# How Not to Make Historical Comparison Empirically

A comment to Pavel Machonin and Milan Tuèek's article "A Historical Comparison of Social Structure in the Czech Republic in the Years 1984 and 1993" (*Czech Sociological Review*, Vol. II, 1994, pp.149-172)

The fact that the post-communist transformation is considered a laboratory of social change – during which political, economic and social processes assume unusual speed and, consequently, generate remarkably strong mutual dependencies – apparently creates a very advantageous position for sociologists studying social change. Indeed, one of the major advantages of studying the post-communist transformation is very often seen in the exceptional possibility of *studying 'historical change' empirically*, that is by analyzing data from sociological surveys, statistical data, data from specially designed longitudinal studies, etc. Thus, it is quite understandable that there has been a strong temptation to analyze the post-communist transformation as if it represented a *'compressed history'* of a society's development toward democracy and a market economy. There are, however, at least three pitfalls to be taken into account when considering an empirical exploration of the post-communist transformation.

First, one must keep in mind that the type of causality we deal with when exploring the post-communist transformation as a specific type of 'social change' is not necessarily identical with *'historical causality'*.[1] As argued by some students of the post-communist transformation (e.g. Offe, Sztompka), it is the *'dilemma of simultaneity'* rather than *'historical causality'* that is typical of the social change underway in the formerly communist countries. Instead, we are dealing with a specific type of causality between economic, political and social processes, a causality that has been shaped much more by blue-prints prepared by political elites than by *'conditional laws of history'*.

However, questioning theoretical models of post-communist transformation that assume 'historical causality' should not result in an unconditional rejection of causal interpretations of vital relationships between various dimensions of post-communist transformation. One of the crucial theoretical premises in the study of changes in social structure and stratification in post-communist countries consists, in my view, in the assumption that the causal links between the structural components of the post-communist transformation are a typical example of *asymmetric causality*. As argued by Lieberson, "causal asymmetry simply means that an outcome generated by a given cause cannot be reversed by simply eliminating the cause or turning back the cause to its earlier condition".[2] Thus, it would be naive to assume that the abrupt dismantling of the totalitarian political regime, the legitimization of private property and the implementation of market mechanisms in the economy will 1. re-establish the meritocratic backbone of the society by re-introducing meritocratic criteria for job allocation and individual mobility, 2. re-

[1]) For defense of the concept of social change as well as for the discussion of various types of theories of social change see e.g. Raymond Boudon: Theories of Social Change. A Critical Appraisal (Cambridge, Polity Press, 1986) or Gerhard Lenski: "History and Social Change" (American Journal of Sociology, 1976, pp. 548-64).

[2]) Stanley Lieberson: Making it Count. The Improvement of Social Research and Theory, University of California Press, Los Angeles, 1987, p. 175.

define social hierarchies and 3. re-establish their consistency and congruency with value orientations and beliefs.

The second pitfall encountered when analyzing the transformation processes in Eastern Europe is that it is particularly important to take into account the role played by life-strategies, deeply ingrained habits, and beliefs about the social system. If it is true that 'social time' is running more slowly than the time of political and economic changes (Dahrendorf), we may assume that the life-strategies and life-styles that crystallized during the forty years of communism will overlap the strategies and life-styles growing from the new conditions, thus creating a peculiar mixture difficult to analyze and explain by traditional theories.

Therefore, neither analyses of the *structural* components of the change nor analyses of the *subjective* responses to the system can provide a satisfactory picture of the transformation of the social structures taking place in post-communist countries, particularly if the two analytical strategies remain separate, or only loosely connected. The gap between *'what exists objectively'* and *'how it is perceived'* has a tendency to widen when social change accelerates and puts in motion not only individuals and social groups along social hierarchies, but also the hierarchies themselves, the underlying systems of value orientations, dominant ideologies, beliefs, etc. In such an exceptional situation, the success of an empirical analysis of the system depends on the *theoretical model* of the relationships and causal effects. If such a model is weak or, indeed, non-existent, the danger of a serious misinterpretation of important processes shaping social structures in the transforming societies increases.

