estimates are less likely to be affected by mobility.

To assess the potential effect of mobility on our estimates, we re-estimate specification 6 using subsamples of workers who remain in the same occupation or, alternatively, who remain in occupations placing comparable importance on a given skill. In section IV we demonstrate that, in fact, mobility does not substantially influence our estimates.[8]

C.3 On-the-Job Training

In equations 1-2, $H(X_{it})$ represents the fact that wages evolve over time as workers augment their pre-market skill via on-the-job training (Becker 1993; Mincer 1974). The omitted variable

[8]Workers may also move between occupations to learn about their own skills (Antonovics and Golan 2012). Our estimates based on subsample of (occupation or importance level) "stayers" allow us to assess empirically the effect of such experimentation on estimates of employer learning.

biases in equation 8 are derived under the assumption that this additional human capital is orthogonal to S_i and Z_i, which ensures that its effect on log-wages is entirely captured by the experience profile when we estimate specifications 6 and 10. As noted by Farber and Gibbons (1996), if instead complementarities exist between Z_i^* (the component of pre-market skill that is orthogonal to schooling) and the subsequent acquisition of human capital, then we will be unable to separate employer learning from the effects of these complementarities.[9] A likely scenario is that these skill investments are complementary with S_i, which implies that $\mathrm{E}(b_4) > 0$ in specification 6 (and $\mathrm{E}(a_4) > 0$ in 10), in contrast to the prediction (per equation 8) that the *S-X* slope is zero. In section IV, we find evidence for such complementarities.

C.4 Speed of Employer Learning

Lange (2007), Arcidiacono *et al*. (2010) and Mansour (2012) estimate a structural speed of learning parameter (k_1) that depends on two factors: the variance of employers' initial expectation errors (which reflects the "need" for employer learning once initial screening occurs) and the variance of subsequent productivity signals (which reflects the "ability" to learn on the basis of time-varying information).[10] As discussed in II.B, we focus instead on (nonstructural) parameters that are directly related to S_{zq} (which reflects employers' ability to screen for skill *Z*) and S_{zg} (which reflects employer learning); this strategy enables us to examine effects of skill importance on both screening and learning. It stands to reason that skill importance also affects the two variances that determine k_1. If employers are relatively more effective at screening for important skills, then they should have relatively little to learn (*i.e.,* their initial expectation errors should have relatively little variance). Similarly, if productivity signals are relatively

[9]Kahn and Lange (2010) test a model of the evolution of wages nesting both employer learning and human capital models and find support for both.
[10]Structural approaches that consider the role of multidimensional skills in determining earnings include James (2011) and Yamaguchi (2012).

more informative for important skills, then those signals should have relatively low variance. We choose to focus on the sample covariances S_{zq} and S_{zg} rather than on k_1 because estimation of the speed of learning parameter cannot readily distinguish between effects of skill importance on screening versus learning.[11]

III. Data

A. Sample Selection

We estimate the log-wage models described by equations 6 and 10 using data from the 1979 National Longitudinal Survey of Youth (NLSY79). We also use data on workers' attributes and job requirements from the Occupational Information Network (O*NET) to construct occupation-specific importance scores for select skills; background information on O*NET data is provided in appendix A. The NLSY79 began in 1979 with a sample of 12,686 individuals born in 1957-1964. Sample members were interviewed annually from 1979 to 1994 and biennially from 1996 to the present. Data are currently available for 1979 through 2010, but we use data through 2000 only for conformity with prior studies, which demonstrate that employer learning is concentrated in the early-career.[12]

In selecting a sample for our analysis, we adhere as closely as possible to the criteria used by AP to facilitate comparison with their study. We begin by dropping the 6,283 female NLSY79 respondents who make up roughly half the original sample. Among the 6,403 male NLS79 respondents, we drop from our sample 428 men who did not take the 10-component ASVAB test in 1980, given that we rely on these test scores to represent productivity factors that employers learn over time. We then drop 2,075 men whose initial exit from school precedes

[11]In light of our discussion of endogenous mobility in C.2, it is worth noting that if workers move systematically to jobs placing more or less importance on a skill, speed of learning models will conflate these systematic changes in λ^z with employer learning.

[12]Farber and Gibbons (1996), Altonji and Pierret (2001), Lange (2007) and Arcidiacono *et al.* (2010) use data through 1991, 1992, 1998 and 2004, respectively.

January 1978 because Census three-digit occupation codes were not systematically identified for jobs held prior to then, and we require such codes to construct occupation-specific importance scores based on O*NET data. AP apply a similar selection rule for the purpose of constructing an actual experience measure based on weekly employment arrays that exist for January 1978 onward. However, they relax the rule for a subset of respondents for whom weekly information can be "filled in" prior to January 1978. We delete an additional 30 men whose reported "highest grade completed" at the time of initial school exit is less than eight. Another 801 men are deleted from the sample because we lack at least one valid wage (an average hourly wage between $1/hour and $200/hour for which a 1970 Census three-digit occupation code is available) for the current or most recent job at the time of each interview. The relevant observation window for the selection of wages begins at initial school exit and ends at the earliest of three dates: (*i*) subsequent school reenrollment; (*ii*) the respondent's last NLSY79 interview through 2000; or (*iii*) 15 years after initial school exit. Of these 801 deletions, only 51 men report an otherwise-valid wage for which an occupation code is missing; most of the remaining 750 men drop out of the survey relatively soon after school exit. These selection rules leave us with a sample of 22,907 post-school wage observations contributed by 3,069 men.

As discussed in II.C, we estimate select log-wage models using subsamples of non-mobile men to determine whether our estimates are influenced by job mobility. We select observations for a subsample of "occupation stayers" by allowing each man to contribute wage observations as long as his three-digit occupation remains unchanged relative to his initial observation. We select subsamples of "importance score stayers" by retaining each sample member as long as his raw skill-specific importance score does not change by more than 0.05 relative to his initial occupation's score. Each subsample has the same number of men (3,069) as

the full sample. The subsample of "occupation stayers" has 8,778 wage observations; sample sizes for "importance score stayers" are tied to the skill measure being used, but range from 9,471 for mechanical comprehension to 10,273 for coding speed (see table 6B).

We also estimate select specifications for a subsample of men with exactly 12 or 16 years of schooling, and for a subsample of observations corresponding to blue collar or white collar occupations.[13] These subsamples are used for comparison with the findings of Arcidiacono *et al.* (2010) and Bauer and Haisken-DeNew (2001) although, unlike those authors, we use pooled samples (S=12 and S=16; blue collar and white collar) and interactions to allow each parameter to vary by type. We also define each "type" to be time-constant for each respondent, whereas Arcidiacono *et al.* (2010) and Bauer and Haisken-DeNew (2001) allow respondents to appear in both subsamples. Our schooling sample consists of 14,979 observations for 1,677 men with 12 years of schooling and 4,516 observations for 560 men with 16 years of schooling; our occupation sample consists of 17,189 observations for 1,900 men in blue collar occupations and 9,597 observations for 1,264 men in white collar occupations.

B. Variables

Table 1 briefly defines the variables used to estimate our log-wage models and presents summary statistics for samples described in the preceding subsection. Our dependent variable is the natural logarithm of the CPI-deflated average hourly wage, which we construct from the NLSY79 "rate of pay" variables combined with data on annual weeks worked and usual weekly hours.

[13]Following U.S. Census Bureau definitions, we define a wage observation as white collar if the worker's *initial* occupation corresponds to professional, technical and kindred workers; managers and administrators, except farm; sales workers; or clerical and kindred workers. A wage observation is classified as blue collar if the *initial* occupation corresponds to craftsmen and kindred workers; operatives, except transport; transport equipment operatives; or laborers, except farm.

