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2|2024 Panel Evidence on Within-Occupation Change in Job Tasks and Individual Wages

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Panel Evidence on Within-Occupation Change in Job Tasks and Individual Wages

Gerrit Müller (IAB)

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Abstract

Drawing on newly available panel data, this paper presents an empirical analysis of the wage effects of changing job tasks, assessed for individuals at their workplace. I am therefore able to exploit within-occupation within-individual variation, over time, to study wage returns to cognitive, interpersonal, physical and routine task intensity. The findings of Autor and Handel's (2013) pioneering work on the significance of such within-occupation ("intensive margin") task variation are reassessed. Unobserved worker attributes and ongoing self-selection into occupations can be accounted for in a much more comprehensive way than possible with purely cross-sectional data, as in the original work.

Zusammenfassung

Auf Grundlage neu verfügbarer Paneldaten präsentiert dieses Papier eine empirische Analyse der Lohneffekte sich verändernder Tätigkeiten am individuellen Arbeitsplatz. Unter Ausnutzung der Datenvariation innerhalb von Berufen und Individuen, über die Zeit, werden Lohnrenditen für die Ausübung von kognitiven, interpersonalen, physischen und Routine-Tätigkeiten geschätzt. Die Erkenntnisse der einflussreichen Studie von Autor und Handel (2013) über die Bedeutung des Tätigkeitswandels innerhalb von Berufen („intensive Margin“) werden erneut untersucht. Insbesondere kann individueller unbeobachteter Heterogenität und Selbstselektion in Berufe besser Rechnung getragen werden, als dies in der ursprünglichen Querschnittsstudie möglich war.

JEL classification

J24, J31

Keywords

Job tasks, occupations, routine-biased technical change, wages, workplaces

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1 Introduction

This paper complements the existing empirical literature on occupational tasks and workers' wages with an explicit focus on that part of task change happening at the job-level, that is, within and independent of occupation. While numerous influential studies, starting with the work by Autor/Levy/Murnane (2003), have considered occupational task requirements, employment and wage outcomes, evidence on the within-occupation dimension ("intensive margin") is scarce. The few pioneering studies that do exist on this topic, the most prominent one being by Autor/Handel (2013), are all cross-sectional in nature, which poses clear limitations on the analyses. This points out a gap in the literature that the present work seeks to address for the first time.

Since the paper by Autor/Levy/Murnane (2003, henceforth ALM), the task-based approach to the labor market has sparked off a large body of empirical literature concerned with the study of technology-induced change in task requirements and associated shifts in employment and wages. Much of this work follows the routinization hypothesis (or routine-biased technical change, RBTC) according to which progressing computerization and automation substitute for routine tasks heretofore done by humans (e.g. Levy/Murnane 1996; Levy 2010). At the current state of technology, "routine" comprises any task that is rule-based and codifiable in form of program routines to be followed and executed by a machine or an algorithm. With prices for computing power and automation ever falling, firms shift their demand for tasks carried out by human labor away from routine, towards nonroutine; which may induce workers to adapt and relocate (e.g. across occupations).¹

At the heart of the task-based approach lies a conceptual distinction between human productive attributes (innate ability, acquired skills) and the batches of main work activity (tasks) to which these attributes are applied, alongside conjectures on the shifting boundary between tasks done by humans and those done by machines. In this regard, the original foundation of the ALM approach is a rather fine-grained, qualitative, consideration of what it is that individuals do at work, and what thereof machines are increasingly able to take over (ALM: 1282–86, and the works cited therein). This initial micro-perspective on workers' job content and task shifts experienced at their workplaces has somewhat been lost, however. One central reason for this state of affairs is measurement, as e.g. discussed in Autor/Handel (2013: S61–62), or the literature overview on the task approach by Autor (2013: 189–192). Most large-scale labor market surveys commonly used in the field simply lack detailed information on individuals' main work activities. Much of the early polarization literature therefore studies (change in) task content in terms of occupational affiliation of workers, and considers occupation-level employment and wage outcomes; most often drawing on aggregate data. Even when worker samples are available and individual-level outcomes can be considered (e.g. in Firpo/Fortin/Lemieux 2011; Cortes 2016; Cavaglia/Etheridge 2020; Böhm 2020), the key explanatory variables capturing workers' job task content remain occupation-level.

¹ This line of reasoning offers one explanation for "polarization" phenomena in occupational employment and wages observed both in the United States and Europe (e.g. Autor/Katz/Kearney 2006; Goos/Manning 2007; Autor/Dorn 2009; Goos/Manning/Salomons 2009, 2014).

The auxiliary assumption that each occupation equates to a characteristic bundle of main work activities delivers the missing link, and has become standard practice in empirical implementations of a task-based approach. Codes from statistical classification systems of occupations (SOC, ISCO, etc.) are readily available in most data sources on the labor market and therefore used as proxies. These codes are commonly aggregated up into (often only less than a handful of) broader categories assumed to represent similar contents, and intensities, in terms of the tasks demanded from workers (e.g. routine–nonroutine). This grouping of occupations either follows the hierarchical logic of the underlying classification system, or happens manually, judgement-based. Sometimes, expert ratings of the typical (average) task content of jobs within a given occupation are merged from databases such as the U.S. Dictionary of Occupational Titles (DOT), or the Occupational Information Network (O*NET), to either qualitatively inform a particular grouping decision (as in e.g. Autor/Dorn 2013), or to construct quantitative (metric) scales for occupation-level task content (as in e.g. Ross 2017).

What is thereby necessarily disregarded is that part of variation in tasks affecting workers within, and independent of, their occupation. It is easily conceivable that different workers holding the same occupational title (or assigned by researchers to the same aggregate statistical group) may be facing quite different task requirements at their respective workplace. Even “identical” workers, in terms of their education and other individual productive attributes, holding the same occupation, are not necessarily engaged in the same main activities; shaped by the specific goods or services provided, organizational processes and workflows within a firm, and so forth. Likewise, the development over time in jobs’ task composition may be as heterogeneous. For some individuals, the kind of task change experienced at their workplace could even run counter to any “global” trend, such as the ubiquitous shift in requirements away from routine towards nonroutine. Yet, so far, task variation at this level of granularity has rarely been considered empirically.

The micro-level panel data used in this paper opens up new possibilities for analysis. A standardized set of task measures adopted from the U.S. STAMP survey (Handel 2007, 2008) and administered to participants in the Adult Cohort of the German National Educational Panel Study (NEPS) assesses the main tasks performed by individuals at their workplace(s) over time. This allows for a follow-up on the pioneering work by Autor/Handel (2013, henceforth AH), who were first to investigate the value added of person-level job task measures in explaining wages based on one cross-section of data from the Princeton Data Improvement Initiative (PDII). Decomposing the variance of each one of their core job task measures (abstract, manual, and routine requirements) into within- and between-occupation components, AH find that there is as much variation within occupations as there is between them.² While this confirms the importance attributed to workers’ occupational affiliation in determining their tasks, it also points towards a very sizeable “residual” variation, typically assumed away and discarded. Further, AH present evidence that this within-occupation (“intensive margin”) variation is an important predictor of individual wages.

As it stands, their paper is still the reference work against which to assess one’s own findings. Of course, AH are well aware of the limitations the cross-sectional data impose on their analyses.

² With some slight variation by the task domain considered (see AH: S80, Table 4).

They clearly point out that the wage premia and penalties associated with their job task measures are to be viewed as suggestive (of causal effects), and likely be confounded by a number of known selection issues. First, they cannot exclude that the within-occupation variation in job tasks is not merely reflecting unobserved variation at the individual level. Second, omitted individual attributes may not only be correlated with job tasks within the chosen occupation, but also be a key driver of the occupational choice in the first place. AH's conceptual discussion of the canonical Roy model of occupational self-selection (Roy 1951), in combination with some of the empirical tests presented, may therefore be viewed as a call for re-approaching the matter in a way that was unavailable to them. What follows from their discussion, in particular, is that the productive value of an individual's vector of unobserved abilities (efficiency in exercising certain tasks) is dependent on the occupation, and may hence be rewarded differentially across alternative choices. In order to estimate the within-occupation wage effects of job tasks as unbiased as possible, one therefore not only needs to keep constant the unobserved abilities of individuals, alongside with their occupation, but also take into account the occupation-specificity of the (returns to these) unobservables.

