

Warszawa, May 15th, 2019

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**Report on a PhD Thesis presented by Paweł Chrostek
entitled „Organisation of labour and technological change”**

Mr Paweł Chrostek presented a PhD thesis on the links between earnings inequality in the context of collective bargaining (the first essay) and team work (the second essay). These two essays contain theoretical models and some preliminary empirical applications in the context of technological change. The formalization of intuitions provides several novel theoretical insights on the mechanisms behind earnings inequality. Thus, the Author contributes to the growing body of literature on links between technological change and earnings inequality.

The PhD thesis of Mr. Chrostek consists of two main chapters (the two essays), as well as short introduction and conclusions. Each theoretical model is complemented with a comprehensive set of proofs relegated to the appendix for the ease of following the main narrative. The proofs are well organized, clear and – as far as I could tell – correct. They are also instrumental in developing the new insights that Mr. Chrostek proposed in his models. Both theoretical models have been provided with a preliminary attempt to seek empirical verification of the formulated theoretical predictions. In the case of the collective bargaining (the first essay) the verification is based on observational data from European economies and the United States. In the case of the team work (the second essay), the Author presented some simulations.

My overall evaluation of the thesis is positive. On the one hand, the two presented models are indeed novel. The theoretical modeling is not only rare among Polish PhD candidates – it is also seldom actually novel. In the case of the thesis in question the theoretical modeling is actually novel, which is commendable. Indeed, Mr Chrostek has demonstrated high level of

skill and ability in deriving the formalized proofs of the intuitions behind his theoretical models. Indeed, the formal part of the thesis is its strong point, with an impressive collection of high quality proofs.

On the other hand, there are only two essays and if I were to judge both essays by the criteria of journal submission, I would deem them to be rather early stage as papers. The theoretical ideas are fully developed but the narrative and taking the models to the data leaves room for improvement. While I commend Mr Chrostek for writing in English, the style, the narrative and the clarity of his writing leave room for improvement as well. Also, certain extent of boldness is admirable in science, but may become arrogance if in excess. I am not convinced that Mr. Chrostek stroke a balance here. Apart from language and narrative though, I have several criticisms towards the “translation” of the theoretical modeling choices to words (or intuitions that are used to either justify the setup of interpret its findings). My greatest concerns relate to the empirical applications. As I know other works of Mr Chrostek, I was surprised to find work of such early stage and multiple caveats presented as a completed Phd thesis. Below, I organize my comments on the two presented studies.

Study 1.

Motivation. The Author begins the narrative by stating that unionization is on decline, but fails to report the empirical evidence on the existing explanations as well as potential weaknesses of these explanations. Most notably, one would like to know how much of the observed decline is already explained by the current state-of-the-art and how much is still left to be explained. When the Author refers to earlier literature, theory and empirics are mixed together, without clearly delineating them from one another. When referring to empirics, it would also be advisable to delineate studies with causal identification from the studies relying on pure correlation. I was a bit confused about the salient mix between current published frontier papers and 16 years old policy papers or published papers that predate any of the processes analyzed in the study (e.g. early 1980s). The impression build by the narrative appears to serve the purpose of justifying the fact that the Author chose to study something that is not completely obscure – rather than explaining the actual contribution of the proposed channel of influence. It goes without saying that picking a random 30 years old empirical paper with pure correlations is a bad justification for studying the topic that did not exist at the time. Meanwhile, there is a lot of literature that the Author should have mentioned, which is (a) more recent; (b) better identified; (c) more comprehensive; and (d) often contradicts the papers chosen by the Author. The findings of Baldwin’s WP from 2003 and Abowd and Lemieux from 1993 (!) have been rebuked a zillion times with stronger identification and data (e.g. Helpman et al, 2017, REStud and references therein). The growing literature on the firm

size and size wage premium as well as bargaining related to firm size patterns is highly relevant here as it provides explanations that affect both trends in unionization and trends in wage inequality, but one does not mediate the other in the context of technological change (Card et al, 2015,QJE; Bloom et al, 2018, AEA P&P; Berlingieri et al, 2018, AEA P&P). Hence, the empirical motivation for studying the direct links unionization \leftrightarrow technology \leftrightarrow inequality is not very strong, given prior findings. By this statement I do not mean to say that it cannot be strengthened by more thorough review of the existing literature in the field and impartial presentation of the empirical relevance of the studied channels. Rather I suggest to take an extra mile and structure the motivation, especially for publication purposes.