For comparability across specifications, we always use a uniform set of baseline covariates. We follow convention in using highest grade completed (S) as a measure of productivity that employers observe *ex ante*.[14] Our schooling measure is based on "created" NLSY79 variables identifying the highest grade completed in May of each calendar year, and identifies the schooling level that prevails at each respondent's date of school exit. Because we truncate the observation period at the date of school reentry for respondents seen returning to school, our schooling measure is fixed at its pre-market level for all respondents, as required by the model; discontinuous schooling is a relatively common phenomenon among NLSY79 respondents (Light 1998, 2001). We also control for potential experience (X)—defined as the number of months since school exit, divided by 12—X^2, X^3, two dummy variables indicating whether the individual is black or Hispanic (with nonblack, non-Hispanics serving as the omitted group), interactions between S and these race/ethnicity dummies, the interaction between S and X, a dummy variable indicating whether the individual resides in an urban area, and individual calendar year dummies. This baseline specification mimics the one used by AP.

In a departure from prior research on employer learning, we control for productivity correlates that employers potentially learn over time (Z) with eight alternative measures of cognitive skills. Our first measure is the one relied on throughout the existing literature: an approximate Armed Forces Qualifications Test (AFQT) score constructed from scores on four of

[14]Highest grade completed is used to represent S throughout the employer learning literature, but we suspect this measure is *not* directly observed by employers: resumes, job applications, and school transcripts typically report degree attainment, credit completion, and enrollment dates, but not highest grade completed. See Flores-Lagunes and Light (2010), Frazis *et al.* (1995) and Kane *et al*. (1999) for discussions of why highest grade completed and highest degree might capture distinct information.

the 10 tests that make up the Armed Services Vocational Aptitude Battery (ASVAB).[15] Our remaining measures are scores from seven individual components of the ASVAB: arithmetic reasoning, word knowledge, paragraph comprehension, numerical operations, coding speed, mathematical knowledge, and mechanical comprehension. We use the first four ASVAB scores because they are used to compute the AFQT score; we include the remaining scores because, along with the first four, they can be mapped with minimal ambiguity to O*NET importance scores.[16] We provide the formula for computing AFQT scores and a mapping between ASVAB skills and O*NET measures in appendix table A1.

As detailed in section II, our use of alternative test scores presents us with a challenge not faced by analysts who rely exclusively on AFQT scores as a proxy for Z: in order to compare estimated coefficients for $Z{\cdot}X$ and Z across test scores and attribute those differences to skill-specific employer learning and screening, we have to contend with the fact that each Z is correlated with S, X, and other regressors, and that these correlations differ across test scores. Table 2 shows correlations between each (raw) test score and S, black, Hispanic, X and IS; because X and IS are time-varying, we use each worker's initial and final values. Unsurprisingly, each test score is highly correlated with S. These correlations range from a high of 0.645 for mathematics knowledge to a low of 0.426 for mechanical comprehension, which is arguably the most vocationally-oriented of our skill measures. Each test score is negatively correlated with black and Hispanic, and with both initial and final values of X—and for each variable, the degree

[15]NLSY79 respondents were administered the ASVAB in 1980. All respondents were targeted for this testing—which was conducted outside the usual in-person interviews—and 94% completed the test.

[16]Our seven skill measures are the only ones satisfying two requirements: (a) they must be measured for NLSY79 respondents prior to the start of the career, and (b) an O*NET measure of skill importance must be available for the skill. Other pre-career NLSY79 skill measures (*e.g.,* the three remaining ASVAB components and noncognitive skill measures such as locus of control and self-esteem) do not map cleanly into O*NET importance scores. Other O*NET importance scores (*e.g.,* physical stamina) are not assessed by the NLSY79.

of correlation again varies considerably across test score.[17] Scores for the more academic tests (mathematics knowledge, arithmetic reasoning, word knowledge, *etc.*) tend to be highly correlated with skill importance, while scores for vocationally-oriented tests (coding speed, mechanical comprehension) are much less—and even negatively—correlated with importance.

To net out these correlations we regress each raw test score, using one observation per person, on each time-invariant regressor (S and the two race/ethnicity dummies) as well as initial and final values for urban status, X, $S{\cdot}X$, black$\cdot X$, Hispanic$\cdot X$, and the importance score corresponding to the particular test. Because NLSY79 respondents ranged in age from 16 to 23 when the ASVAB was administered, we also include birth year dummies in these regressions. We then standardize score-specific residuals to have a zero mean and standard deviation equal to one for the "one observation per person" sample of 3,069 men; the standard deviations continue to be very close to one in the regression sample consisting of 22,907 person-year observations.

Our use of alternative test scores also compels us to consider whether the seven ASVAB components measure distinct skills, or whether they simply provide alternative measures of a single, general skill. In the top panel of table 3 we demonstrate that correlation coefficients among raw scores for the seven tests range from 0.54 to 0.84, with the largest correlations belonging to two pairs: word knowledge and paragraph comprehension, and arithmetic reasoning and mathematics knowledge. The bottom panel of table 3 shows that most of these correlations fall to 0.30-0.50 when we use residual scores, although they remain at about 0.7 for the two pairs just mentioned. Clearly, much of the correlation in the raw scores reflects the fact that sample members who are older and/or more highly-schooled tend to perform better on all tests. Once

[17]The pronounced, negative correlation between skill and X_f (final potential experience) reflects the fact that less skilled individuals leave school earlier, and are therefore more likely than their more skilled counterparts to contribute an observation at (or near) the maximum experience level of 15 years.

those factors are netted out, the dramatically lower correlation coefficients in the bottom panel suggest that we are not simply measuring "general skill" with seven different tests—although the skills measured by word knowledge and paragraph comprehension are undeniably similar, as are those measured by arithmetic reasoning and mathematics knowledge. The evidence in table 3 suggesting that we are, in fact, measuring five distinct skills ("verbal," "math," numerical operations, coding speed and mechanical comprehension) is corroborated by several studies that use factor analysis or item response theory to analyze ASVAB scores (*e.g.,* Ing and Olsen 2012; Stoloff 1983; Welsh *et al.* 1990).

As a result of correlations among our residual test scores, estimated effects of screening and employer learning for a given skill will potentially reflect screening and learning with respect to any other skill with a correlated test score. Suppose, for example, that employers learn about numerical operations over time, but not about paragraph comprehension. Given the modest correlation between the test scores for these skills seen in table 3, our estimates would identify employer learning with respect to paragraph comprehension—but would correctly identify *more* employer learning for numerical operations than for paragraph comprehension. In general, our estimates will correctly "rank" the degree of learning (and screening) across distinct skills, even if the test score correlations shown in table 3 cause some estimates to be overstated.[18]

In another departure from the existing literature, our covariates include occupation-specific importance scores (IS^Z) for each skill measure except AFQT scores. These scores, which we construct from O*NET data, represent the importance of each skill (or type of knowledge) measured by the given ASVAB component in the three-digit occupation associated

[18]In principle, we can eliminate correlations among test scores by including all "other" scores in the regressions used to compute skill-specific residual scores. We do not use this strategy due to concerns that "noise" dominates the remaining variation in each residual score. We do not include multiple test scores in a single regression because AP's test for EL requires scalar measures of Z.

with the current job (employer spell); we use the first-coded occupation for each job, so IS^Z is time-invariant within job. For example, the score for arithmetic reasoning reflects the importance in one's occupation of being able to choose the right mathematical method to solve a problem, while the score for mathematics knowledge measures the importance of knowing arithmetic, algebra, geometry, *etc*. The importance scores range from one for "not important" to five for "extremely important" and reflect the average responses of workers surveyed in an occupation.[19] Some skills are important in most jobs while others are important in only a few jobs, as the distributions in table 4 indicate. For instance, paragraph comprehension and word knowledge are "important," "very important," or "extremely important" (scores 3-5) in more than half of all observations in our sample, while arithmetic reasoning and numerical operations range from "important" to "extremely important" in only about 10% of the observations in our sample.