The last pitfall is a methodological one. A long debate in the literature over the problem of *comparative measurement* has shown that the results of comparative analyses based on *observed* and, in particular, on *latent* variables that do not meet the requirement of *measurement* equivalence "(…) may stimulate sociological imagination, inviting all sorts of ex-post explanations, but they are not likely to provide explanations solidly grounded in empirical data".[3] Violating the assumption of measurement equivalence is no less (but rather more) harmful when the variables enter various multidimensional techniques and analyses of relationships. Using factor analysis to develop latent constructs from observed variables (indicators) measured on different scales, not to mention different wording of survey questions may be especially problematic, since there is no evidence of how differences in measurement affect differences in factor solutions.

In my view, although in their paper Machonin and Tuèek (hereafter M&T) touch on a number of extremely important questions concerning the development of social structure and social stratification during the post-communist transformation, and use extremely rich data from surveys capturing social stratification both before the collapse of the communist regime and after four years of dramatic change, they fell into all three pitfalls mentioned above. At the end, this causes a paradoxical situation in which – after

[3]) The quotation is from M. Kuechler: "The utility of surveys for cross-national research." *Social Science Research*, 16 (1987), p. 239. For the review of the problem and an extensive bibliography see for example D. F. Alwin: The Application of Structural Equation Models in Comparative Sociological Surveys Research: Studying Beliefs About Inequality in Six Nations. (The paper presented at the conference on Social Science Methodology, ISA Research Committee on Logic and Methodology, Trento 1992) or M. Kuechler: "The utility of surveys for cross-national research." (*Social Science Research*, 16 (1987): 229-244).

enormous manipulation of data and the performance of many analytical steps – no question is in fact answered, no thesis seriously challenged and thus the room for speculation remains at least as wide as at the beginning of the whole exercise. Since these are quite serious allegations, I would like here to bring in at least the most important arguments supporting my critique.

M&T declare at the very outset that the major purpose of their paper is not theoretical elaboration but an empirical examination of transformation processes, particularly the development of social structure in the Czech Republic between 1984 and 1993. The absence of a *theoretically* relevant and well-formulated question is, however, the crucial reason for doubting the success of the whole, quite complicated empirical exercise. The principal idea, as stated at the very beginning of the paper, is only a brief sketch of various approaches supporting the authors' argument that what we are witnessing in East-Central Europe is not a simple *transition* to the market and democracy, but rather a very complex and deep *transformation*, or, as M&T put it, "a qualitative historical change (…) involving changes in civilization and culture, economy, the political system and spiritual life" (p. 150). I am not questioning the broadness of the concept of the post-communist transformation, but I doubt that such a sketchy definition can provide the necessary *theoretical* background for such an extensive comparative analysis. Moreover, it is hard to understand why changes in social and economic *inequality*, *social structure* and *social stratification* – which, in fact, represent the core issue of the analysis M&T plan to carry out – are eliminated from their 'broad' definition of the transformation.

As a consequence, I could find no clearly formulated *analytical question* raised for the empirical examination, not to mention clearly formulated *hypotheses*. Since there is no *primary research question* to be *answered*, no *hypotheses* to be tested, and no *thesis* to be challenged, it is difficult (if not impossible) to draw any *conclusions* at the end of the whole exercise, or even to summarize the most important results.

Though M&T are not explicit about what the *general* question they want to answer is, I believe it can be drawn from various formulations concerning the aims of their empirical exercise. In my view, the core question M&T address would read as follows: *Was there, in the Czech Republic between 1984 and 1993, any significant progress in the reconstruction of a deeply eroded 'meritocracy', progress brought by the gradual dismantling of the egalitarian distributive system and the reconstruction of vertical social differentiation based on a higher congruency among various social hierarchies? If such a process has been present, we should be able to find higher status consistency in 1993 than in 1984.*[4] This question may not necessarily cover all the goals M&T set themselves, but I believe that it lies at the heart of their analytical effort.