On top of this motivation shortcoming, there is a fundamental weakness in using *decline* in unionization as justification for blending collective bargaining into the technology story: the model developed subsequently is static, the Author can pursue some comparative statics, but cannot use the model to discuss trends or tendencies (transitions in a dynamic setup). Incidentally, this aspect is very thoroughly elaborated in Study 2, where comparative statics are related to potential underlying tendencies.

Model. As a profoundly empirical person, I tend not to expect theory models to be useful descriptions of the world: a model is to be an intellectual construct useful in rigorously identifying some intuitions. This is why I do not debate the assumptions made by the Author per se. However, some of the explanations for these assumptions are at the very least annoying. There are several examples and interest of brevity I will limit myself to just a few: (a) if the general human capital is irrelevant for the firm, it should not enter the output, eq (1.1); (b) a worker who exerts no effort has zero productivity, rather than z (numeraire?); but more importantly it is not very obvious to me why z should be alternatively interpreted as an *upper* bound on observable effort, especially since there is no cost of monitoring considered (expected value probably, but why *upper* bound?); (c) the Author assumes the skill premium to be firm specific without even trying to justify this choice; (d) the Author states that *the task-content-of-jobs literature finds* that technology increased productivity of unobservable effort, where the whole structure of this literature relies on observing tasks on the job and changing task content of jobs, hence literally the *observed and observability* of the workers efforts; (e) I could not agree less with the alternative justification of the sequence of actions (bottom of page 22), actually I think the logic is confused, because the narrative is opposite to the actual modeled sequence; (f) the assumption that the collective bargaining is equivalent to an equal wage for all workers, which is the key assumption in the model, is the one assumption never related to empirical or previous theoretical literature; (g) the model talks about performance pay and collective bargaining, but in fact it is performance pay and equal pay with autonomous utility derived from being in a collective, such naming would have been much less

controversial had the Author spelled out his understanding of these terms in the beginning of the chapter and critically evaluated such choice of terms. These comments are intended as guidance to the Author, if he intended to work on the first study for publication purposes.

There is also a couple of issues with the narrative around the results. For example, in Equilibrium 1, there is no uncertainty of output (all is in levels, agents behave in expectation, uncertainty disappears). If that is the case, all parameters are observable at the moment of contract, there is no need for incentive compatibility and participation constraints, etc. If the model considered uncertainty (second order moment), then the results would have been substantially different than the ones offered in levels and it is not clear why the Author did not extend the reasoning to cases where uncertainty actually matters (i.e. concavity enters). The sections related to firm heterogeneity and matching are convoluted and difficult to follow, the Author would do himself a favor discussing in detail the changes in the setup, in particular with reference to equations 1.35-1.37. Also, I cannot understand how come $x_{i,j}$ suddenly becomes fixed (the top of page 34), whereas this statement is key for the derivation of propositions listed in Equilibrium 2.

Hypotheses. The model derives several propositions and/or intuitions, but the Author states that there are three testable hypotheses in his study. I think it is a shame in a sense that either all the other theory was not really needed (no link between most of the theory and empirics) or the Author should have invested far more effort into the empirical analyses. Given that the thesis is already quite short, I lean towards the latter opinion.

The very hypotheses are problematic to me. The collective bargaining concerns the change in wages (growth rates rather than levels) and concerns all the workers, not solely those, who *resigned* of the performance contract. Hence, the world and the model features are completely off sync. I have also serious doubts concerning Hypothesis 2 specifically. First, the collective bargaining agreements concern exactly the profit sharing (which in the model is the performance pay) and relate the size of the bonus to general payroll (at plant level or worker level). Second, both H1 and H2 implicitly require that not only the workers have no outside options (not necessarily the case in reality), but also that they cannot change sectors (otherwise there is no point to use industry as a proxy for firm, although this last implicit assumption is actually never spelled out in the thesis). Given the substantial decline in industry-specific human capital across multiple occupations, shortening job tenures, as well as profound change employment shares across the few recent decades used to motivate this study, translating firm-level theory to industry-level analysis may be an excessively long shot. It this requires more through discussion. Hypothesis 3 is a mistake, since the model is static and the hypothesis talks about transitions, so I am mercifully abstracting from discussing it further.

All in all, my impression is that the Author does not exhibit thorough understanding of the links between his model and reality, which does not provide the reader with an optimistic forecast for the quality of empirical application.