Given that importance scores (IS^Z) play such a critical role in our analysis, we conclude this section by addressing two questions: Do the scores appear to make sense? What do these scores measure that might be missed by conventional occupation categories? To address these questions, in table 5 we present importance scores for several three-digit occupations, along with the mean growth in residual wage variance (*GRV*) reported in Mansour (2012) for the aggregate occupation group to which the three-digit occupation corresponds. Unsurprisingly, the importance scores for "word knowledge" and "paragraph comprehension" are highest for lawyers and lowest for dancers, truck drivers, and auto mechanics. Similarly, importance scores for "arithmetic reasoning" and "numerical operations" are highest for mathematicians and lowest for dancers. Coding speed, which is the ability to find patterns quickly and accurately, is more important for key punch operators than for other occupations in our selected group, while

[19]See appendix A for additional details on how O*NET creates "importance" measures and how we construct our *IS* variables.

mechanical knowledge is most important for auto mechanics. If any surprise is revealed by table 5, it is that basic reading, language, and mathematical skills are deemed to be fairly important in each of these disparate occupations.

Table 5 also illustrates the type of heterogeneity that is captured by IS^Z but potentially missed by Mansour's (2012) occupation-based classification (*GRV*). A comparison of secondary school teachers and truck drivers reveals that, unsurprisingly, importance scores for most skills differ dramatically across these occupations. However, these highly dissimilar occupations fall into aggregate occupational groups with *identical* mean *GRV*. At the other extreme, physicians and biological scientists fall into aggregate occupations (health diagnosing and natural scientists) with *GRV* values at opposite ends of the distribution (0.291 versus -0.064), despite the fact that importance scores for these occupations tend to be similar. Auto mechanics and truck drivers make another interesting comparison: importance scores for most skills are virtually identical for these two occupations and, in this case, they have fairly similar values for *GRV*. However, mechanical comprehension is extremely important for mechanics and much less so for truck drivers. We use these examples to suggest that a measure of "job type" based strictly on occupation codes (as proxied by *GRV*) lacks the substantive content embodied in a task-based or skill-based measure (IS^Z). Importance scores suggest that employer learning with respect to word knowledge might be more pronounced for teachers but not for truck drivers *because* this particular skill is important for teaching. The use of *GRV* not only predicts identical employer learning for teachers and truck drivers, but lacks the "content" to justify why this (or any) similarity might exist.

IV. Findings

Table 6A reports estimates for eight versions of specification 6, which is the standard log-wage

model used by AP and others to test for employer learning. The first column of estimates uses AFQT scores to represent Z, the skill component that is unobserved by employers. The next seven columns replace AFQT scores with scores for individual components of the ASVAB. In the top panel, we transform each raw test score by regressing it on birth year dummies to account for age differences when the tests were taken, and then standardize the residual scores to have unit variance. In the bottom panel—as well as in *all* subsequent tables in this section—we switch to the construction method described in II.B.1 and III.B in which residuals are obtained from regressions that also include S, X, IS^Z, and other covariates.

We begin by comparing our AFQT-based estimates reported in the top panel of table 6A to those obtained by AP using a similar specification, but data through 1992 only.[20] Our estimated coefficient for $Z{\cdot}X/10$ (0.067) is larger than the estimate reported by AP (0.052), while our estimated coefficient for $S{\cdot}X/10$ (0.015) is precisely estimated and of the opposite sign compared to AP's (imprecise) estimate of -0.019. Because our AFQT-based estimated coefficients for $Z{\cdot}X$ are positive, we join AP in finding support for employer learning—but not in finding support for statistical discrimination.

When we replace AFQT scores with individual ASVAB scores in the top panel of table 6A, the estimated coefficients for $Z{\cdot}X$ range from 0.046 for coding speed to 0.069 for word knowledge. It is difficult to interpret these differences because, as discussed in II.B, each estimated coefficient reflects covariances between Z and other regressors, including S; as indicated by table 2, these covariances differ substantially across test scores. If we were to ignore these confounding covariances we would conclude that employer learning is most pronounced for word knowledge and least pronounced for coding speed and mechanical

[20]As reported in their table I, column 4, AP's estimates (robust standard errors) for Z, $Z{\cdot}X/10$, S and $S{\cdot}X/10$ are 0.022 (.042), 0.052 (.034), 0.079 (.015) and -0.019 (.012), respectively.

comprehension, which are the only two tests under consideration that measure vocational skill rather than general verbal and quantitative skills.[21] This "straw man" result is surprising insofar as we might expect word knowledge to be a skill that workers can accurately signal to employers *ex ante*, while vocational skills would be among the skills employers learn over time by observing performance.

However, such judgments should be based on the bottom panel of table 6A, where we use the portion of Z that is orthogonal to S, X, IS^Z, and other regressors. We can now apply the expression for B_{3t} in equation 8, which tells us that a positive estimated coefficient for $Z{\cdot}X$ is consistent with employer learning *and* that the magnitude of each estimate is a direct measure of employer learning. While each estimated $Z{\cdot}X$ coefficient continues to be positive in the bottom panel, we cannot reject the null hypothesis that all eight estimates are identical (nor can we reject the null hypothesis that any pairwise difference is zero). Stated differently, we find evidence of employer learning for all eight skill measures, but no evidence that the degree of employer learning is skill-specific. The (statistically significant) difference in the top panel between the smallest estimated $Z{\cdot}X$ coefficient and the largest is entirely attributable to the fact that coding speed has a relatively small correlation with S while word knowledge has a large correlation with S (table 2).[22]

The estimates in the bottom panel of table 6A are noteworthy for two additional reasons. First, the estimated coefficients for Z range from a statistically insignificant 0.007-0.012 for paragraph comprehension and word knowledge to a precisely estimated 0.035 for numerical

[21]While we fail to reject the joint hypothesis of equality of the estimated $Z{\cdot}X$ coefficients in the top panel of table 6A, the pairwise difference between the estimated word knowledge and coding speed $Z{\cdot}X$ coefficients is statistically significant.

[22]When we construct standardized, residual test scores from regressions of Z on birth year dummies and S (but no additional regressors), we obtain estimates (not reported) that are virtually identical to those in the bottom panel of table 6A. Thus, we conclude that differences between the top- and bottom-panel estimates in table 6A are due to S_{zs}.

operations. As shown by expression B_{30} in equation 9, these estimates reflect the extent to which pre-market information other than schooling is correlated with Z (S_{zq}) and the importance of Z in determining productivity (λ^z). We can conclude, therefore, that word knowledge and paragraph comprehension are either less-screenable or less important than other skills. Second, the estimated coefficients for $S{\cdot}X$ are small in magnitude, but uniformly positive and statistically significant. This again contradicts the model's prediction (per the expression for B_{1t} in equation 8) that the relationship between S and log-wages should not change with experience. As noted in II.C. (following Farber and Gibbons, 1996), a positive S-X slope is consistent with a feature of wage determination abstracted from in the model—*viz.,* that highly-schooled workers invest more intensively than their less schooled counterparts in on-the-job training and/or receive a higher return to these skill investments.[23]

Before proceeding to a discussion of how skill importance affects our inferences, we discuss the robustness of the estimates reported in the bottom panel of table 6A. In results available from the authors, we experimented with nonlinear skill-experience gradients ($Z{\cdot}X^2$) for the model specifications shown in table 6A given that Lange (2007) and Arcidiacono *et al*. (2010) find that most employer learning occurs in the first few years of the labor market career. In no instance did this added flexibility affect our inferences. Table 6B shows estimates for specification 6 based on subsamples of "occupation stayers" and "importance score stayers" described in III.A. Given that job mobility—especially toward jobs that place greater importance on the skill measured by test score Z—can produce a positive estimated Z-X slope in

[23]Farber and Gibbons (1996) include interactions between S and year dummies in their wage model to net out secular increases in the price of skill. When we add similar interactions terms, our estimated $S{\cdot}X$ coefficients fall to zero or, in some cases, become negative. Because we use a narrow birth cohort and measure experience as elapsed time since school exit, we believe that skill-price effects cannot be distinguished from the effects of post-school skill acquisition.

the absence of employer learning, our goal is to assess the potential influence of mobility on our "full sample" estimates (table 6A) by comparing them to estimates based on subsamples of workers who do not change occupations, or who do not change occupations "enough" for the *Z*-specific importance score to change. Both sets of estimates in table 6B reveal that job mobility has little effect on the full sample estimates. In particular, we fail to reject the null hypothesis that all seven estimated $Z{\cdot}X$ coefficients are equal in both the "occupation stayer" and "importance score stayer" samples. In a few instances the estimated coefficient for $Z{\cdot}X$ changes noticeably relative to the table 6A estimates. For example, in the "importance score stayer" sample the estimated arithmetic reasoning parameter increases from 0.048 (table 6A) to 0.065 (table 6B), while the estimated math knowledge parameter falls from 0.047 to 0.030; only for these two skills do we fail to reject the pairwise equality of the estimated coefficients for $Z{\cdot}X$. Overall, the finding that employer learning does not differ significantly across test scores continues to hold in samples of non-mobile workers, indicating that job mobility does not significantly affect our findings.