Given the available panel data, an empirical strategy that nests such a Roy-type model is to exploit variation within unique worker-occupation combinations. Hereby, not only unobserved individual and occupation characteristics are accounted for separately and additively (as e.g. in Ross 2017). The conditioning is on the interaction between individual and occupation. Since occupation-specific returns to individual unobservables are characteristics of a given "pair", time-demeaning within each worker-occupation combination sweeps the unobserved selection factors at the desired level. The same identification strategy has recently been used in related work by Cortes (2016) and Cavaglia/Etheridge (2020). In line with these authors, the unique individual-occupation combinations are called spells and, henceforth, spell-fixed effects estimation of the wage equations.

In anticipation of some of my results, let me briefly note here that when I replicate AH's cross-sectional analyses using my data in a similar way (i.e. by pooling the panel waves, and using a similar variety of controls), I find quite comparable coefficients on the job task variables in terms of sign, statistical significance, and magnitude (change in log wages due to 1SD change in a task measure). This is quite remarkable as there are some notable differences between the two data sources in terms of the survey and questionnaire design, country/labor market and time period(s) considered. When comparing these cross-sectional estimates with those obtained from the spell-fixed effects specification, the changes in return estimates are consistent with strong worker sorting based on unobservable ability differences. For example, the large and statistically significant wage premium to exercising abstract tasks (a combination of cognitive and interpersonal requirements) found before, vanishes. Likewise, the large and significant wage penalty to exercising manual tasks is reduced by about three-quarters. The task domain that turns out to be most robustly related to wages, negatively so, is routineness. It shows the smallest relative change in coefficient when moving from the cross-sectional to the spell fixed effects reference specification (a reduction by about half). Investigating some effect heterogeneities, I find that the relation between routineness and wages is moderated by what other tasks individuals are required to perform concurrently at their job. Some of these tasks appear to lend protection against, while others amplify, routineness' adverse effects on workers'

wages. Although my findings clearly confirm AH's concern about ability-related sorting issues, they do not reject and rather support their claim that within-occupation job task variation is a relevant component of overall task change, warranting separate and equal consideration.

There have been earlier attempts to follow-up on AH, notably by Cassidy (2017), Rohrbach-Schmidt (2019) and Storm (2020), who all draw on individual-level job task data available from the German Qualification and Career Survey (GQCS). Next to the British Skills Survey (BSS) (Felstead et al. 2007; Felstead/Green 2008), the GQCS is one of the very few data sources internationally that allows for a within-occupation perspective (e.g. Spitz-Oener 2006; Black/Spitz-Oener 2010). However, as the BSS, the GQCS is designed as a repeated cross-section. This places clear limits on any empirical analysis in accounting for individual unobservables and their role in occupational self-selection in much the same way as in AH's original paper. Starting back in 1979, the GQCS has also not been specifically designed with a task-based approach in mind. Moreover, there are only few questionnaire items on job tasks with a consistent, unaltered, question wording across consecutive cross-sections over the last decades (see Rohrbach-Schmidt/Tiemann 2013).

As already mentioned before, there is a related literature that draws on individual-level panel data to consider wage returns to occupational tasks and affiliation (e.g. Cortes 2016, Cavaglia/Etheridge 2020). The panel dimension of their data allows these authors to control for time, individual and occupation fixed effects, as well as the interaction among the latter two (i.e. spell fixed effects). Selection issues are thereby addressed in a much more comprehensive way than in AH or other prior work. Cortes' paper, based on the U.S. Panel Study of Income Dynamics (PSID), has been the first to use individual-level panel data to estimate occupational task premia, applying spell fixed effects estimation. Workers' tasks, however, are again proxied for by an aggregation of detailed occupation codes into three broad categories (routine, nonroutine cognitive, nonroutine manual). Moreover, any detailed occupation code's assignment to one of these aggregate categories is fixed at the beginning of the observation period, and kept constant over time. In a very similar vein, Cavaglia and Etheridge use individual-level panel data of the British Household Panel Survey (BHPS) and German Socio-Economic Panel (GSOEP) to estimate occupational task returns for two major European economies.

The implicit assumption in all these studies is that workers employed in the same aggregate occupational group are exposed to identical task requirements at their workplaces, and change therein. This view is clearly challenged by AH's key finding of a large and independent share of job-level task variation within occupations. Furthermore, if one assigns occupations to broad task categories once and for all, neither the average occupational task content is allowed to change over time, nor the individual job content within. In light of the long time periods spanned by the PSID, BHPS, GSOEP with ongoing technical change and changing requirements, this unavoidable restriction appears particularly unfavorable.

Perhaps closest in spirit to the present work is Ross (2017). Following Cortes (2016), he draws on PSID data and employs a fixed effects estimation approach. Instead of grouping detailed occupation codes into very broad, time-invariant categories, he merges information from the O*NET expert database. By using the occupational affiliation of the individual workers in his sample (at any given point in time) as a link, he constructs scores for an occupation's routine and abstract task intensity. This strategy allows him to consider time-variation in occupational task

requirements (i.e. group-level means) to explain changes in individual workers' wages. By construction, any worker in a given occupation, at a given point in time, is treated identical to any other worker within that occupation in terms of the tasks performed. So, again, there is no within-occupation variation across individuals and their workplaces.

In a nutshell, existing empirical studies drawing on individual-level panel data have been able to address selection issues in a more convincing way than the early aggregate-level task literature, yet, owing to data constraints, remain confined to the occupational level in terms of their key explanatory variables. A number of studies employing cross-sectional data sources such as the BSS, PDII, and GQCS, has been able to consider within-occupation variation in workers' tasks, but at the same time been unable to account for individual (and other) fixed effects. This points out a gap in the current empirical task literature, which the present paper is able to address for the first time.

The remainder of the article is structured as follows. The next section outlines the empirical model used to estimate the wage effects of job tasks. Section 3 describes the new data set and the measurement instrument. Section 4 provides some initial descriptive analyses of the task measures, their variation between and within occupations, as well as their intercorrelations and developments over time. Section 5 estimates a series of wage equations (increasingly stringent, by an expanding set of fixed effects) to examine individual wage gains and losses attributable to job-level task change. Section 6 explores effect heterogeneities of the focal routine task domain which has received the most attention in the literature. Section 7 reports on several robustness checks. Section 8 summarizes my findings and concludes.

2 Empirical Implementation

Interest centers on estimating the within-occupation wage effects of changes in abstract (cognitive and interpersonal), physical and –in particular– routine job tasks demanded from statistically identical workers. As in all related empirical studies the question is how, in the absence of experimentally controlled random variation, it is possible to adjust for unmeasured correlates exerting their own effect on wages as well as on the intermediate selection step of occupational choice. The common theoretical modelling approach here is some version of the canonical Roy (1951) model, as detailed and applied in e.g. AH, Cortes (2016), Cavaglia/Etheridge (2020), Firpo/Fortin/Lemieux (2011), and Ross (2017). A parsimonious yet powerful empirical strategy that nests such a model of occupational self-selection is multi-way fixed effect estimation. More specifically, I shall follow the approach developed in Cortes (2016), also adopted by Cavaglia and Etheridge (2020), who condition on spell fixed effects, that is, characteristics of unique worker-occupation combinations. Note, that this presents a much stricter way of purging estimated task prices from confounding influences than merely adjusting for fixed occupation effects (as e.g. in the initial cross-sectional work by AH) or for occupation and individual effects separately and additively (as in other more recent panel data-based studies, e.g. by Ross 2017). The reason is that time-demeaning the data within spells not only sweeps out any fixed occupational and individual characteristics but also removes any characteristics of the match. In particular, the differential returns workers' unobserved

productive attributes may receive across alternative occupational choices are thereby accounted for –a key driver of self-selection in Roy-type models based on comparative advantage.

As the empirical framework for task price estimation has by now been presented several times in recent works (e.g. in very much detail by Cavaglia/Etheridge 2020: section 2.1), I shall go straight to the wage regressions to be estimated, after briefly introducing the most basic notation needed. The dataset consists of individual workers $i = 1, \dots, N$ interviewed at various points in time $t = 1, \dots, T_i$ while being employed in occupation $j = 1, \dots, J$. Since workers are not only nested within occupations (in the sense of “clusters”), but may of course move, the mapping of worker i to occupation j at time t is denoted by function $j(i, t)$. There are hence three margins of heterogeneity –workers, occupations and pure time– that (minimally) need to be eliminated in order to identify job-level task change and its effect on wages. Equation [1] denotes a generic formulation of the reference wage equation estimated in this paper, which fully encompasses those estimated in the related occupation-level literature as I shall highlight below.

$$\ln w_{it} = \mathbf{tasks}_{it}\beta + a_i + \omega_{j(i,t)} + \delta_t + \mathbf{x}_{it}\zeta + \varepsilon_{it} \quad [1]$$

The dependent variable is (log) real hourly earnings x_{it} of worker i in year t , and is regressed on a vector of time-varying job task measures comprising four {cognitive; interpersonal; physical; routine} domains. The coefficients in vector β are the key parameters of interest. The model accounts for individual worker fixed effects a_i , an array of occupation fixed effects $\omega_{j(i,t)}$, time fixed effects δ_t , further time-varying individual-level observables x_{it} (e.g. job spell tenure), plus some idiosyncratic error ε_{it} assumed to be satisfying strict exogeneity, as is conventional.