If my understanding of their primary research question is correct, then, of course, M&T do not simply make a *descriptive* comparison of social structures depicted at two points in time. Instead, they concern themselves with a quite complex *analytical* comparative strategy, for which the ultimate goal is not to compare and interpret simple distributions of the *observed* variables, but to elaborate and interpret quite a complex

[4]) I put it this way because it is obvious that M&T implicitly assume that if there has been progress in what they call the 'meritocratization' of the stratification system, we should be able to find a 'higher degree of social consistency' (p. 151). Whatever M&T mean by 'social consistency', it is clear from the foregoing empirical exercises that it is status consistency.

structure of causal *relationships* between *latent* variables. Though in the *descriptive* approach, the equivalence of measurement protocols creates a much stronger position for comparative statements than comparisons of the distributions of variables based on different measurement protocols, it is always possible (though not highly recommended) to draw some rather loose conclusions regarding similarities and differences among the distributions, while acknowledging that the measurement protocols for the compared variables were *different*. However, this is not the case for the *analytical* approach, since the possibility of drawing *any* conclusions about relationships and causal effects is *unconditionally* based on the assumption of measurement equivalence. In the absence of this, one cannot decide whether the conclusions concern differences or similarities between the measurement protocols or between the compared causal structures or social situations under study.

Aside from the lack of necessary theoretical elaboration for the principal analytical question, the major problem in M&T's paper consists in the fact that the entire empirical exercise, in fact, does not provide empirical evidence for *any* statement concerning development in the social structure and in vertical social differentiation between 1984 and 1993. In light of what has just been said, it is not clear to what extent the results presented by M&T arise from differences or similarities in the measurement protocols, and to what extent they reveal actual changes that occurred in the social structure and social stratification over the course of the period under study.

Moreover, neither do the authors provide sufficient information concerning the variables they used in the various steps of their analyses, nor is there sufficient description of the transformations applied to the various observed and latent variables. Therefore, even with the same data sets in one's possession, one has little (if any) chance to replicate M&T's work or suggest alternative analytical strategies. Since there is not enough space here to examine every single step of M&T's analysis and reveal the inadequacies of both their analytical strategy and interpretations, I will restrict my critical comments to the most serious problems.

**Factors influencing earning distribution**

In the analysis of change in the determination of earnings, M&T draw striking and, in fact, incorrect conclusions due, in particular, to an inadequate and confusing operationalization of the analytical variables. M&T write: "However, the most remarkable change is the increase in the predictive force of managerial position distribution following from this variable's embodiment of self-employment. Thus, from the first step of our analysis, two typical processes in the ongoing social differentiation are observable: the parallel assertion of meritocratic and class principle" (p. 156). Leaving aside the obscurity of this formulation, it is easy to demonstrate that this conclusion is a typical product of the peculiar and confusing operationalization of one of the independent variables, namely that of 'managerial position'. Looking at Table 1 in M&T's paper (p. 154), we find that that the definition of *'managerial position'* confounds two things: a) position in the hierarchy of management (measured on slightly different scales in 1984 and 1993);[5] and b) the dis-

[5]) M&T do not explain, however, how they define the last category ('high managers') for 1993. This category, which in the 1984 survey was explicitly present among the categories of answers, was *not* pre-coded in the 1993 survey (the highest pre-coded category in 1993 was 'more than 10 subordinates').

tinction between employees and self-employed. The same table shows, however, that the former dimension (the number of subordinates) shows just minor differences between 1984 and 1993, the latter (self-employment) being, of course, entirely absent in 1984. Thus, from a *methodological* point of view, combining the two dimensions in *one* analytical variable is totally inappropriate in a comparative analysis. It is certainly also unacceptable from a theoretical point of view, since self-employment – a truly new phenomenon in the social structure of the formerly communist countries (particularly in the Czech Republic) – creates a new dimension of social stratification and wage and income inequality. Thus, the two dimensions should not have been confounded, if for no other reason than for allowing the possibility of capturing the differences between the two segments of the economy in view of the perpetuation of egalitarian patterns of wage policy.