In table 6C, we present estimates for specification 6 based on a subsample of men with S=12 or S=16, and a subsample of observations associated with blue collar or white collar occupations; for each subsample, we allow *every* parameter in the model to differ by "type." These estimates permit comparison with the findings of Arcidiacono *et al*. (2010), who identify positive $Z{\cdot}X$ coefficients for men with S=12 but not S=16 using the NLSY79, and Bauer and Haisken-DeNew (2001), who identify positive $Z{\cdot}X$ coefficients for (low-wage) blue collar but not white collar workers using German Socioeconomic Panel Study data.

The top panel of table 6C reveals that the estimated $Z{\cdot}X$ coefficients are statistically indistinguishable for men with 12 years of schooling and men with 16 years of schooling for

each test score. This finding contrasts starkly to evidence in Arcidiacono *et al*. (2010), who report an imprecisely estimated *AFQT·X* coefficient equal to 0.01-0.02 for the *S*=16 sample, and a precisely estimated coefficient that is ten times larger for the *S*=12 sample. While they conclude that employer learning occurs only for less-schooled men, we find no evidence that employer learning differs across the two schooling groups for any skill.[24]

When we compare the estimated *Z·X* coefficients for blue collar and white collar workers in the bottom panel of table 6C, we find that the point estimates are larger for white collar workers than for blue collar workers for all but word knowledge and mathematical knowledge, although only for numerical operations is the pairwise difference statistically significant. This contradicts the conclusions of Bauer and Haisken-DeNew (2001), who find evidence of employer learning for low-wage, blue collar workers only. The two studies, however, are not strictly comparable given that Bauer and Haisken-DeNew use German data and a measure of parental schooling in lieu of test scores to represent *Z*.

As a group, our estimates for specification 6 reveal that employer learning exists for each skill type, for both S=12 and S=16 workers, and for both blue collar and e collar workers, but that the degree of learning *does not vary* across skills or worker types. The purpose of comparing worker types (*S*=12 versus *S*=16 and blue collar versus white collar) is to demonstrate that evidence of heterogeneous employer learning reported by Bauer and Haisken-DeNew (2001) and Arcidiacono *et al*. (2010) are not replicated in our data.

In table 7 we report estimates for specification 10 (in which coefficients for *Z* and *Z·X* are allowed to vary linearly with skill importance (IS^Z)) to determine whether skill-specific employer

[24]Using data and programs provided by the authors (available at http://www.aeaweb.org), we determined that the findings reported by Arcidiacono *et al*. (2010) are driven by college-educated men whose potential experience is significantly overstated. See Light and McGee (2013) for details.

learning and screening are themselves functions of the skill's importance to productivity. Turning first to the right-most column in table 7, we observe that for mechanical comprehension the estimated coefficient for *Z increases* from -0.013 on jobs for which mechanical comprehension is not important (IS^Z =1) to 0.063 on jobs for which it is extremely important (IS^Z =5), while the estimated coefficient for *Z·X/10 decreases* from 0.125 to -0.055 over the same range of IS^Z scores. As discussed in II.B.2, the positive effect of skill importance on the estimated *Z* coefficient represents an increase in S_{zq} multiplied by skill importance; we interpret this total effect as increased screening. For mechanical comprehension, therefore, we find a distinct tradeoff between screening and learning: when this skill is unimportant, employers do not screen and instead rely on performance histories to reveal productivity over time, but when mechanical comprehension is important employers rely more on screening and less on learning. This suggests that signals of mechanical ability are available at the outset of the career, but that these signals are impractical to obtain when employers are relatively unconcerned about the skill.[25] We find qualitatively similar patterns for coding speed and mathematical knowledge, although for the former skill the $Z{\cdot}IS^Z$ and $Z{\cdot}X{\cdot}IS^Z$ coefficients are estimated very imprecisely.

Table 7 reveals the opposite pattern for word knowledge, paragraph comprehension and numerical operations—*i.e.,* the estimated *Z* coefficients *decrease* in IS^Z (which, as discussed in II.B.2, necessarily means S_{zq} decreases in importance) while the estimated *Z·X/10* coefficients *increase*, although most interaction terms are imprecisely estimated. This pattern suggests that signals (elements of *q*) available to employers at the outset of the labor market career are insufficiently informative for jobs for which the skill is highly important. For such jobs, employers rely on performance to learn about the worker's skill over time in the absence of a

[25]When hiring automobile mechanics, for example, employers care about mechanical comprehension and are likely to obtain signals provided by certification programs.

more informative and easily obtained pre-market signal.

The Z and $Z{\cdot}X/10$ parameters will not vary linearly in skill importance if, for example, employers only engage in intensive screening when a particular skill is highly important to the worker's job. To explore these nonlinearities, in table 8 we report estimates from a specification in which the key parameters are allowed to differ when skill importance is "high." As shown in table 4, some skills are important (IS^Z =3-5) in relatively few occupations while others are important in most occupations. Given the inherent difficult of defining "high" importance uniformly for all seven skills, we use two alternative definitions. In the top panel of table 8 we define "high" as any importance score in the top quartile of the Z-specific distribution; this requires the raw importance score to exceed 2.56 to 3.48, depending on the skill (table 4). In the bottom panel of table 8 we define "high" uniformly across skills as any importance score equal to 3.25 or higher; the percentage of observations meeting this absolute cutoff ranges from 3% for numerical operations to 40% for work knowledge and paragraph comprehension.

With few exceptions, the estimates in table 8 reveal the same patterns seen in table 7. Using quartile-based definitions of "high" skill importance, estimated $Z{\cdot}$high IS^z coefficients in table 8 have the same sign as the corresponding $Z{\cdot}IS^z$ estimates in table 7 for all skills. Estimated coefficients for $Z{\cdot}X/10{\cdot}$high IS^z in table 8 and $Z{\cdot}X{\cdot}IS^z$ in table 7 have the same sign for all skills except work knowledge, where the estimates are effectively zero in both tables. Estimates in the bottom panel of table 8 are qualitatively similar to those in table 7 for all skills except arithmetic reasoning and numerical operations, where "high" importance is defined for a *very* small number of observations. For the most part, the estimates in table 8 suggest that the relationships between skill importance and screening and learning are adequately captured by the linear specification in table 7, which has the advantage of using all of the variation in IS to identify the

relationships between skill importance and screening and learning.[26]

V. Conclusions

In light of the potential centrality of employer learning to economists' understanding of life-cycle wage paths, numerous analysts have looked for evidence of employer learning in broad samples of workers (Altonji and Pierret 2001; Farber and Gibbons 1996; Lange 2007; Pinkston 2009; Schönberg 2007) and some have explored heterogeneity in employer learning with respect to worker type (Arcidiacono *et al*. 2010; Bauer and Haisken-DeNew 2001) or job type (Mansour 2012). However, existing studies have relied exclusively on a single cognitive test score (AFQT scores), which means they have identified employer learning with respect to the basic language and quantitative skills measured by this test. Moreover, existing studies of heterogeneous employer learning have relied on broad definitions of worker or job type that do not capture the skill needs associated with each narrowly-defined job.