Since I am not interested in the unobserved fixed error components per se but only in consistently estimating the coefficients of the four task variables, I can simply time-demean the data within-individuals within-occupations. As explained, instead of doing this separately and additively, I define spell fixed effects as $\gamma_{ij} \equiv a_i + \omega_{j(i,t)}$ and subtract averages at the spell level. Further subsuming the fixed time effects under the vector of observable covariates, one obtains the following compact notation for the wage equation to be estimated:

$$\ln w_{it} = \mathbf{tasks}_{it}\beta + \gamma_{ij} + \mathbf{z}_{it}\zeta + \varepsilon_{it} \quad [2]$$

Note that this closely resembles equation (5) in Cortes (2016) and equation (3) in Cavaglia and Etheridge (2020), which can basically be written as follows:

$$\ln w_{it} = \theta_{jt} + \gamma_{ij} + \mathbf{z}_{it}\zeta + \varepsilon_{it} \quad [3]$$

The main difference is that the indicator variables for workers’ affiliation with some broad occupational group (e.g. “routine”, “abstract”, “manual”) at a certain point in time, denoted as time-varying occupation fixed effects θ_{jt} , are no longer the estimands of interest. I have run specifications including them as additional controls though, with little impact on the estimates (see section 7).

3 Data

My analysis builds on data available from the Adult Cohort of the National Educational Panel Study (NEPS) for Germany. Overall, the NEPS is one of the largest scientific survey projects in the social sciences conducted in Germany in recent years.³ It is designed as a multi-cohort panel study that tracks individuals' educational pathways and skill formation from birth and early childhood on, through elementary, secondary and higher education, vocational training, up until entering the labor market and beyond. With some slight variation by starting cohort, the NEPS was inceptioned in 2010 with annual rounds of interviews ever since.

The Adult Cohort data I use for my analysis covers the working-age population. Every fourth wave, a specific module on job tasks is rotated into the Adult Cohort's regular annual question program. The tasks module was first introduced in 2011/12 and readministered in the 2015/16 and 2019/20 waves.⁴ This yields a panel of person-job level observations with tasks measured at three distinct points in time, evenly spaced over the entire last decade. While one could wish for more observations per individual, the longer lag between any two measurement points is very favorable in the given context, as it mitigates the danger of picking up mostly spurious change in the reported task content of work.

The fact that these time-varying job tasks are assessed at the person-job level, irrespective of the occupation held, really sets the NEPS dataset apart.

3.1 Measurement of Job Tasks

The conceptual considerations that guided the development of the NEPS job tasks module are fully documented in Matthes et al. (2014), who are its originators. The instrument is constructed very much along the theoretical lines of the ALM framework. From a practical point of view, the U.S. STAMP survey (Handel 2007, 2008) served as a blueprint. Quite a number of questions are directly adapted and translated, albeit some narrowing down of the original item battery and modifications of response scales. In light of arguments in favor of standardized “off the shelf” measures (e.g. Autor 2013: 195), the NEPS clearly offers an assessment of job tasks that strives to connect well to the established international literature. Further questionnaire items were added in complement from the German Qualification and Careers Survey (GQCS), known from studies by Spitz-Oener (2006) or Gathmann/Schönberg (2010), in order to further tailor the instrument to the German labor market.

Following the theoretical discussion, focus lies on the “core” domains that feature prominently in the ALM framework: abstract tasks, represented by cognitive and interpersonal job demands, physical tasks, and –assessed independently of the preceding three domains– the routineness of job tasks. Table A1, in the paper appendix, details the full item list with question texts grouped by

³ The NEPS is a multi-million-euro project financed by the Federal Ministry of Education and Research (Bundesministerium für Bildung und Forschung, BMBF). It is supported by well over a dozen research institutes and university departments all across Germany, headed by the Leibniz Institute für Educational Trajectories (LIfBi) in Bamberg. For more details on the entire project, see the compendium by Blossfeld/Rosbach (2019) and the many references cited therein. I work with the NEPS (2021) Scientific Use File <https://doi.org/10.5157/NEPS:SC6:12.0.0>.

⁴ The fieldwork period for the Adult Cohort typically starts in autumn and lasts into spring of the following calendar year.

task domain, and informs about scales of measurement and aggregation of individual items to standardized scores.⁵

Cognitive tasks are captured by questions on numeracy and literacy requirements, that is scope and level in the usage of mathematics, reading and writing as a regular part of one's job. Interpersonal demands are assessed by questions on communication and frequency of social interaction with people. Questionnaire items on the extent of e.g. standing, walking, heavy lifting and carrying, or assuming strenuous body postures, measure physical activity typically to be exerted at the workplace.⁶ As discussed in Matthes et al. (2014: 279), the routine intensity of tasks is assessed "ex negativo" by asking for everyday job requirements that actually impede and complicate automation and computerization, e.g. whether one frequently has to learn new things, carry out various changing tasks, adapt to unforeseen situations, or solve non-standard problems.

In line with the arguments put forth in Matthes et al., other authors have noted too that asking survey respondents to make a judgement about how "routine" their core job tasks are -in a rather direct attempt to extract information about codifiability and proneness to automation- is not a viable approach (e.g. Autor 2013: 192; see in particular his insights with respect to surveying repetitive tasks in the PDII). Respondents seem to interpret immediate questions on routine intensity as being about how commonplace, standard, and potentially "trite" certain tasks are. This understanding is very different from the notion of routineness that is at the heart of the ALM framework, which is essentially about substitutability of human tasks by technology. So, inverting the measurement and asking about job demands that actually stand in the way of automation and computerization -complex, frequently and unforeseeably changing tasks- may indeed be a more promising approach (see e.g. Haslberger 2022: 182).

Overall, the procedures detailed in Matthes et al. (2014) yield four bipolar scales for cognitive, interpersonal, physical, and routine job requirements. That is, for each domain separately, the task intensity of a job is characterized along on a continuum spanned by two opposite poles: ranging from low to high cognitive, interpersonal, physical demands, and – likewise – from low routine (i.e. nonroutine) to high routine intensity.

3.2 Classification of Occupations

While an individual's occupation is not the key explanatory variable in this paper, occupation-level characteristics and occupational worker sorting are to be controlled for when exploring the separate role of job tasks. Therefore, I need to draw on some common statistical classification scheme.

The NEPS data offer information on occupational affiliation coded according to both, the national classification, as well as the ISCO (International Standard Classification of Occupations, ISCO-08). I draw on the 2010 issue of the German Classification of Occupations (Klassifikation der Berufe, KldB-2010) as the primary source to account for occupational characteristics, as it is

⁵ The original questionnaires (German and English translations) are available as part of the documentation from the NEPS data center (<https://neps-data.de>) upon registration.

⁶ Note, that the NEPS job task items -in line with STAMP- survey physically demanding workplace conditions in a much wider sense than purely manual requirements (i.e. "using hands to do something" or "manual dexterity") as in the original ALM paper.

specially tailored to the national labor market and, equally important, the German educational and apprenticeship system.⁷ At its most detailed level, the KldB-2010 offers a five-digit taxonomy. The first three digits group occupational titles at various levels of structural and definitional detail, in a “horizontal” manner. That is, the grouping is by occupational content, according to similarities in the specific fields of knowledge, goods and services provided, or tools and machines used. The fourth digit depicts, among other things, supervisory and leadership functions. The fifth digit is orthogonal to the first four and references skill level requirements (Anforderungsniveaus) in a “vertical” manner.⁸

The most detailed five-digit level of the KldB, with well over twelve hundred distinct codes, is designed and suitable for official statistics drawing on register data of the entire workforce with millions of observations. Using sampled survey data, however, some aggregation is inevitable. With an eye to the overall size of my sample and associated cell sizes of occupations, I shall stick to the two digit-level throughout this paper. Since the KldB is constructed along the orthogonal dimensions of occupational content and skill level in the way just described, interactions can be formed between the two at any desired, or feasible, level of detail.