In order to prove that M&T's conclusion regarding the changing patterns of earnings distribution is clearly a product of their confusing manipulation of the independent variables, I have performed a similar regression analysis for 1993 using two different coding schema for 'managerial position' (choosing different cutting points to define a 'dummy variable') and controlling for the independent effect of self-employment. Table 1 of this text displays the results. The regression coefficients for sex, age, education and socio-economic status (which I prefer to use instead of 'work complexity') are similar if not identical to those presented in M&T's paper, whereas the coefficient for managerial position is very different. As expected, the highest coefficient in my equation belongs to 'self-employment', even after controlling for the effects of education, managerial position, and socio-economic status. What is then 'remarkable', but quite understandable, if starting from a reasonable theoretical hypothesis, is not an 'assertion of meritocratic principles' as M&T try to convince the reader (the effect of education does not seem to be strengthening at all), but the gap between the 'bureaucratically' operated segment of the economy (i.e. the state sector) and the emergent private sector. I also strongly oppose the 'class interpretation' of the gap, since self-employment, as defined in both and M&T's and my own analyses, compounds quite a wide spectrum of classes, from small shopkeepers to private attorneys or large proprietors, each of whom represent members of different 'social classes'.

If M&T had a strong hypothesis before designing their variables and a particular strategy for their empirical exercise, they would have built separate regression models for at the very least two segments of the labor market: namely the state and private sector.

**Development of the class structure**

Another conclusion not grounded in the empirical data – though M&T believe it is – concerns the development of class structure. Table 3 of their paper displays two distributions: class position in 1984 and in 1993. The problem is that in comparing the two distributions, *no* conclusions can be drawn regarding the differences or similarities of class structures in the two historical periods as the distributions simply cannot be compared. First, there are quite different measurement protocols behind the two variables (the transformation used in 1993 creating 'EGP class' cannot be applied to 1984 data). It is true, however, that the approximation to the original EGP transformation developed by Boguszak[6] for 1984 survey data is quite close to the standard EGP classification, but it is not clear whether M&T use Boguszak's classification (if so, it should be acknowledged). However, the principal reason why the results M&T present in Table 3 are in fact useless for the stated purpose is that they have seriously modified the EGP class schema used in 1993 (though they apparently forgot to report about the modification they made). The results of my re-analysis, presented in Table 2 of this text, reveal the problem in its full light. Before interpreting the differences it should be noted, however, what kind of 'modification' M&T most likely made. It seems that, among other 'improvements', M&T re-classified members of all classes who reported 'self-employment' to the two categories composing the category of self-employed regardless other important attributes of *class location* (compare columns 1993b and 1993c in Table 2). Since the 'EGP class schema' is not only a 'technical tool' but also has a quite strong theoretical underpinning,[7] M&T's modification is *unacceptable*, primarily from the *theoretical* point of view. Moreover, it actually hinders any comparison since it in fact introduces differences between the two distributions (i.e. 1984 and 1993) that are then interpreted as *historical* changes.

Since it remains unstated what other 'improvements' to the EGP class classification M&T have made, I will demonstrate the problem using my own 'reconstruction' of their classification (column 1993b), comparing it to both the original classification adopted by the other members of the international research team (column 1993a) and to the retrospective assessment of class position in 1988 and 1984 (using identical classifications for all years under examination).

Comparing the three distributions, the only conclusion that can be drawn without additional speculation is that class structure in the Czech Republic between 1984 and 1993 shows unusual stability with two, quite obvious, exceptions: a) the decline in the proportion of farm workers, and b) the rapid growth of the self-employed (*petty bourgeoisie*). What M&T find in addition to these two trends, for example 'a substantial fall in the share of skilled workers and a partial fall in the total of semi- and unskilled workers' (p. 157), is not supported by either their Table 3 or my results in Table 2 of this paper.[8] M&T's interpretations regarding the category of self-employed are similarly unacceptable. If M&T had compared a simple variable showing the proportion of people who were self-employed in 1984 and 1993 (such a variable exists in both data files), their conclusions would be acceptable, but within the EGP class schema, the category 'self-employed' has a quite different meaning (it excludes large proprietors, managers and professionals who are actually 'self-employed', but by other relevant attributes of the *class* location they do not belong to the class of 'petty bourgeoisie', which is the term for classes IVa and IVb used in the original EGP class schema).