In the current study, we use seven cognitive test scores—each measuring a well-defined skill such as mathematical knowledge or coding speed—to determine whether employers learn more about some skills than others. We also use direct measures of each skill's occupation-specific importance to productivity to learn whether employer learning is more or less pronounced when a given skill is relatively important for the work being performed, and to assess tradeoffs between employer learning and *ex ante* screening. We are able to accomplish these objectives by combining test score data from the NLSY79 with O*NET data on each skill's importance on each three-digit occupation, and by deriving conditions under which the

[26]Additional experiments (not tabulated) reveal that the patterns seen in table 7 are robust to allowing the effects of X (and higher-order terms in X), S, and $S{\cdot}X$ to vary with IS, adding $Z{\cdot}X^2$ effects that are allowed to vary with IS and allowing the screening and learning coefficients to vary with IS in nonlinear specifications other than those in table 8. We also estimate specification 10 for pooled S=12/S=16 samples and pooled blue collar/white collar samples and continue to find that parameters do not vary across worker type; for each skill the p-value for the null hypothesis that parameters are equal across workers type is 0.40 or larger.

magnitudes (and not simply the signs) of parameter estimates are directly tied to the extent of screening and employer learning.

We identify four key results. First, employer learning exists for each skill type and, within each skill type, for high school graduates, college graduates, blue collar workers, and white collar workers. Second, before the role of skill importance is brought to bear, we find little evidence that the degree of employer learning differs across skill types or worker types. Third, upon incorporating information on skill importance, we find distinct tradeoffs between employer learning and screening for several skills. Fourth, we find that the effect of skill importance on employer learning and screening differs across skills. When mathematics knowledge and mechanical comprehension are relatively important for a given occupation, employers screen for these skills rather than learn about them over time. In contrast, when word knowledge or paragraph comprehension is important to occupational productivity, employer learning occurs over time but screening is nonexistent. These findings suggest that the manner in which worker ability is revealed to their employers depends intrinsically on the interplay between skill type and skill importance. Studies that focus on a single, general skill and/or explore heterogeneity in employer learning across broad types of workers have masked much of this variation.

Having developed an approach (building on Farber and Gibbons (1996)) that facilitates a comparison of how employer learning differs across skills, we conclude by suggesting two dimensions in which our analysis can be extended. First, an examination of employer learning with respect to noncognitive skills seems warranted. We have focused exclusively on cognitive skills that range from basic verbal and quantitative skills to vocationally-oriented skills. Ignoring skill importance, we conclude that employer learning does not differ across these skill types; a different conclusion might be reached if measures of conscientiousness, agreeableness, locus of

control, *etc*. are considered—although, as noted in section III, a lack of data on the occupation-specific importance of noncognitive skills is why we confine our attention to cognitive skills. Second, existing evidence (Pinkston 2009; Schönberg 2007) that employer learning is largely public rather than private might not hold up in an analysis that considers both alternative skill types and the role of occupation-specific skill importance.

Appendix A: O*NET Data

We use O*NET data to associate seven skill-specific importance scores (*IS*) with each unique job (defined as a spell with a given employer) in our NLSY79 data. These importance scores are used as regressors in select specifications of the log-wage model. O*NET refers to the Occupational Information Network, which is a data collection and dissemination project (replacing the Dictionary of Occupational Titles) sponsored by the Employment and Training Administration of the U.S. Department of Labor and conducted by the North Carolina Employment Security Commission. Details on the project and the data used for our analysis are available at www.onetcenter.org.

The O*NET database has descriptive information for 1,102 distinct occupations defined by the O*NET-SOC occupational taxonomy, which is modeled after the Standard Occupational Classification (SOC) taxonomy. The descriptive variables (referred to in O*NET documentation as "descriptors") consist of 277 distinct measures of the abilities, knowledge, skills and experience needed in the workplace as well as the tasks and activities associated with various types of work. These descriptors comprise the O*NET content model, which decomposes the various dimensions of work into three worker-oriented domains (worker characteristics, worker requirements, and experience requirements) and three job-oriented domains (occupational requirements, workforce characteristics, and occupation-specific information). Each domain contains a large set of measurable characteristics (descriptors). For example, the worker characteristics domain contains numerous measures of abilities that influence performance on the job, ranging from written comprehension to selective attention to explosive (physical) strength; it also contains measures of preferences for different work environments (artistic, social, *etc*.) and work styles that affect job performance (persistence, initiative, attention to detail, *etc*.). The worker requirements domain contains numerous types of knowledge, ranging from economics to

mathematics to telecommunications, while the experience requirements domain contains measures of the amount of experience (in writing, mathematics, programming, time management, *etc*.) needed to enter each occupation. Some descriptors measure the importance of an ability or type of knowledge to each occupation, others measure the frequency with which a type of knowledge is used or a task is performed, while others measure the impact of decisions, amount of experience needed, *etc*. We focus exclusively on descriptors that identify—using a scale of one to five—the *importance of select abilities and types of knowledge* for each occupation.

O*NET data are updated on a "rolling" basis by conducting a survey approximately every six months that focuses on a subset of occupations in the O*NET database. Each data collection effort involves randomly sampling businesses that are likely to employ workers in the selected occupations, randomly sampling workers within those businesses, and then randomly assigning the sampled workers questionnaires designed to elicit occupation-specific information associated with a subset of O*NET descriptors. The data collected from surveyed workers are used to score descriptors for their occupations.

We face a number of challenges in combining O*NET data with NLSY79 data and constructing occupation-specific importance scores (*IS*). First, we require a clear-cut mapping between our chosen NLSY79 skill measures and the associated O*NET importance scores. Table A1 briefly describes the skill that is measured by each of the seven ASVAB scores along with the O*NET descriptor that we use to measure the skill's "importance" on the job. Using word knowledge as an example, we are measuring sample members' "ability to select the correct meaning of words presented in context," and measuring the importance in their current job of

knowing "the meaning of words" as well as other language-related components.[27]

Second, we require uniform occupation codes in order to merge O*NET data with NLSY79 data. The O*NET database only contains O*NET-SOC codes, while for our observation period the NLSY79 provides 1970 3-digit Census occupation codes. We use a cross-walk to convert O*NET-SOC codes to DOT codes, and then another cross-walk to convert from DOT codes to 3-digit 1970 Census codes. In cases where multiple O*NET-SOC categories map into a given Census category, we compute the average O*NET importance score for that Census category.

Third, we need to associate each job reported in the NLSY79 with a single occupation code. Jobs that are reported by NLSY79 respondents in multiple interviews can have time-varying occupation codes. Temporal variation might reflect true changes in respondents' work assignments, or it might reflect the fact that verbatim job descriptions recorded in each interview are coded differently across interview rounds.[28] To skirt the within-job variation in occupation codes, we associate each job in the NLSY79 with the first-coded occupation; we also confirmed that using the modal or last-coded occupation does not affect our findings.

[27]We do not use the three remaining ASVAB scores (general science, auto/shop knowledge, electronics information) or the noncognitive skill measures available in the NLSY79 (Rotter Locus of Control, *etc*.) because it is much less obvious which O*NET descriptor would measure the importance of those skills on the job.

[28]From 1994 onward, within-job variation in occupation is reduced because new job descriptions were only elicited from survey respondents who first stated that their job responsibilities had changed. We cannot exploit this regime change because most respondents are well into their careers by 1994 and employer learning has been shown to be concentrated in the first few years (Lange 2007). Moreover, given the difficulties inherent in coding verbatim job descriptions, one-time reports do not necessarily produce more accurate occupation codes than multiple reports.