As we go along, I will present results of sensitivity checks to a number of alternative choices as regards the occupational coding, both in terms of KldB vs. ISCO, as well as varying levels of detail within each scheme.⁹ Note again that most of the related studies in the field start out at the one-digit level (ISCO, SOC, etc.) and then aggregate further up, to less than a handful of very broad occupational categories (see e.g. Cortes 2016, following Autor/Dorn 2013; Cavaglia/Etheridge 2020). Moreover, these serve as key explanatory variables, assumed to be mirroring all relevant task differences. An assumption that is not borne out by data sources that do offer person-workplace level information on tasks.

3.3 Wages, Human Capital and Demographic Controls

In common with the bulk of the literature, the dependent variable in my regressions is the (log of) workers’ hourly gross wages. The variable is constructed from monthly earnings before taxes and deductions reported by individuals for their main job, as well as actual weekly working hours. Wages are indexed to 2015 euros using the official CPI-deflator of the Federal Statistical Office for Germany (destatis). By convention, I divide the gross monthly earnings figures by the corresponding number of working hours and multiply by 12/52.¹⁰ As a general procedure in my

⁷ Nevertheless, by design, the KldB-2010 has a high degree of compatibility with the ISCO-08 classification.

⁸ While these requirement levels reflect characteristics of tasks and not characteristics of individuals, they are closely aligned with the educational degrees and certificates typically expected to be held by workers. Level 1 tasks typically do not require anything beyond primary education and some short initial on-the-job training. Level 2 tasks call for a formal degree in a recognized apprenticeship program of two or three years duration, often done right after completion of lower and middle secondary school in Germany. Level 3 tasks usually demand a degree of some higher educational institution, e.g. acquired after studying up to 3 years (e.g. bachelor’s degree) at tertiary level, or holding a German “Meister” or “Techniker”. Level 4 tasks mark the highest level and require completed tertiary education with a typical study duration of 4–6 years (master’s degree) or even more (MD, PhD). For further details, see the official handbook of the KldB-2010 (Bundesagentur für Arbeit 2011: 26–28).

⁹ In anticipation, let me already briefly state here that I generally do not find wage returns to job tasks to depend very much on whether and how I control for workers’ occupational affiliation, making any particular choice in terms of the coding scheme quite innocuous in the context of this paper. As I will show, overall, the estimates are much more affected by controlling for individual rather than occupational unobservables.

¹⁰ Contractual working hours at the respective dates of interview were not surveyed in the initial panel wave with a job tasks assessment and are therefore not usable as an alternative.

data preparation, I always use all nonmissing data as reported by respondents; besides some deliberate restrictions on the study population detailed below.

As control variables in the most basic pooled cross-sectional regressions, I enter some human capital and demographic attributes which are standard in earnings equations: to account for individuals' premarket skills and human capital investments I draw on the CASMIN educational classification (Brauns/Scherer/Steinmann 2003) as it integrates information on general (school-leaving) and vocational (e.g. apprenticeship, college and university) qualifications. Individuals' demographic background is captured by indicator variables for being married and foreign born. The educational and demographic variables can be considered time-invariant for all practical purposes. They are entered as regressors in the initial pooled cross-sectional models and jointly absorbed into the individual fixed effects in later panel specifications.

Furthermore, all regressions account for quadratics in respondents' age and job spell tenure, and whether a workplace is located in former East Germany; in addition to time, occupation and individual (spell) fixed effects in the panel specifications.¹¹ Job spell tenure is defined as the time elapsed between the respective date of interview and the moment someone started working in a particular employer-occupation combination. That is, as soon as an individual changes either the employer or the occupation, the tenure variable reset to zero. Note finally that, as the NEPS data track a single cohort of adults, there is no way – but also no need or intention in the context of this paper – to disentangle pure time from age or tenure effects.

3.4 Sample Selection

The estimation sample consists of members of the NEPS Adult Cohort, who participated in the initial wave with a job tasks assessment in 2011/12 and responded to at least one further wave with the tasks questionnaire module rotated in. My sample units are males in their prime working years, who are in regular full-time employment in all sectors except agriculture and the military. I further exclude those, who are doing an apprenticeship or internship, who are temporary or seasonal workers, helping family workers or in public employment and training programs. I also do not include anyone reporting to be self-employed or working as a freelancer.¹² The age range is set from 25 up to at most 50 years at the initial wave interview. This allows to observe workers across various task assignments over time, yet without going into retirement. I thereby follow the studies most closely related to mine, notably Cortes (2016), Ross (2017) and Cavaglia/Etheridge (2020), who all apply very similar restrictions on the age range and study population.¹³

Table A2 displays summary statistics with information on the panel structure of the core estimation sample (Panel A) in combination with some descriptives on wages, highest level of education, demographics and distribution of observations across broad occupational categories

¹¹ Panel regressions in which I fully interacted the four job task measures with the regional dummy do not yield evidence in favor of significantly different task returns in East Germany, allowing me to work with a larger overall (i.e. nationally representative) sample and enhancing statistical power. Also, accounting for region in a more detailed manner by introducing a full set of fixed effects for all sixteen German states and city states (Bundesländer) does not affect the core results in any significant way, and is hence not pursued further. Results are available upon request.

¹² These restrictions on the study population are applied to the sample at the person-year level. Individuals are only dropped from the sample altogether if contributing less than two observations after final data preparation.

¹³ Only the consideration of gender varies a bit across these studies. Ross (2017) uses a pooled sample of males and females, while Cortes (2016) and Cavaglia/Etheridge (2020) each focus on males only; skirting the difficult issue of –credibly– accounting for endogenous selection into the labor market by females. I shall follow the latter two.

(Panel B). The aggregation of occupations shown therein is for expositional reasons only, showcasing the overall sample composition at a comparative level of detail as in the cited reference studies. As explained in Section III, the key regression models account for spell fixed effects derived from the two-digit level KldB-2010 codes, with twenty-eight distinct occupations being populated in each panel wave (see Panel A of Table A2).

4 Descriptive Statistics

I start out by presenting some descriptive evidence on the four task measures and their variation over time, within occupations, and within job spells. Table 1, below, depicts sample averages by survey wave (Columns 1–3), mean changes over the full observation window (Column 4), as well as variability of tasks in terms of standard deviations (Column 5). All statistics in the table are based on the original scores on a 0–1 scale, as described in Matthes et al. (2014).

Table 1: Sample Variation of NEPS Job Task Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Job Task Measures	2011/12 mean	2015/16 mean	2019/20 mean	change in means, 2011/12 - 2019/20	std. dev. pooled sample	share of task variation within occupations	share of task variation within job spells
Cognitive	.633	.631	.626	-.007	.209	.756	.504
Interpersonal	.467	.458	.447	-.020	.202	.706	.434
Physical	.306	.304	.292	-.014	.270	.593	.469
Routine	.387	.400	.412	.025	.172	.865	.429
Observations	944	858	688		2,490		

Note: The table shows variation in NEPS job task measures: means by panel wave, overall variation (std. dev.) and mean change over the full observation period. Depicted are the raw scores on a 0-1 scale as developed in Matthes et al. (2014). The two rightmost columns show the (proportion of) variance in job task scores left after demeaning the data with respect to occupations (KldB-2010 two-digit codes) and job spells (unique individual-occupation combinations).

Source: Text. © IAB

I note only minute overall changes from one wave to another, with no clearly discernable trend over the given period for any single measure. Cognitive, interpersonal, and physical mean task change is signed negatively, and positively for routine. Yet, these differences are always indistinguishable from zero, statistically, with the respective standard deviation being larger by multiple orders of magnitude for each measure.¹⁴

Given the focus of this paper, and the comparative advantage of the NEPS data (which is not long-term trend analysis), a more relevant examination concerns the extent of task variation present within occupations. For this purpose, I regress out any fixed occupation effects from each one of the four job task measures and then divide the variance of the (respective) residual scores by their overall variance. Very much in line prior cross-sectional studies on job tasks, also the

¹⁴ The wave-specific standard deviations for each task domain are virtually identical to their pooled counterparts, which is why I do not report them separately.