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[6]) M. Boguszak: Transition to Socialism and Intergenerational Class Mobility: The Model of Core Social Fluidity Applied to Czechoslovakia. In M.Haller (ed.): *Class Structure in Europe.* Armonk, Sharpe, 1990.

[7]) See for example R. Erikson and J. H. Goldthorpe: *The Constant Flux.* Oxford, Clarendon Press, 1992.

[8]) It is truly puzzling as to where M&T find their conclusions, since – according to their table – the proportion of manual workers (regardless of skill level) dropped between 1984 to 1993 from 47.8% to 45.3% (which certainly is not a 'substantial' fall as they try to convince us), while according to my results, it fell from 50.7% to 44.8%.

**Multidimensional status and clusters**

The core of M&T's empirical exercise is their definition of 'multidimensional status' variables and their subsequent application in cluster analysis aiming at the identification of 'status clusters' in 1984 and 1993. From the methodological and statistical point of view, this part of M&T's paper is most problematic. It moreover suffers the most from an evident lack of sufficient documentation on how the relevant variables were created, what classifications were used, and what modifications to the original scales were introduced. Due to this fact, I can make only general comments regarding the plausibility of the procedure and point out the most obvious inadequacies.

Regarding the factor analysis presented in Table 4 of their paper, there are two variables of dubious comparability between 1984 and 1993: 'managerial position' (MP) and 'cultural activities' (CA). As demonstrated above, due to the asymmetry introduced by M&T's decision to combine two dimensions into one variable (one's position in the hierarchy of management and self-employment), the two variables (i.e. MP-1984 and MP-1993) are not compatible. As far as 'cultural activity' is concerned, M&T do not provide sufficient information on how these latent variables were constructed for 1984 and 1993 (original 'observed' variables, scales of measurement, etc.). Hence, it appears that there was some 'hidden agenda' behind the two constructs, and the reader is left to believe that it was appropriate. Thus, the authors are in a difficult position to cast off the suspicion that this operationalization, if carried out in a different way, may have led to quite different results.

Indeed, looking at the results of the factor analyses, the differences between 1984 and 1993 are mostly due to different relationship patterns among 'managerial position', 'earnings' and 'cultural activities'. These are exactly as is to be expected if the measurement protocols were different for managerial position and cultural activities, which – as we have already seen – is certainly true for managerial position (the problem of bi-dimensionality and asymmetry) and very likely true for cultural activities (different scales and an unreported procedure applied in the creation of an index or latent construct).

Since all the subsequent steps of M&T's empirical exercise are based on the latent constructs (factors) of dubious comparability, I do not think that it would make any sense to continue in examining the individual statements M&T make regarding differences or similarities between cluster solutions or individual clusters identified for 1984 and 1993. None of these *comparative* statements are, in fact, well-grounded in the empirical data. There are, however, some further general methodological problems I would like to address, though briefly.

The major problem in M&T's analytical strategy, in my view, consists in how they applied cluster analysis, in very weak statistical evidence regarding the optimal cluster solution, and in the lack of any further exercise that would show the statistical strength of their final typology. I am particularly skeptical about the 'quick cluster' procedure implemented in SPSS. Simply speaking, 'quick cluster', the final step of M&T's clustering exercise, is only an 'exploratory' technique. It may be an efficient and simple tool in situations in which we know little or nothing about the structure we are analyzing. There is no criterion we can use to test a null hypothesis of no differences among clusters (the rejection of such a null hypothesis would be a *necessary* condition for accepting one out of many cluster solutions as the particular one that defines groups showing statistically significant differences in terms of clustering variables). Even the univariate F-tests for *individual* clustering variables provided by 'quick cluster' are only *descriptive* and cannot be used to test any hypothesis regarding differences between clusters. That is why the authors of the SPSS statistical guide explicitly suggest the application of other techniques (discriminant analysis, analysis of variance, etc.) to *test* the results of cluster analysis. No results of such highly recommended additional tests are presented in the paper.