Table A1: Description of NLSY79 Skill Measures and Corresponding O*NET Importance Scores

NLSY79 skill measure (Z)	Description[a]	O*NET score (IS^z)	Description[b]
AFQT score	Composite of 4 raw ASVAB scores: AR+WK+PC+½·NO	—	—
Arithmetic reasoning (AR) ASVAB score 2	Ability to solve arithmetic word problems.	Mathematical reasoning[c]	Ability to choose the right mathematical methods or formulas to solve a problem.
Word knowledge (WK) ASVAB score 3	Ability to select the correct meaning of words presented in context.	English language[d]	Knowledge of the structure and content of the English language including the meaning and spelling of words, rules of composition, and grammar.
Paragraph comprehension (PC) ASVAB score 4	Ability to obtain information from written passages.	Written comprehension[c]	Ability to read and understand information and ideas presented in writing.
Numerical operations (NO) ASVAB score 5	Speed test of simple numerical calculations.	Number facility[c]	Ability to add, subtract, multiply, or divide quickly and correctly.
Coding speed ASVAB score 6	Speed test of finding numbers in a table.	Perceptual speed[c]	Ability to quickly and accurately compare similarities and differences among sets of letters, numbers, objects, pictures, or patterns.
Mathematics knowledge ASVAB score 8	Knowledge of high school mathematics principles.	Mathematics[d]	Knowledge of arithmetic, algebra, geometry, calculus, statistics, and their applications.
Mechanical comprehension ASVAB score 9	Knowledge of mechanical and physical principles.	Mechanical[d]	Knowledge of machines and tools, including their designs, uses, repair, and maintenance.

[a]Source and *NLSY79 User's Guide* available at http://www.nlsinfo.org/nlsy79/docs/79html/tableofcontents.html

[b]Source: *O*NET® Content Model: Detailed Model with Descriptions* available at http://www.onetcenter.org/dl_files/ContentModel_DetailedDesc.pdf.

[c]Ability measure from the Worker Characteristics section of the O*NET Content Model; abilities are defined as enduring attributes of the individual that influence performance.

[d]Knowledge measure from the Worker Requirements section of the O*NET Content Model; knowledge is defined as an organized set of principles and facts applying in general domains.

Appendix B: Supplementary Estimates

Table B1: Additional Estimates Corresponding to the Bottom Panel of Table 6A (Full Sample)

	Skill measure used as regressor (Z)							
Variable	AFQT	Arith. Reason.	Word Know.	Paragr. Comp.	Numer. Oper.	Coding Speed	Math. Know.	Mech. Comp.
Constant	.249	.242	.240	.230	.238	.241	.234	.239
	(.081)	(.081)	(.081)	(.081)	(.080)	(.080)	(.081)	(.081)
$X^2/10$	-.072	-.071	-.071	-.070	-.071	-.071	-.071	-.070
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)
$X^3/100$	.017	.016	.016	.016	.016	.016	.017	.016
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
black	-.080	-.079	-.081	-.079	-.080	-.079	-.078	-.080
	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)
black$\cdot X/10$	-.013	-.014	-.013	-.014	-.014	-.014	-.014	-.014
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Hispanic	-.015	-.014	-.014	-.013	-.013	-.012	-.015	-.012
	(.024)	(.024)	(.024)	(.024)	(.024)	(.024)	(.024)	(.024)
Hispanic$\cdot X/10$	.000	-.000	.000	-.000	-.000	-.000	.000	-.000
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Urban	.089	.092	.089	.090	.092	.092	.090	.091
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)

Note: The full sample consists of 22,907 observations for 3,069 men. Estimated coefficients for year dummies are not shown. Standard errors (in parentheses) are robust to clustering on individuals.

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Table 1: Means and Standard Deviations for Select Variables in Alternative Samples

Variable	Full sample	Occ. stayers	S=12	S=16	Blue collar	White collar
Log of CPI-deflated average hourly wage	1.99	2.01	1.89	2.41	1.92	2.25
	(.54)	(.56)	(.46)	(.53)	(.47)	(.61)
Highest grade completed (S)	12.59	13.09	12.00	16.00	11.80	14.57
	(2.17)	(2.31)	—	—	(1.49)	(2.32)
Years of potential experience (X)[a]	6.69	4.57	7.08	6.04	6.89	6.25
	(4.11)	(3.88)	(4.13)	(3.96)	(4.12)	(4.05)
$S{\cdot}X/10$	8.35	6.04	—	—	8.12	9.02
	(5.30)	(5.31)			(4.97)	(5.97)
1 if black	.25	.23	.29	.16	.25	.19
black·X	1.79	1.07	2.17	1.00	1.78	1.27
	(3.71)	(2.73)	(4.03)	(2.83)	(3.72)	(3.16)
1 if Hispanic	.16	.15	.15	.08	.17	.14
Hispanic·X	1.11	.72	1.10	.50	1.23	.94
	(3.04)	(2.32)	(3.05)	(2.07)	(3.21)	(2.78)
1 if urban	.75	.75	.73	.84	.71	.83
Raw AFQT score (Z)[b]	63.83	68.04	60.22	87.22	58.57	78.54
	(22.59)	(22.48)	(20.13)	(12.15)	(20.80)	(19.62)
Raw ASVAB scores (Z)[b]						
Arithmetic reasoning (AR)	16.33	17.68	15.12	23.88	14.70	20.93
	(7.39)	(7.60)	(6.46)	(5.73)	(6.57)	(7.28)
Word knowledge (WK)	22.88	24.28	21.56	30.82	21.12	27.79
	(8.62)	(8.32)	(8.07)	(4.05)	(8.33)	(7.04)
Paragraph comprehension (PC)	9.45	9.99	8.94	12.87	8.66	11.63
	(3.79)	(3.74)	(3.60)	(1.79)	(3.64)	(3.13)
Numerical operations (NO)	30.35	32.18	29.19	39.31	28.20	36.38
	(11.25)	(11.20)	(10.59)	(8.02)	(10.70)	(10.09)
Coding speed	38.20	40.79	36.40	50.04	35.27	46.51
	(15.63)	(15.45)	(14.50)	(12.55)	(14.78)	(14.45)
Mathematical knowledge	12.18	13.41	10.65	19.92	10.39	16.88
	(6.30)	(6.64)	(4.95)	(4.86)	(5.10)	(6.53)
Mechanical comprehension	14.09	14.86	13.66	17.75	13.44	16.31
	(5.59)	(5.59)	(5.46)	(4.49)	(5.53)	(5.22)
Number of observations	22,907	8,778	11,960	3,313	12,253	6,181
Number of men	3,069	3,069	1,461	480	1,516	953

[a]Elapsed months since first school exit, divided by 12.
[b]Raw ASVAB scores reflect the number of correct answers; raw AFQT score equals AR+WK+PC+0.5·NO. Regressions reported in tables 6-8 use standardized residual scores.
Note: All specifications also control for $Z{\cdot}X$, X^2, X^3 and calendar year dummies.

Table 2: Pearson Correlation Coefficients for Skill Measures and Select Covariates

Skill measure	S	Black	Hisp.	X_0	X_f	IS_0^z	IS_f^z
AFQT	.608	-.402	-.108	-.065	-.201	—	
Arithmetic reasoning	.574	-.374	-.114	-.078	-.191	.355	.346
Word knowledge	.546	-.395	-.093	-.054	-.195	.351	.378
Paragraph comp.	.533	-.334	-.105	-.048	-.173	.359	.366
Numerical operations	.500	-.297	-.071	-.044	-.144	.281	.268
Coding speed	.470	-.316	-.039	-.057	-.132	.099	.078
Math knowledge	.645	-.304	-.101	-.080	-.188	.293	.250
Mechanical comp.	.426	-.428	-.095	-.058	-.136	-.021	-.057

Note: The sample consists of one observation for each of the 3,069 in the full sample. Skill measures are raw (nonstandardized) scores. Potential experience (X) and importance scores (IS^Z) correspond to the first and last observation for each individual. All correlation coefficients are statistically distinguishable from zero at 1% significance levels.

Table 3: Pearson Correlation Coefficients for Skill Measures

Skill measure	Skill measure					
	Word Know.	Paragr. Comp.	Numer. Oper.	Coding Speed	Math. Know.	Mech. Comp.
Raw scores						
Arithmetic reasoning	.76	.74	.68	.62	.84	.72
Word knowledge		.83	.65	.62	.72	.72
Paragraph comp.			.64	.61	.70	.68
Numerical operations				.72	.66	.54
Coding speed					.61	.55
Math knowledge						.66
Residual scores						
Arithmetic reasoning	.54	.54	.47	.38	.69	.54
Word knowledge		.70	.43	.39	.48	.54
Paragraph comp.			.42	.39	.47	.49
Numerical operations				.58	.44	.31
Coding speed					.37	.33
Math knowledge						.47

Note: The sample consists of one observation for each of the 3,069 in the full sample. The top panel uses the raw (nonstandardized) scores; the bottom panel uses standardized, residual scores. All scores are statistically distinguish from zero at 5% significance levels.