NEPS data evidence a sizeable share of within-occupation variation (Column 6); with some slight differences by domain.¹⁵ Of course, the exact magnitudes always hinge on the kind and level of detail of the occupational categorization. The more (less) aggregated the selected coding scheme, the more (less) variation there is within occupations, by construction. In that regard, the KldB two-digit level used throughout this paper covers a middle ground in terms of detail.¹⁶

As a final step, column 7 considers the share of residual task variation present within spells, that is, after having netted out all characteristics of individuals, occupations, and their unique combinations. Even after applying this data transformation, a substantial share of the initial job task variation is still remaining: about half, or somewhat less than half, depending on the domain. This implies that not only do tasks vary substantially across individuals holding the same occupation, but also for a given individual in a given occupation: that is, over time, within the same spell. Both types of “within” variation of tasks are usually not considered and assumed away. Columns 6 and 7 clearly underscore AH’s point that we miss a nonnegligible portion of task change, if we do not extend the scope of our empirical analyses to include the person-job level.

In the next table, Table 2, I look at the way job tasks are interrelated with one another. Starting with the full cross-sectional variation in Panel A, we note a certain co-occurrence of interpersonal and cognitive task domains, and negative associations of these two with routineness.¹⁷ Physical tasks in turn correlate negatively with both, cognitive and interpersonal tasks, albeit much weaker so with the latter. At the same time, physical tasks are slightly positively correlated with routine job tasks. These association patterns fit well with what AH have found using their cross-sectional job tasks data (ibid.: S79, Table 3). To a degree, however, they reflect differences in the typical (average) task contents of the occupations held by workers.

In order to consider intercorrelations of job tasks with such occupation-level effects removed, Panel B recalculates all associations among job task measures drawing on their respective within-occupation variation only. Compared to Panel A, we see a general weakening of all cross-correlations, which is exactly what one would expect to happen if tasks are systematically bundled together at the occupation level. Yet, all associations of at least moderate strength before (say, $>|.3|$) do persist and are also found again within occupations.

To complete the discussion of Table 2, Panel C describes how the various job task domains co-develop over time (the last decade), again, net of any occupation effects. Note, by considering the intercorrelations of first-differenced versions of the job task measures, any individual worker heterogeneity is kept constant, too. Let us first turn to change over time in jobs’ routine intensity and how this relates to changes in the other domains, as this has received by far the most attention in the literature.

¹⁵ Physical tasks exhibit a somewhat higher level of between occupation variation.

¹⁶ When using the most fine-grained five-digit KldB-2010 or four-digit ISCO-08 classifications provided in the NEPS data, about half of the variation in job tasks is still left unexplained by occupation indicators (318 and 189 distinct codes in my sample, respectively), again with some variation by domain, and well in line with the magnitudes reported by AH. Detailed results available upon request. So, the general conclusion that there exists a non-negligible amount of within-occupation task variation clearly holds through. As argued in the data section, the full five-digit KldB-2010 or four-digit ISCO-08 classifications are too detailed to serve as reference frames in the given sample, with quite many occupational categories containing less than a handful of observations, not seldom only a singleton per code.

¹⁷ The correlation between cognitive and interpersonal tasks is clearly positive (+.369) but far from perfect (1.00), supporting the view that these can be considered distinct activities, or requirements. Not seldom, these two domains are amalgamated to a single measure for the “abstractness” of tasks (cf. AH, in particular S91-92; and Ross 2017).

Table 2: Intercorrelations of Job Task Measures

	Cognitive	Interpersonal	Physical	Routine
A. (pooled) Cross-sectional				
Cognitive	1.000			
Interpersonal	+.369	1.000		
Physical	–.354	–.079	1.000	
Routine	–.507	–.357	+.174	1.000
B. (pooled) Cross sectional, occupation-level effects removed				
Cognitive	1.000			
Interpersonal	+.279	1.000		
Physical	–.193	+.066	1.000	
Routine	–.438	–.338	+.005	1.000
C. First-differenced, occupatio-level effects removed				
Cognitive	1.000			
Interpersonal	+.126	1.000		
Physical	–.021	+.052	1.000	
Routine	–.181	–.200	–.073	1.000

Note: All correlations are based on the full sample of 2,490 observations as summarized in Table A2.

Source: Text. © IAB

The bottom row of Panel C shows that declining routine task requirements go along with increasing cognitive and interpersonal requirements (the correlations in FDs being moderately strong with –.181 and –.200, respectively). The fact that these correlations are far from perfect, however, indicates that there is a lot of heterogeneity present in the way jobs (within the same occupation, and for statistically identical workers) change in terms of their task composition. Still, overall, they are commensurate with RBTC explanations of task change, suggesting that the kind of shifts typically found between occupations (i.e. away from routine and towards nonroutine-cognitive/interpersonal) also work within occupations. Finally, the correlation between first-differenced routine and physical task measures is signed positively but appears a bit weak ($<|.1|$) to draw any clear conclusions. As far as the interrelations among changes over time along other domains are concerned, there is some weak evidence pointing towards increasing complementarity between cognitive and interpersonal job task contents (correl. in FDs about +.13). This is a phenomenon that has been noted earlier in terms of skills, as opposed to tasks, by Weinberger (2014).

5 Job Tasks and Wages

Turning to the analysis of job tasks and wages, displayed in Table 3 below, I pick up the thread where AH left off and begin by estimating pooled cross-section wage regressions in columns 1 and 2. I control for a similar standard variety of human capital and demographic covariates as they do: a set of indicators for workers' highest level of (vocational) education attained, foreign

born and marital status. I also always include quadratics in age and job spell tenure.¹⁸ Column 2 additionally accounts for occupation-specific effects. Column 3 is the first regression model exploiting the individual-level panel structure of my data, thereby absorbing the vector of (practically time-invariant) human capital and demographic controls into the set of worker-fixed effects. Column 4 extends the model further by time-demeaning the data within each unique worker-occupation spell, as explained above. All specifications include time fixed effects and a dummy variable for an individual's current workplace being located in former East Germany. Each column of Table 3 reports estimation results with (log) real hourly wages as the outcome variable and person-year as the unit of observation; standard errors being clustered at the individual level throughout. The explanatory job task variables are normalized to have mean zero and standard deviation one.

Table 3: Wage Returns to Cognitive, Interpersonal, Physical and Routine Job Tasks

Outcome is (log) real hourly wages	(1)	(2)	(3)	(4)
Cognitive	+0.080***	+0.083***	-.001	-.005
[s.e.]	[.012]	[.012]	[.010]	[.010]
Interpersonal	-.013	+0.007	-.000	-.003
[s.e.]	[.009]	[.010]	[.008]	[.009]
Physical	-.106***	-.090***	-.034***	-.027**
[s.e.]	[.010]	[.011]	[.011]	[.012]
Routine	-.053***	-.033***	-.024***	-.025***
[s.e.]	[.009]	[.009]	[.007]	[.007]
Time fixed effects	X	X	X	X
Human capital and demographic controls	X	X		
Occupation fixed effects		X	X	
Individual fixed effects			X	
Individual-occupation spell fixed effects				X
Observations	2,490	2,490	2,490	2,235

Note: The data source is the Adult Cohort of the German NEPS covering the pooled waves 2011/12, 2015/16 and 2019/20 with workplace task measures. The analysis sample is restricted to full-time working males with nonmissing information. Each column reports estimation results with (log) real hourly wages as the outcome variable and person-year as the unit of observation. The cognitive, interpersonal, manual and routine scores are normalized to have mean zero and a standard deviation of one. Occupation codes are in accordance with the 2010 issue of the German Classification of Occupations (Klassifikation der Berufe, KldB-2010) at the two-digit level. Standard errors are reported in brackets and clustered at the individual level.

***p<.01, **p<.05, *p<.10

Source: Text. © IAB

Starting with the cross-sectional model shown in column 1, we see a statistically significant positive coefficient for cognitive job tasks, and significant negative coefficients for physical and routine tasks. The magnitudes are quite sizeable, with predicted wage premia and penalties of about +8, -11 and -5 log points due to a one standard deviation change in the respective task scale. Adding occupation fixed effects in column 2 reduces the estimates obtained for physical

¹⁸ Note, that for all members of my sample age grows collinearly with general experience (age - # years of education - 6), by survey design.

and routine tasks somewhat, about 2 log points each. Interpersonal tasks do not show up significantly. Although an immediate comparison with AH's cross-sectional estimates is impeded by differences in task measures, samples, other survey design features, countries and time point(s) considered, the overall patterns and magnitudes are surprisingly similar (cf. AH: S82, Table 5, in particular columns 4 and 7).¹⁹

Column 3 is the first specification that goes beyond AH and estimates task returns based on individual-level fixed effects regressions. The large and significant coefficient for cognitive tasks found before is driven down to zero. Likewise, we see a large reduction in the negative coefficient for physical tasks by almost 6 log points. These changes in estimates are consistent with (strong) worker sorting based on unobservable ability differences. The estimate obtained for routine tasks is reduced by only about 1 log point and interpersonal job tasks again do not show up significantly.