Second, leaving aside the dubious comparability of the variables in the entire exercise, the major problem of M&T's approach is that despite the enormous effort expended to produce statistical evidence and despite an unusual quantity of various results and numbers in the paper (altogether 14 tables), the truly *statistical* evidence that would support any (unfortunately nonexistent) *testable hypotheses* is scarce (if any). There is, for example, no statistical evidence that the two-factor solutions imposed on the 'status forming variables' fit the correlation matrices well. What would happen if self-employment entered the analysis as a separate indicator of position in the social structure. Why has confirmatory factor analysis not been used to test the acceptability of factor solutions?

Third, since there is no statistical evidence supporting the choice of cluster solutions, we are left to believe that the ten-clusters solution are the best for both years (1984 and 1993). Various cluster solutions could have been tested at least *a posteriori* by discriminant analysis. As argued above, neither have M&T run such tests nor is there a strong theoretical reason for their decision not to do so. Moreover, when presenting the distributions of the various 'original' variables in clusters, no statistical evidence is provided regarding the *differences between individual clusters*. The coefficient of contingency (CN) is used to compare the strength of the relationships 'produced' by tables of different size (see e.g. Table 9). What is more, there are comparative statements without *any* support from the results of statistical analyses (e.g. the statement regarding the *growing* interdependence between the attitudes and social class – page 170), etc. All in all, the whole exercise is not only statistically very weak and quite messy, but in many respects even counterproductive (less in this case would actually mean more). Since the results M&T present in their paper do not allow any statistically well-grounded comparative statements, no historical comparison has been done – this despite the enormous amount of empirical material presented in the paper. The fact that no conclusions are presented at the end of the paper supports the suspicion that when there is no primary research question or hypothesis, no conclusion can be drawn.

Table 1. Regression of individual earnings in 1993
Standardized regression coefficients and T-values

| | Model I | | Model II | | M&T's Model |
|---|---|---|---|---|---|
| | Beta | T-value | Beta | T-value | Beta |
| Male | 0.33 | 22.8 | 0.33 | 22.9 | 0.33 |
| Age | 0.04 | 3.0 | 0.05 | 3.5 | -0.04 |
| Education | 0.14 | 8.1 | 0.15 | 8.3 | 0.15 |
| Socio-economic status* | 0.18 | 9.9 | 0.19 | 11.1 | 0.15 |
| Managerial position | 0.14 | 9.0 | 0.10 | 6.6 | 0.26 |
| Self-employed | 0.21 | 14.7 | 0.22 | 15.4 | - |

Notes:
Earnings: Logarithm of average monthly earnings from the main job in 1992
Male: 0 = female, 1 = male
Age: age in years
Education: Total number of school years
Socio-economic status: International index of socio-economic status (ISEI)
Managerial position in Model I: 0 = no subordinates, 1 = one or more subordinated persons
Managerial position in Model I I: 0 = 0-9 subordinates, 1= 10 or more subordinated persons
Self-employed: 0 = employee, 1= self-employed
*) in M&T's model "Work complexity" (the correlation between SES and Work complexity is about 0.9).

Source: Survey "Social Stratification in Eastern Europe after 1989"

Table 2. Class position in 1984, 1988 and 1993 (in %)

| Class position (EGP) | 1984 | 1988 | 1993a | 1993b | 1993c |
|---|---|---|---|---|---|
| Higher professionals | 8.4 | 9.2 | 9.2 | 7.9 | 9.7 |
| Lower professionals | 14.0 | 15.1 | 16.9 | 13.8 | 14.5 |
| Routine non-manuals | 14.3 | 14.3 | 14.3 | 14.3 | 14.0 |
| Self-employed with employees | 0.1 | 0.1 | 1.2 | 3.3 | 2.4 |
| Self-employed without employees | 0.5 | 0.5 | 4.2 | 6.8 | 7.2 |
| Manual supervisors | 4.7 | 4.8 | 3.8 | 3.8 | 2.1 |
| Skilled manual | 19.8 | 19.8 | 17.9 | 17.8 | 16.8 |
| Semi-skilled and unskilled manual | 30.9 | 29.4 | 26.9 | 26.7 | 28.5 |
| Farm labor | 6.9 | 6.7 | 4.9 | 4.9 | 4.8 |
| Self-employed farmer | 0.3 | 0.2 | 0.7 | 0.7 | 0.0 |