Table 4: Importance Score Distributions (Full Sample)

Importance scores (IS^Z)	Percentile					Mean	S.D
	10th	25th	50th	75th	90th		
Arithmetic reasoning	1.88	2.05	2.26	2.56	2.94	2.33	.47
Word knowledge	2.68	2.79	3.06	3.42	3.74	3.14	.45
Paragraph comprehension	2.57	2.85	3.02	3.48	3.81	3.13	.45
Numerical operations	2.00	2.16	2.32	2.59	2.79	2.37	.41
Coding speed	2.47	2.61	2.80	2.91	3.03	2.77	.24
Mathematical knowledge	2.52	2.69	2.89	3.09	3.46	2.91	.41
Mechanical comprehension	1.58	1.84	2.77	3.14	3.49	2.60	.75

Note: The full sample consists of 22,907 observations for 3,069 men. Importance scores range from one to five, and correspond to the three-digit occupation corresponding to each observation.

Table 5: Importance Scores and Growth in Variance of Log Wage Residuals (GRV) for Select Occupations

Code[a]	Occupation	GRV[b]	Importance Score						
			Arithmetic reasoning	Word knowledge	Paragraph comp.	Numerical operations	Coding speed	Mathematical knowledge	Mechanical comp.
031	Lawyer	-0.116	2.280	**4.525**	**4.253**	2.285	2.780	2.663	1.448
035	Mathematician	0.002	**3.875**	3.875	3.690	**3.565**	2.690	**4.260**	1.490
044	Biological scientist	-0.064	3.321	4.193	4.139	3.156	2.988	3.718	1.959
065	Physician	0.291	2.668	4.218	4.184	2.688	3.099	3.023	1.606
144	Teacher (secondary)	0.067	2.876	4.399	4.016	2.655	2.781	3.519	1.796
182	Dancer	-0.035	***1.630***	***2.780***	3.315	***1.755***	***2.625***	***2.100***	***1.200***
345	Key punch operator	0.087	2.630	3.710	4.250	2.380	**3.750**	2.950	1.480
473	Auto mechanic	0.054	2.150	3.127	3.046	2.216	2.820	2.959	**3.969**
715	Truck driver (light)	0.067	2.005	3.088	***2.815***	2.253	2.878	2.530	2.765

[a]1970 Census three-digit occupation code.

[b]Mean growth in variance of log wage residual for corresponding 1980 Census two-digit occupation, as reported in table 1 of Mansour (2012).

Note: High (low) scores for each column are in bold (italics). See appendix A for details on O*NET scores.

Table 6A: Estimates for Model 6 Using Alternative Skill Measures (Full Sample)

Variable	Skill measure used as regressor (Z)							
	AFQT	Arith. Reason.[a]	Word Know.[a]	Paragr. Comp.[a]	Numer. Oper.[a]	Coding Speed	Math. Know.	Mech. Comp.
Z independent of birth year only[b]								
Z	.029	.033	.009	.007	.042	.028	.037	.022
	(.012)	(.010)	(.009)	(.009)	(.009)	(.009)	(.010)	(.009)
$Z{\cdot}X/10$	.067	.059	.069	.058	.059	.046	.060	.055
	(.011)	(.013)	(.013)	(.013)	(.012)	(.012)	(.014)	(.013)
S	.093	.093	.097	.097	.092	.094	.091	.095
	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)
$S{\cdot}X/10$	.015	.008	.007	.008	.008	.011	.008	.013
	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)
X	.100	.108	.107	.106	.109	.104	.111	.100
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.014)	(.013)
Root MSE	.448	.450	.451	.452	.448	.451	.450	.451
Z independent of birth year, S, and all other covariates[b]								
Z	.023	.023	.012	.007	.035	.026	.026	.018
	(.007)	(.008)	(.007)	(.008)	(.008)	(.008)	(.008)	(.008)
$Z{\cdot}X/10$	.065	.048	.053	.047	.053	.042	.047	.050
	(.010)	(.011)	(.010)	(.011)	(.010)	(.011)	(.011)	(.011)
S	.098	.097	.098	.098	.098	.098	.097	.097
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
$S{\cdot}X/10$	.015	.017	.017	.016	.016	.017	.019	.018
	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)
X	.097	.095	.095	.096	.096	.095	.096	.096
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)
Root MSE	.448	.450	.451	.452	.448	.450	.450	.451

[a]These four ASVAB scores are used to compute AFQT scores.

[b]In the bottom panel, all Z are standardized, residual test scores obtained by regressing each test score on birth year dummies and starting/ending values of all covariates (including IS), as detailed in section III.B. In the top panel, we use standardized, residual test scores obtained by regressing each test score on birth year dummies only.

Note: The full sample consists of 22,907 observations for 3,069 men. All specifications include controls for X^2, X^3, black, Hispanic, black$\cdot X$, hispanic$\cdot X$, urban, and year dummies; see table B1 for additional parameter estimates corresponding to the bottom panel. Standard errors (in parentheses) are robust to clustering on individuals.

Table 6B: Estimates for Model 6 Using Alternative Skill Measures
(Subsamples of occupation stayers and importance score stayers)

	Skill measure used as regressor (Z)							
Variable	AFQT	Arith. Reason.[a]	Word Know.[a]	Paragr. Comp.[a]	Numer. Oper.[a]	Coding Speed	Math. Know.	Mech. Comp.
Occupation								
Z	.015	.018	.002	.003	.030	.020	.025	.009
	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)
$Z{\cdot}X/10$	.062	.053	.044	.039	.046	.040	.035	.044
	(.025)	(.023)	(.027)	(.024)	(.022)	(.021)	(.023)	(.024)
S	.093	.093	.094	.094	.093	.093	.094	.093
	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)
$S{\cdot}X/10$	.032	.031	.033	.032	.035	.035	.030	.032
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)
X	.110	.111	.109	.110	.104	.104	.114	.109
	(.022)	(.022)	(.022)	(.022)	(.021)	(.022)	(.022)	(.022)
Root MSE	.442	.442	.444	.444	.441	.443	.442	.443
Import. Score								
Z		.019	.002	.004	.031	.021	.026	.011
		(.009)	(.008)	(.009)	(.009)	(.009)	(.009)	(.009)
$Z{\cdot}X/10$		.065	.053	.045	.052	.035	.030	.047
		(.021)	(.022)	(.020)	(.019)	(.019)	(.021)	(.022)
S		.093	.095	.094	.091	.095	.095	.094
		(.005)	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)
$S{\cdot}X/10$		.030	.028	.033	.034	.033	.035	.030
		(.012)	(.012)	(.012)	(.011)	(.011)	(.012)	(.012)
X		.110	.116	.104	.099	.099	.104	.110
		(.020)	(.019)	(.020)	(.019)	(.019)	(.020)	(.020)
Root MSE		.444	.445	.443	.444	.443	.444	.444
Observations		9,725	10,000	9,779	9,897	10,273	9,911	9,471

[a]These four ASVAB scores are used to compute AFQT scores.

The subsample of occupation stayers consists of 8,778 observations for the 3,069 men in the full sample; each man contributes observations as long as he maintains his initial three-digit occupation. Each subsample of importance score stayers consists of observations for the 3,069 men in the full sample; each man contributes observations as long as his skill-specific importance score varies less than 0.1 relative to his initial score, so sample sizes are skill-specific. All specifications include controls for X^2, X^3, black, Hispanic, black$\cdot X$, hispanic$\cdot X$, urban, and year dummies; the Z are standardized residual test scores. Standard errors (in parentheses) are robust to clustering on individuals.