Column 4 shows estimation results for the most comprehensive spell fixed effects specification. This step reduces the coefficient for physical tasks somewhat further, while routine tasks' wage impact remains virtually identical at about -2.5 log points (for one standard deviation increase in the score).

Appendix Table A3 offers a number of sensitivity checks to alternative ways of occupational coding, by using finer levels of granularity (no. of digits) and different classification schemes (German KldB vs. ISCO). The overall conclusion from these checks is that my particular choice is rather innocuous and not the main driver behind the results reported above. Any other way of accounting for occupation yields almost identical estimates. With an eye to sample size I prefer to stick to the more aggregate KldB-2010 two-digit level throughout the paper.

Appendix Table A4 presents some further sensitivity checks with respect to outliers (individual wages, occupation size), sample definition, and the inclusion of time-varying occupation fixed effects; all estimates being highly comparable to those obtained from the reference specification in Table 3, Column 4.

As an interim conclusion, I note that both physical and routine tasks happen to receive significant wage penalties; net of workers' occupational affiliation, their inherent unobservable productive attributes, and any occupation-specificity these unobservables may have. In contrast, the wage premia for cognitive tasks previously found in cross-sectional specifications can be explained entirely by ability-related sorting. This precisely confirms the concern AH articulated with respect to their own cross-sectional estimates, but were unable to address given the data available to them.

¹⁹ The estimates do not change by much when conditioning on occupation at the more detailed three-digit level, as in AH (using the German KldB in my case).

6 Wage Effects of Routine Intensity by a Job's Other Task Requirements

As briefly noted in the data section on measurement, yet worth highlighting again here, the NEPS surveys cognitive, interpersonal and physical job task requirements as separate domains and without mention of the “routineness” concept in the underlying item formulations. Analogously, routine intensity of job tasks is assessed without reference to the other domains. By example, there is no attempt to qualify whether a certain level of reported routine intensity represents, say, cognitive-routine rather than manual-routine. From the data we only know whether routine intense tasks are exercised in a job environment that is at the same time cognitively demanding (or not), physically demanding (or not), and so forth. In a way, the questionnaire design takes the ALM framework at face value: it is “pure” routineness of human job tasks -their susceptibility to automation and substitution by machines and algorithms through codifiability- that is considered most relevant and to have all kinds of implications for wages and the division of labor.²⁰

Commensurate with this view and survey implementation, it appears natural to start out with wage regression models that consider the various task domains side by side; that is, holding the respective other domain(s) fixed. When re-estimating the most comprehensive reference specification (Column 4 of Table 3) repeatedly, yet each time entering only a single task measure, I obtain virtually identical parameter estimates. So, in terms of its estimated average wage effect, routineness indeed appears to be operating largely orthogonal to the other domains considered. While the ALM framework assumes a rather agnostic stance with regard to whether routineness manifests itself in form of –or in conjunction with– cognitive, interpersonal, or physical tasks, it nevertheless has to emerge in some way. It therefore appears worthwhile to examine in some more detail whether the primary and persistent negative routineness effect is moderated by a job's typical other requirements. By unmasking some of the underlying effect heterogeneities connected to the focal routine task domain, we can potentially identify job task profiles that are particularly (un)favorable from a worker's perspective.

More concretely, I take the job spell fixed effects specification from Table 3 as my point of departure and ask whether (within-job spell) changes in routine task intensity have a differential wage impact depending on whether a worker's current spell ranks low, intermediate, or high in terms of its' other task requirements. I take a pragmatic approach and split spells into three tertiles, ranking from low, intermediate to high in terms of cognitive, interpersonal, and physical requirements, consecutively. These categorical indicators are then interacted with the routine task scores, as defined before.²¹ Estimation results are depicted in Table 4.

²⁰ As Autor (2013: 186) writes, it is all about the boundary between “labor” and “capital” tasks, and which happens to shift over time depending on changes in technological feasibility and economic cost (of automation). Autor/Dorn (2013), for example, also construct a “pure” routineness index, purged of any abstract and/or manual occupation task requirements, and put it at the center of their empirical analysis.

²¹ Using finer categories (quartiles, quintiles) or using fully continuous interactions (linear, squared, cubed, etc.) does not unearth any reliable additional insights; and asks too much of the data, in my view. The available sample size and panel variation simply does not lend itself well to go further into the task distributions' tails or identify complex nonlinear patterns.

Table 4: Wage Effects of Routine Task Intensity by Job's Other Task Requirements

Outcome is (log) real hourly wages				
A. by level of cognitive task intensity				
	low	intermediate	high	
Routine	-.028**	-.029**	-.015	0.47 F(high-low)
[s.e.]	[.013]	[.012]	[.013]	.492 p-value
B. by level of interpersonal task intensity				
	low	intermediate	high	
Routine	-.027**	-.020*	-.033**	0.11 F(high-low)
[s.e.]	[.013]	[.012]	[.014]	.736 p-value
C. by level of physical task requirements				
	low	intermediate	high	
Routine	-.008	-.017	-.048***	4.42 F(high-low)
[s.e.]	[.012]	[.012]	[.014]	.036 p-value

Note: Model specification and analysis sample are the same as in the most comprehensive specification of Table 3 (Column 4). Job spells are classified into tertiles according to their level of cognitive, interpersonal and physical task requirements. The resulting categorical variables are then fully interacted, consecutively, with the standardized routine task measure as used before. Standard errors are reported in brackets and clustered at the individual level.

***p<.01, **p<.05, *p<.10

Source: Text. © IAB

Panel A considers heterogeneous routineness effects by level of cognitive task intensity. I find that the negative wage effect of routine task intensity is concentrated in job spells that rank low to intermediate in terms of their cognitive requirements. Yet, all three estimated coefficients are signed negatively and the difference in coefficients between the highest and lowest tertile is not statistically significant, as the F-test in the rightmost column shows.

Panel B suggests that the level of interpersonal task requirements on the job is not a strong moderator of the negative routineness effect, albeit some slight variation in coefficients across tertiles. The difference between any two categories never exceeds one standard error in magnitude, and never more than half a standard error when comparing each individual coefficient to the estimated overall average effect from the reference specification.

This stands in stark contrast to the findings for the level of physical task requirements, displayed in Panel C. The wage penalty for routine intense tasks appears to be confined to workplace contexts that are at the same time physically demanding. The point estimate of about –5 log points for jobs ranking in the highest tertile of physical task intensity is roughly twice as large as the overall average effect. An equal change in (within-job spell) routineness does not have any significant negative effect when unfolding in workplace contexts that are physically less demanding.

In sum, routineness may be considered a characteristic of human job tasks that cuts through other domains and is affecting wages independently, yet its effect strength is clearly moderated by the kind and level of other tasks required concurrently.²² High cognitive demands appear to

²² Table 4 considers levels of cognitive, interpersonal and physical task requirements consecutively. One could also think of evaluating the routineness effect at various combinations of these other domains, that is in a multidimensional way. Absent any

shield workers against the adverse effects of routineness, while these adverse effects are particularly pronounced in jobs where physical task requirements are also high.

To complete the discussion, I have also examined effect heterogeneities when interacting tasks in a domain-by-domain way (see Appendix Table A5). That is, I ask how the wage effect of any particular domain varies over the tertiles of its own intensity level distribution. By example, a (potential) payoff accruing to cognitive tasks may be different depending on whether a job rates low, intermediate, or high in terms of its cognitive requirements. Repeating this for each domain, separately, and always holding the other domains constant in the background, the following results emerge: First, I do not detect any important nonlinearities in cognitive or interpersonal tasks that might have been masked by the linear panel specification.²³ Second, the negative average effect of physical tasks on wages found before, is driven by jobs with high physical requirements, *ceteris paribus*. Third, in contrast, the negative average wage effect of routineness emerges persistently across all tertiles of the routine task intensity distribution, and in about the same strength. I therefore tentatively conclude that the protection against, or amplification of, routineness' adverse effects on wages is related to the (bundle of) other tasks that workers are required to exercise; perhaps more so than to the level of routine intensity itself.