Notes:
1984, 1988, 1993a: Standard international definition of EGP (self-employed professionals are coded as 'professionals')
1993b: Modified definition of EGP (self-employed professionals are coded as 'self-employed')
1993c: EGP class schema used by Machonin and Tucek

Source: Survey "Social Stratification in Eastern Europe after 1989"

*Petr Matìjù*

# There Are Many Roads Leading to the Understanding of Historical Change

A reply to Petr Matìjù

The post-communist transformation can indeed be considered a laboratory of social change. However, at the end of the 20th century, it is generally acknowledged that there are a plurality of theoretical and methodological paradigms and their resulting research instruments which can be applied in this laboratory. (Compare [Petrusek 1992: 60-80].) Our critic does not conceal his adherence to the influential sociological stream of empirically-based status attainment research which, with some modifications, continues the traditions of social stratification studies typical of the 1960s. He likes to quote as his methodological 'credo' G. Lenski's paper, in which this respectable American scholar presents an outline of the consequent construction of a perfectly empirically verified 'multilayered theory' which he demonstrates on status-attainment research. [Lenski 1988] Although Lenski admits the difficulties in developing subtheories connected with the introduction of too many new variables, he does not call into question the possible development of an empirically verified general theory of social stratification. Matìjù apparently believes in the possibility of an even broader applicability of the explorative model taken from the natural sciences: theory – hypotheses – measurement – testing of hypotheses – conclusion (revised theory). The goal of such research activities consists mainly in developing general sociological knowledge valid for many or, at least, several countries on a similar level of cultural and social development.

We have a different task before us: the systematic study of the historical change in vertical social differentiation of one Central Eastern European country, Czechoslovakia – now divided into two new states – in the period from the end of 1930s to the present. While not ignoring the existing analogies with some other European post-communist societies, we cannot overlook the fact that the topic of our study involves a very unique configuration of frequent and profound political and social shifts in contradictory directions, combined with substantial foreign interventions. Our goal, then, is to describe the course of changes (using, in addition to the standard instrumentarium, our already empirically applied modifications of the generally accepted conceptual and methodological apparatus) and to identify any elementary regularities in the data. In this way, we hope to be able to formulate – in co-operation with the students of analogous changes in other countries and with those currently working on international comparisons – new hypotheses and, eventually, generalisations. This will be our contribution to the necessary revision of theories constructed prevailingly on the experience of relatively stable advanced Western countries. Our leading paradigm in such a situation would more likely be Marx's concept of theoretical cognition as a spiritual reproduction of the historical concrete – a principle unfortunately forgotten in the further development of Marxism and not sufficiently respected by most non-marxist schools.

We are lucky enough to have at our disposal the results of five representative social stratification and mobility surveys as well as those of some surveys perceptions of the stratification and of many other data collected in the Czech Lands and Slovakia since 1967. Thus we can draw upon both historical mobility analysis and historical comparison as two sources in the fulfilment of our task. However, these are not surveys intentionally undertaken for purely comparative reasons. On the contrary, the surveys from the years

1978 and 1984 in particular, were manipulated in the time by the 'normalisation' power structures in order not to be comparable with the then condemned stratification survey of 1967. The original data from 1967 were destroyed. In the 1993 stratification and mobility survey, the general international comparison approach was prioritised over the requirements of historical comparison. Given such conditions, it is quite clear that we will almost never have exactly identical measurement protocols of the compared variables and their derivative constructs.

Even if we achieve this in the future, the historical and cultural differences in two various stages in the development of 'the same' society would result in some factual differences in the meanings of formally identical questions. (Incidentally, this objection could also be raised against the cross-national comparisons where identically formulated questions can have different meanings in distinct socio-cultural contexts.)