A bigger concern with the hypotheses and interpretation of the model intuitions concerns the fact that as much as a model should be abstract, its empirical application cannot abstract from reality. In general, we observe several secular trends in wages related to firm size, productivity growth, labor share, and so on. One very important trend concerns the changing role of within firm and between firm wage inequality. The evidence from the US, which identifies the key role of “star firms” and growing between-firm wage inequality with a secular decline in within-firm wage inequality (Bloom et al, 2018, QJE, but the working paper has been around for a couple of years; it has at this point ~ 300 citations; see also related literature: Mueller et al, 2017, AER) is particularly relevant for the story in this thesis. This may be one of the reasons, why the empirical results are so weak (I discuss this point at large below).

Another important “intellectual omission” on the side of the Author is the link between his setup and the long tradition of the worker managed firms, dating back to Vanek (1970) and his students: Derek Jones, Janez Prasnikar, and Jan Svejnar. All these papers were published in top journals at the time and show important links between profits, bargaining over wages by unitary workers as opposed to joint workers decision making. This literature has delivered a number of relevant findings on the links between profits, workers sorting (and old name for workers heterogeneity), inequality and contract design. Some of the results by the Author are analogous, especially in those of these old papers, which have an equally simplified definition of the production process. It is my understanding, that in particular Propositions 4 and 5 are related to this earlier literature and I believe it would be informative for the Author to see how his operationalization of technology fares against this nearly three to five decades old theoretical literature. As a side comment, let me state that testing the empirical validity of Propositions 4 and 5, with the available linked employer-employee data, would be of particular contribution to the empirical literature in the field (to the best of my knowledge, at least).

Empirical application. The Author presented four tables with results, preceded by very rudimentary and sometimes actually arrogant description of the data used. Such rough treatment of the data would have been justifiable if the Author intended to use these results as a motivation for his theoretical setup, rather than its verification. However, if the results were to serve as motivation – they should work in the expected directions. Meanwhile, it is not the case. Naturally, this is not the fault of the Author that the data do not confirm his theory, but this lack of support should be an important signal for the Author to discuss thoroughly the match between his theoretical design and the empirical application.

As I mentioned above, I have issues with using industry (and such broad industry definition!) as proxy for firm, but conditional on this choice, the results by and large fail to confirm the hypotheses formulated by the Author. Almost nothing is significant in Table 1.1, the same countries get implausibly different estimates in Table 1.2 despite addressing supposedly the same phenomena only a few years apart. Since the Author does not report any descriptives, one can only guess that there is some cooking with the snake oil related to measurement issues, definitional issues or even worse. The Author finds *significant positive* coefficients for some countries, where his interpretation of his theoretical model suggest a negative coefficient and leaves that without a word of comment. In a sense, I want to commend the Author for reporting Table 1.1. and 1.2, because things are neat and elegant in Table 1.3 (which reports analogous results but in a pooled regression rather than by countries), but even Table 1.3 reports as insignificant or barely significant the coefficient of interest. I could possibly write many versions of interpretations for the results for Europe, but one thing I would never dare writing having obtained such results is that “The econometric results are broadly in line with the predictions of the model.” (last sentence on p. 54), because it is simply factually incorrect, even with the “broadly” adjective. I applaud the Author for identifying that SES data contains information about collective bargaining coverage. I would however expect to see some analysis of the within-firm and between-firm variation in this variable. It is a mystery to me why – using the SES data – the Author did not work with a finer definition of industries. It would be useful to move step-wise towards higher level of aggregations to study the within and between variation, prior to engaging in the very aggregated correlational study.

The study using the US data is troubled with a different issue. Most notably, as I mentioned above, the theoretical model is static and the estimated relationships relate to trends over time. The presented estimation has not a single trend-related control variable, hence the estimated coefficient exploits only time variation and finds two inversely correlated time trend. Interpreting these results as evidence for anything rather than simply inversely correlated time trends is an intellectual transgression.

Study 2

Motivation. The Author begins the narrative by stating that firms increasingly organize workers in teams, but the operationalization of teams is never explained when empirical results are reported. This matters, because essentially the understanding of the team in the theoretical setup relates to the number of workers (which is identical to the size of the firm/plant). Hence, in parallel to Study 1, the specific word matches (“team” in management literature and “team” in Author’s theory model), but the specific meanings do not. The motivation and the overview

of the literature for Study 2 is even shorter than in the case of Study 1, leaving me with little more to discuss.

Model. After having read the thesis carefully, it remains a mystery to me, what is the actual contribution of this model. I am not expert in “search for ideas” literature, but the Author does not explain what are the actual innovations he introduces relative to the existing state-of-the-art. Certainly, it is not the “teams”, as this term signifies simply the number of workers (=size of the plant). The Author considers a case where workers skills come from different distributions, which may generate positive synergies (the synergies could in principle be also negative, but this case is excluded from further analysis by the property of equation 2.29, it is a shame because generalization should be straight forward). Hence, the Author follows a thoroughly researched path of adding concept analogous to economies of scale to an otherwise standard setup for power sizes and distributions of sizes (with great contributions from Growiec as well). It could be, naturally, that I am missing something, but it should be evident from the writing of the Author, what is contribution really is.