Table 6C: Estimates for Model 6 Using Alternative Skill Measures
(Subsamples of men with schooling=12 or 16, and men in blue or white collar occupations)

Variable	Skill measure used as regressor (*Z*)							
	AFQT	Arith. Reason.[a]	Word Know.[a]	Paragr. Comp.[a]	Numer. Oper.[a]	Coding Speed	Math. Know.	Mech. Comp.
Schooling=12								
Z	.027	.021	.015	.014	.047	.038	.021	.011
	(.010)	(.010)	(.010)	(.011)	(.010)	(.011)	(.010)	(.010)
Z·X/10	.067	.053	.060	.046	.052	.048	.053	.057
	(.013)	(.013)	(.012)	(.013)	(.013)	(.014)	(.013)	(.014)
Schooling=16								
Z	.043	.030	.001	.009	.054	.040	.056*	.026
	(.026)	(.020)	(.030)	(.029)	(.025)	(.020)	(.021)	(.021)
Z·X/10	.084	.048	.042	.056	.045	.064	.059	.039
	(.038)	(.027)	(.045)	(.045)	(.037)	(.039)	(.028)	(.032)
Root MSE	.437	.440	.440	.442	.436	.438	.439	.441
Blue collar								
Z	.025	.025	.010	.005	.045	.029	.024	.017
	(.010)	(.010)	(.010)	(.011)	(.010)	(.011)	(.010)	(.010)
Z·X/10	.054	.041	.052	.044	.030	.021	.044	.038
	(.013)	(.014)	(.012)	(.013)	(.013)	(.014)	(.014)	(.013)
S	.081	.083	.082	.083	.080	.082	.083	.082
	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)
S·X /10	-.000	.003	.000	.000	.001	.002	.009	.003
	(.010)	(.010)	(.010)	(.010)	(.010)	(.010)	(.010)	(.010)
White collar								
Z	.038	.027	.029	.035*	.036	.039	.048	.040
	(.015)	(.014)	(.016)	(.016)	(.016)	(.014)	(.013)	(.015)
Z·X/10	.078	.055	.040	.049	.078**	.043	.032	.041
	(.025)	(.022)	(.028)	(.026)	(.025)	(.025)	(.022)	(.024)
S	.114***	.111**	.113***	.114***	.111***	.113***	.111***	.112***
	(.008)	(.009)	(.008)	(.008)	(.009)	(.008)	(.008)	(.008)
S·X/ 10	.022	.020	.019	.019	.024*	.022	.020	.023
	(.012)	(.013)	(.013)	(.013)	(.012)	(.013)	(.013)	(.013)
Root MSE	.445	.447	.448	.448	.445	.448	.447	.448

[a]These four ASVAB scores are used to compute AFQT scores.

*, **, and *** indicate that the p-value for the null hypothesis that the two parameters are equal across types (S=12 and S=16, or blue and white collar) is less than or equal to 0.15, 0.10, and 0.01.

Note: The schooling subsample consists of 11,944 observations for 1,461 men with S=12 and 3,312 observations for 480 men with S=16; the occupation subsample consists of 12,278 observations for 1,516 men in blue collar occupations and 6,188 observations for 953 men in white collar occupations. All specifications include the full set of controls described in table 6A (with S and S·X dropped from the schooling sample) fully interacted with dummies indicating *S*-level or blue/white collar status; the *Z* are standardized, residual test scores. Standard errors (in parentheses) are robust to clustering on individuals.

Table 7: Estimates for Model 10 Using Alternative Skill Measures (Full Sample)

Variable	Skill measure used as regressor (Z)						
	Arith. Reason.[a]	Word Know.[a]	Paragr. Comp.[a]	Numer. Oper.[a]	Coding Speed	Math. Know.	Mech. Comp.
IS^Z	.134	.124	.159	.133	.253	.187	.049
	(.015)	(.018)	(.017)	(.016)	(.024)	(.016)	(.009)
Z	.040	.097	.084	.108	-.022	-.051	-.032
	(.037)	(.056)	(.052)	(.040)	(.078)	(.055)	(.027)
$Z{\cdot}IS^Z$	-.008	-.029	-.026	-.032	.017	.025	.019
	(.016)	(.018)	(.017)	(.017)	(.029)	(.019)	(.010)
$Z{\cdot}X/10$	.074	.035	.010	.052	.117	.228	.170
	(.063)	(.090)	(.082)	(.062)	(.137)	(.086)	(.041)
$Z{\cdot}X{\cdot}IS^Z/10$	-.011	.006	.011	.000	-.027	-.062	-.045
	(.027)	(.030)	(.027)	(.027)	(.050)	(.029)	(.014)
S	.084	.082	.078	.087	.093	.086	.102
	(.005)	(.005)	(.005)	(.005)	(.004)	(.004)	(.004)
$S{\cdot}X/10$	.020	.020	.021	.019	.019	.025	.019
	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)
X	.090	.090	.089	.093	.087	.085	.091
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)
Root MSE	.447	.449	.448	.445	.447	.444	.449

[a]These four ASVAB scores are used to compute AFQT scores.

Note: The full sample consists of 22,907 observations for 3,069 men. All specifications include controls for X^2, X^3, black, Hispanic, black·X, hispanic·X, urban, and year dummies; the Z are standardized, residual test scores. Standard errors (in parentheses) are robust to clustering on individuals.

Table 8: Estimates for Modified Model 10 with Nonlinear *IS* Effects (Full Sample)

Variable	Skill measure used as regressor (Z)						
	Arith. Reason.[a]	Word Know.[a]	Paragr. Comp.[a]	Numer. Oper.[a]	Coding Speed	Math. Know.	Mech. Comp.
High IS^c=1 if in top quartile of Z-specific distribution							
IS^c	.169	.100	.153	.157	.207	.147	.044
	(.019)	(.026)	(.022)	(.020)	(.031)	(.022)	(.012)
IS^c ·high IS^c	-.054	.032	.009	-.030	.039	.054	.010
	(.021)	(.026)	(.023)	(.020)	(.017)	(.020)	(.019)
Z	.025	.014	.010	.045	.018	.021	.012
	(.009)	(.008)	(.009)	(.008)	(.008)	(.008)	(.009)
Z·high IS^c	-.014	-.020	-.023	-.054	.029	.002	.020
	(.016)	(.020)	(.020)	(.019)	(.018)	(.018)	(.016)
$Z{\cdot}X/10$	.052	.052	.041	.051	.046	.059	.063
	(.012)	(.010)	(.011)	(.011)	(.012)	(.012)	(.013)
$Z{\cdot}X/10$·high IS^c	-.018	-.001	.021	.007	-.022	-.042	-.045
	(.024)	(.033)	(.029)	(.025)	(.027)	(.025)	(.022)
Root MSE	.447	.449	.448	.444	.446	.444	.449
High IS^c=1 if $IS^c \geq 3.25$							
IS^c	.129	.076	.128	.135	.262	.186	.037
	(.015)	(.025)	(.021)	(.016)	(.025)	(.021)	(.010)
IS^c ·high IS^c	.072	.067	.048	-.019	-.322	.002	.057
	(.075)	(.027)	(.024)	(.030)	(.069)	(.028)	(.022)
Z	.020	.014	.009	.033	.025	.022	.014
	(.007)	(.008)	(.008)	(.008)	(.007)	(.008)	(.008)
Z·high IS^c	.161	-.019	-.020	.459	.240	.013	.017
	(.112)	(.022)	(.022)	(.055)	(.033)	(.027)	(.028)
$Z{\cdot}X/10$	.049	.052	.042	.053	.042	.050	.060
	(.011)	(.010)	(.011)	(.010)	(.011)	(.011)	(.012)
$Z{\cdot}X/10$·high IS^c	.011	-.005	.015	-1.639	-.390	-.029	-.078
	(.162)	(.035)	(.031)	(.306)	(.046)	(.040)	(.031)
Root MSE	.447	.449	.448	.445	.446	.444	.449

[a]These four ASVAB scores are used to compute AFQT scores.

Note: The full sample consists of 22,907 observations for 3,069 men. All specifications include controls for S, $S{\cdot}X$, X, X^2, X^3, black, Hispanic, black·X, hispanic·X, urban, and year dummies; the Z are standardized, residual test scores. Standard errors (in parentheses) are robust to clustering on individuals.