7 Robustness Checks

The chosen spell fixed effects approach exploits time-variation within unique individual-occupation combinations. Together with an array of additional fixed effects and controls, this removes a host of influencing factors typically not accounted for by prior studies on job tasks. Still, in order to interpret the estimated coefficients as “selection-free” wage premia and penalties, one must be willing to assume the absence of any other nonrandom error components related to both tasks and wages. In light of the non-experimental nature of the approach, it is a necessary assumption that it shares with all other recent panel-data based studies employing spell fixed effects estimation, notably Cortes (2016) and Cavaglia/Etheridge (2020).

What appears possible, beyond that, is to think more concretely about margins of heterogeneity and intermediate selection steps that have been omitted so far, and arguably pose a relevant concern. One potential candidate that may cause spurious correlation between tasks and wages –even when holding the individual-occupation spell fixed– is employer heterogeneity, and the related fact that there is nonrandom assignment of individuals to employers. In contrast to the issue of occupational sorting and mobility (e.g. workers deselecting routine occupations) this concern has received only scant attention in the empirical task literature. However, workers may well react to changing task demands and returns by a change of employer. It may even be their first line of adjustment, before switching the occupation. The mechanism is entirely analogous to that discussed in terms of occupational self-selection. Individuals choose (and are being chosen by) employers based on where their particular set of abilities is expected to be most rewarding (or productive). By extending the spell definition accordingly, that is, to unique individual-

clear theoretical predictions to test, however, searching across all sorts of task profiles would involve a fair amount of data mining in which I would not like to engage.

²³ The rightmost column of Table A5 displays test statistics and p-values, based on the corresponding F-test for each domain.

occupation-employer combinations, the spell fixed effects approach can be readily adapted. Defining job spells in this manner also sweeps any employer heterogeneity per se, e.g. differences in pay policies (“high wage firms” vs. “low wage firms”), contents of collective agreements, and many other factors.

I am therefore able to control for unobserved employer characteristics (and their interaction with individual unobservables) without having explicitly available the numerical identifiers, such as employer IDs, which is rarely possible. Adding employer fixed effects to panel wage regressions is usually not an option if based on survey data samples. At best, a few observable characteristics (e.g. industry, number of employees) are available and linked to the person-level responses. However, since the NEPS departs from a “pure” panel study when it comes to the collection of employment information from its respondents, it allows one to infer employer changes of workers.

Employment is surveyed by asking for coherent episodes, with begin and end date, and not as a status variable (applying at the interview date). By design, any reported episode refers to an uninterrupted period of time that an individual works for the same employer, within the same occupation. If either changes, the employer or the occupational affiliation, a new episode is being recorded. This questionnaire design and episode data structure implies, conversely: whenever an occupation code remains unchanged between two adjacent employment episodes of the same individual, then, an employer change must have occurred. This procedure allows me to modify and extend the spell definition to unique individual-occupation-employer combinations. It thereby accounts for employer-specific returns to individual unobservables and also removes any fixed employer effects (in one go) in the regression analyses, as discussed.

Table 5: Robustness of Estimates w.r.t. Employer Characteristics and Changes

	Cognitive	Interpersonal	Physical	Routine
(i) Adding "firm type" fixed effects (firm size X industry cells)				
(n=2,235)	-.002	-.005	-.025**	-.024***
	[.011]	[.009]	[.012]	[.008]
(ii) Using only within-individual-occupation-employer spell variation in job tasks				
(n=2,100)	+.001	-.001	-.024**	-.020***
	[.011]	[.009]	[.012]	[.008]

Note: Model specification and analysis sample are the same as in the most comprehensive specification of Table 3 (Column 4). Firm types are defined by the full interaction of categorical firm size and industry variables. I aggregate the detailed two-digit industry codes available in the data (according to the German Klassifikation der Wirtschaftszweige, WZ 2008) into broader aggregates (more in line with ISIC/NACE) and distinguish the following 9 categories: (1) Mining, electricity, gas, steam, water, (2) Manufacturing, (3) Construction, (4) Wholesale, retail, transportation, storage, (5) Information and communication, (6) Financial and insurance activities, (7) Professional, scientific, technical services, (8) Public administration, education, etc., (9) Other service activities. The firm size variable distinguishes 5 categories: (a) less than 5 employees, (b) 5 to less than 20, (c) 20 to less than 200, (d) 200 to less than 2.000, (e) more than 2.000 employees. Since one cell is empty, I am able to control for 44 distinct firm types. For completeness, note, that I allow the wage impact of each firm type to differ by time period. Standard errors are reported in brackets and clustered at the individual level.

***p<.01, **p<.05, *p<.10

Source: Text. © IAB

Table 5 displays the estimates obtained when employer characteristics and employer related self-selection of workers are additionally controlled. As a first take, for comparison, observable

employer characteristics are used to generate a vector of indicator variables capturing “firm types”, an interaction of categorical firm size information and industry codes. This would usually be the only way forward with survey panel data in terms of accounting for employer heterogeneity in productivity, pay policies, etc., as discussed above. Row (i) of Table 5 shows that the estimated coefficients on physical and routine task intensity are only minimally affected when compared to the reference specification in Table 3 (Column 4). In row (ii) of Table 5 the estimates obtained from a spell fixed effects model using the augmented spell definition, including the employer dimension, are presented. Compared to row (i), only the estimated coefficient on the routine intensity domain shows some sensitivity (change from $-.024$ to $-.020$). Qualitatively, this confirms that employers’ characteristics, and systematic worker sorting among them, may indeed be explaining part of the relation between job tasks and wages, as suspected. Quantitatively, they do not appear to be dominant (here).

8 Concluding Remarks

This paper presents empirical evidence that measures of job-level task content have a significant, independent, impact on individual wages. I base my analyses on newly available panel data for Germany that allows me to apply individual-occupation(-employer) spell fixed effects estimation in this context, for the first time. My findings suggest that RBTC explanations of occupational task change and economic outcomes also hold within occupations. For example, a persistent negative wage effect of routine task intensity –in the sense of increasing substitutability of human tasks through machines and algorithms– is present at the job-level, too. What also emerges is that this negative wage impact is moderated by what other tasks workers are required to perform concurrently. Complex cognitive activities lend protection, while manual or physical tasks exacerbate the wage penalty. By controlling for unobserved worker, occupation, employer and match-specific heterogeneity, I find selection issues to play a major role, and to explain large parts of the cross-sectional association between job tasks and wages documented in previous studies; most prominently by AH. Still, despite the sizeable reductions in the point estimates of the average wage returns to several task domains, I consider my results supportive of AH’s general conclusion: intra-occupational variation at the job-level is an important component that warrants separate consideration, complementary to the task change happening between occupations. This holds even more so, as individuals do not, or cannot, always easily leave their occupation of origin. While occupational mobility has received a lot of attention in the literature, staying is the default state for many, and examining how task change affects those who (are confined to) stay is equally relevant.

A final remark relates to the ease with which occupations are often equated to tasks, and only so. There is usually not much discussion about what occupations are, and why they should matter for individual economic outcomes, besides the idea that they reflect characteristic task bundles. As pointed out earlier by Bittarello/Kramarz/Maitre (2018: 1), an occupation is not a well-defined economic concept. Workers’ job tasks are something else than their occupation and, conversely, occupations are more than characteristic task bundles (alone). A possible alternative, or complementary, view on occupations is that of labor market institutions. Certificates and

occupational licensing are cases in point (e.g. Kleiner/Soltas 2023; Kleiner/Krueger 2013). More generally, occupational groups may create boards, chambers and associations, that counsel and influence the government on norms and regulations, working hours and conditions, (exemptions from) minimum wages, and so forth; some more so than others. These few examples illustrate that occupations may (differentially) impact on wages and employment of the individuals affiliated with them, beyond tasks. Using aggregate occupation codes as proxies for tasks, and interpreting the estimated occupation fixed-effects as task prices, therefore not only discards the within-occupation heterogeneity. It picks up these distinct influence factors and attributes them to tasks. Drawing on job-level task measures may therefore offer a valuable alternative that closely aligns the entities on which the economic theory is based (i.e. the work activities) and their measurement. That said, continued data collection efforts covering additional countries, spanning longer time periods, generating larger samples, hold great potential for future empirical research adopting a task-based approach.