The narrative for equations 2.32-2.35 is so convoluted that building any deeper understanding of the actual contribution for the Author extends far beyond my duties as a Referee.

Hypotheses. There are several propositions in this part of the thesis, but according to the Author none has merited to be emphasized as a hypothesis.

Empirical application. There is no empirical application. The Author presented two tables with simulation results for a setup described as “general case” in section 2.4.2. The purpose of these two tables is to quantify the links between the team composition and the properties of ideas generation for the teams members (using the language of the model). As the Author states in the last paragraph of Study 2, “[w]hat remains to be done is to take this model to the data”, but his narrative about the implementation remains normative, it does not become any more practical, or specific.

Other comments are minor and I give them to assist Mr Chrostek in further developing his concepts for publication purposes.

- The reviewing of the earlier literature is generally bad. The purpose of a literature review in a paper/essay/chapter is to inform the reader about the current state of debate, the “known knowns” and the “known unknowns” at the very least. The reader should be able to understand from the literature review section why is more development in this field needed and how the presented contribution fills any knowledge gaps. The reviews presented in this thesis state that some concepts

(collective bargaining for the first essay and team work for the second essay) have been at all studied in some previous literature, i.e. they exist. This is definitely not sufficient.

- I find the writing of the thesis generally sloppy. I list just examples for justifying my statement (i.e. the list below is far from exhaustive). Not all symbols are explained when they are used, starting from equation (1.1). The Author introduces unemployment and subsistence consumption (bottom of page 21) to then state that the model has exogenous number of matches and does not feature unemployment (the first paragraph of section 1.1.5 at page 23). In Assumption 12, the Author assumes the distribution F to be non degenerate, but then the subsequent propositions (e.g. Proposition 11) still condition the statements on whether or not this distribution is degenerate. The assumption of risk neutrality is never spelled out properly in Study 1 (a random sentence in passing). Having carefully read the thesis, I am not what the Author had in mind writing several times “generalize to almost any distribution”, because he neither explains which are “almost any”, nor does he list those who are not. Here, I do not criticize any of the assumptions, rather I bring to the front the internal inconsistency of the Author in presented thesis. What the Author calls unionization is actually collective bargaining and collective bargaining makes sense only if there is any surplus to bargain over – an issue that is never even mentioned. As another example, I do not think it is customary to provide proofs for equilibrium (by construction: a definition). Proofs are in order for statements that can be denied and definitions are not such statements. Hence, one can prove e.g. existence of the equilibrium, uniqueness, feasibility and other properties, *once the concept of the equilibrium has been defined*. Perhaps this is a purist approach to writing, but I see no reasons for why writing should not be adhering to such logical standards.
- The use of many words is generally too careless for academic standards. For example, the Author uses causal sentence structures when reporting earlier literature without ever delineating if a statement attributed to given author(s) is theory, empirics or both and whether or not it is causal or simply correlational. The thesis is full of statements such as “small”, “large” or “rather limited” without any metric of justification allowing the reader to judge if such adjectives are valid. In general, they are thus redundant. If a pattern fails to display for 3 out of 25 analyzed cases (>10%), then it is not universal (English is rich enough to suggest multiple adequate adjectives, if the Author insists on using them to describe facts). The Author also likes to repeal assumptions (instead of relaxing them) and to be “generally confident” with his choices, without delving into

raising the confidence of the reader. Such arrogance in writing may stem from insufficient command of English rather than actual disrespect for the readers, but either way it is not likely to earn the Author many credits with the referees or editors.

The above comments relate to the writing of the thesis, not to its substance. I do not comment on the introduction and the concluding section of the thesis, as they are “rather limited” (to ironically cite the Author) and as a reader, I did not have the impression that the Author expected the reader to take these two sections seriously.

Mr Chrostek has presented high ability to develop theoretical setups and to thoroughly analyze the properties of these setups. The setups developed in this thesis are novel in terms of proposed mechanics and have not been studied previously in the literature, to the best of my knowledge. Given this overall evaluation, I deem that the requirements of the law on degrees and titles in science and arts (the act of March 14th 2003, Dz. U. Nr 65, poz. 595, with subsequent changes) have been met and Mr. Chrostek should get the opportunity to publicly defend his dissertation.

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