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Appendix

Table A1: NEPS Tasks Item List with Question Texts, by Domain

"In the upcoming questions we will ask you about things you have to do regularly as part of your job as <insert>. Please do not refer to things you have done, or have been trained for in the past, but only to things you are presently doing."

Numeracy

"As part of your job, do you

...use mathematics or deal with numbers in any way?

...work at a cash desk or with cash in another way?

...measure or count things?

...do simple calculations such as addition, subtraction, multiplication, or division?

...do calculations that involve fractions or percentages?

...use mathematics to calculate areas, circles, or volumes?

...use advanced mathematics, such as calculus, or probability and statistics, such as regression analyses?"

Literacy

"As part of your job, do you read

...anything at work, even very short notes or instructions?

...texts at least one page long?

...texts at least five pages long?

...texts at least twenty-five pages long?"

"As part of your job, do you write

...anything at work, even very short notes or instructions?

...texts at least one page long?

...texts at least five pages long?

...texts at least twenty-five pages long?"

Interpersonal

"As part of your job, how often do you

...have contact with people other than colleagues or coworkers, for example with customers, clients, students, or the general public?

...give people simple information?

...give people advice, for example in financial or legal matters?

...counsel people and help them with their personal problems?

...teach or train people?

...interview people, for example people applying for loans, government benefits, jobs, or other things?"

Physical

"As part of your job, how often do you

...stand for at least two hours?

...walk, run, or cycle longer distances?

...lift or carry a load of at least 10 kg?

...get into an uncomfortable body posture, for example in a kneeling, squatting, crawling or stooping position?

...work while being exposed to great heat or cold?"

"In the upcoming questions we will ask you about things you have to do regularly as part of your job as <insert>. Please do not refer to things you have done, or have been trained for in the past, but only to things you are presently doing."

Routine

"As part of your job, how often do you

...have to find solutions to problems that cannot be solved easily or immediately?

...have to learn something new (to be able to carry out your work)?

...get tasks that require some familiarization first?

...have to react to unanticipated situations?

...have to change the tasks you are required to carry out?

...have to do things which you have not done before?"

Note: Numeracy and literacy items are framed as "yes" or "no" questions, and aggregated up as outlined in Matthes et al. (2014, appendix A.2). Responses to items belonging to the interpersonal, physical and routine domains are recorded along a five-point scale ranging from "always/very often", "often", "sometimes", "seldom", to "very seldom/never" and also aggregated up to an overall score per domain, each ranging from zero to one (ibid., appendix A.3, A.4, and A.5). Table 1 shows sample means for these original scores by survey year, and some information on variability; overall, within and between occupations and individuals. In my empirical analyses, I prefer to work with standardized scores (normalizing each of the original scores to have mean zero and standard deviation one) in order to obtain a consistent interpretation of the relative wage impact of the various task measures in terms of a "one standard deviation change" in respective job task content.

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Table A2: Summary Statistics

A. Panel Structure		
(i)	Number of observations	2,490
	Proportions by participation pattern across panel waves:	
	"111" (fully balanced)	.725
	"1.1"	.069
	"11."	.206
(ii)	Number of individuals	944
(iii)	Number of occupations (KldB-2010 two-digit codes)	28
	Mean # of obs. per occupation code	89
	Median # of obs. per occupation code	65
	Min–Max	8–243
(iv)	Number of job spells / unique individual-occupation combinations	1,128
B. Sample Descriptives (Means and proportions)		
(v)	Mean log hourly real wages	3.16
	Std. dev.	.447
(vi)	Highest level of education	
	≤ Lower secondary schooling + apprenticeship	.182
	Upper secondary schooling + apprenticeship	.439
	College, university	.379
(vii)	Mean age (in years)	45.3
	Std. dev.	7.8
	Min–Max	25–58
(viii)	Foreign born	.060
(ix)	Married	.754
(x)	Mean job spell tenure (in years)	11.3
	Std. dev.	8.8
	Min–Max	0–39
(xi)	Occupational categories (aggregated)	
	Manual professions	.243
	Services	.086
	Commercial and Administrative	.215
	Technicians and Engineers	.188
	Professionals and Managers	.269
(xii)	Workplace located in (former) East Germany	.158

Note: The table summarizes the panel structure in terms of individuals and occupations, and provides sample means/proportions for the dependent variable as well as some selected human capital and basic demographic variables. The aggregated occupational categories are based on Blossfeld's Occupational Classification as provided in the NEPS data Files. On the original conception, see Blossfeld (1985: 68), and on updating it to the KldB-2010 see Schimpl-Neimanns (2020).

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Table A3: Sensitivity of Estimates to Alternative Ways of Occupational Coding

Outcome is (log) real hourly wages	(1)	(2)	(3)	(4)	(5)
Cognitive	-.007	-.009	-.010	-.011	-.008
[s.e.]	[.011]	[.010]	[.011]	[.010]	[.010]
Interpersonal	-.000	+.002	+.003	-.000	+.003
[s.e.]	[.009]	[.009]	[.009]	[.009]	[.009]
Physical	-.026**	-.027**	-.025*(*)	-.025**	-.026**
[s.e.]	[.012]	[.012]	[.013]	[.012]	[.013]
Routine	-.025***	-.026***	-.024***	-.025***	-.023***
[s.e.]	[.008]	[.007]	[.008]	[.007]	[.007]
Individual-occupation spell fixed effects based on:					
KldB-2010: two-digit + fifth	X				
KldB-2010: three-digit + fifth		X			
KldB-2010: five-digit			X		
ISCO-08: three-digit				X	
ISCO-08: four-digit					X
Time fixed effects	X	X	X	X	X
Observations	2,182	2,157	2,126	2,169	2,143

Note: Data and analysis sample are as described in Section 3. Each column uses the maximum number of observations, excluding singletons. Naturally, the higher the level of occupational detail accounted for, the more singleton observations dropped and the smaller the respective estimation sample. Occupation codes are in accordance with the 2010 issue of the German Classification of Occupations (Klassifikation der Berufe, KldB-2010) or the 2008 issue of the International Standard Classifications of Occupations (ISCO-08), at varying levels of granularity. In addition to controlling for time effects and individual-occupation spell fixed effects, all models include quadratics in age and job spell tenure, and an indicator for whether the current workplace is located in (former) East Germany. Standard errors are reported in brackets and clustered at the individual level.

***p<.01, **p<.05, *p<.10

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Table A4: Further Sensitivity Checks of Job Task Return Estimates

	Cognitive	Interpersonal	Physical	Routine
(i) Deleting wage outliers (top and bottom 1%)				
(n=2,199)	+0.001	+0.001	-.023**	-.024***
	[.010]	[.008]	[.011]	[.007]
(ii) Deleting small occupations (with less than 20 observations)				
(n=2,175)	-.003	-.000	-.026**	-.024***
	[.011]	[.009]	[.012]	[.008]
(iii) Forcing sample to be fully balanced				
(n=1,671)	-.001	-.004	-.026**	-.023***
	[.011]	[.009]	[.012]	[.008]
(iv) Controlling for time-varying occupation fixed effects				
(n=2,235)	-.004	-.005	-.024**	-.022***
	[.010]	[.009]	[.012]	[.007]

Note: Model specification and analysis sample are the same as in the most comprehensive specification of Table 3 (Column 4). Standard errors are reported in brackets and clustered at the individual level.

***p<.01, **p<.05, *p<.10

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Table A5: Heterogeneous Job Task Effects: domain × domain

Outcome is (log) real hourly wages				
	low	intermediate	high	
Cognitive	+0.011	-.023	-.009	0.69 F(high vs. low)
[s.e.]	[.017]	[.019]	[.018]	.406 p-value
Interpersonal	+0.003	-.019	+0.013	0.14 F(high vs. low)
[s.e.]	[.018]	[.013]	[.017]	.707 p-value
Physical	-.007	-.020	-.041**	0.93 F(high vs. low)
[s.e.]	[.031]	[.019]	[.017]	.336 p-value
Routine	-.024*	-.024**	-.028**	0.05 F(high vs. low)
[s.e.]	[.014]	[.012]	[.013]	.823 p-value

Note: Model specification and analysis sample are the same as in the most comprehensive specification of Table 3 (Column 4). Job spells are classified into tertiles according to their level of cognitive, interpersonal, physical and routine task requirements. The resulting categorical variables are then fully interacted, domain by domain, with the respective standardized task measure as used before. Standard errors are reported in brackets and clustered at the individual level.

***p<.01, **p<.05, *p<.10

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