It is also clear that such profound qualitative changes in managerial positions, in ownership structures and some other social characteristics as are occurring in the course of transformation can never be measured by identically worded questions and categorisation and scale constructions.

This all means that we simply cannot fully apply the – in principle neo-positivist – methodology of verifying and falsifying clearly formulated individual partial hypotheses by means of identically operationalised variables. Unless we are prepared to abandon our research goals, we have only one choice: to use the data collected in the macrostructural surveys, wherever possible, with the maximum exactitude allowed by the operationalisations of the given variables and to correct the results by a maximum of other, independent instruments of quantitative analysis. In addition, intensive use of qualitative methods is necessary as is the use of historical statistics and the knowledge gained by social historiography, demography, economical statistics etc.

From what has been said it should be clear that the very background of the differences in outlooks between ourselves and P. Matìjù consists in different research goals and, correspondingly, theoretical and methodological paradigms.

Matìjù is right in some points of his critique and some of his opinions and suggestions deserve serious considerations and appropriate empirical testing. This advice we have already used or will use in the near future. However we cannot accept his principal position pressing us to the full application of the methodology standard to intentionally prepared cross-national comparative surveys. We will continue in our systematic and patient use of the current concepts and methods. We will modify them, invent and try new ways of revealing certain regularities in the complicated social and cultural changes that have occurred in our countries. The main manner of verification and falsification of our interpretations will be a) in the congruence or incongruence of the pieces of knowledge acquired by various methods and techniques from various sources and by various researchers and b) in the testing of the explanatory force of the theoretical conclusions in relation to factual historical development.

In Matìjù's critical commentary, we are said to have succumbed to three pitfalls. The first of them remains unclear to us. Are we criticised for overestimating 'historical causality' instead of supporting Lenski's probabilist evolutionism? In this respect, we wholly agree with Lenski. Or are we asked to join Boudon's prevailing scepticism concerning the possibility of building senseful theories of objective social change? We know that the building of such theories is an extraordinarily difficult task but we believe that to contribute to its realisation – even by adding small stones of the mosaic of knowledge – is an important aspect of any serious sociological activity. If we are invited to share the view that we are dealing now "with a specific type of causality (…) that has been shaped much more by blue prints prepared by political elites than by 'conditional laws of history'", we are not prepared to do so. Our justification is the historically proved and apparent failures of most political elites in individual countries that tried in the first phase of the post-communist transformation to shape their societies according to their strategies and thus lost democratic support and the possibility of continuing their endeavour. If we are, finally, instructed not to be so naive as to assume that democratisation, legitimisation of private property and implementation of market mechanism will automatically lead to the re-introduction of meritocracy, we answer quite simply that we have known this since 1989 and that neither our theory nor the interpretation of data have anything to do with such simplistic images. Quite to the contrary: from the early 1990s we have openly criticised the normative and fatalist 'transition approach' and have tried to develop a probabilist 'transformation approach' to the post-communist changes. [Machonin 1992]

The second pitfall is explained as an underestimation of the legacy of communism and, perhaps, even of the discrepancies between 'objective' processes and 'subjective perception'. However, our arguments in favour of the transformation approach have always been based on the fact that several decades of life in a totalitarian and anti-meritocratic system had a profound influence on the attitudes, habits and life-styles of the population and institutions, and that this legacy continues to operate in the present cultural and social reality. [Machonin 1993] We know perfectly well that the 'objective' and 'subjective' changes are of a different temporality. However, it seems to us – on the basis of a comparison of Czech data from 1991 and 1993, of the analysis of the developments in voting preferences in public opinion polls in our country and of the observation of electoral behaviour in the other three Visegrád countries – that the step-wise-acquired social experience rather diminishes the discrepancy between objective positions and people's social and political attitudes.

The subsequent point of the 'second pitfall' concerns the theoretical background of our study. The theoretical model of the relationships and causal effects is certainly important. However, instead of pedantic formulations of partial hypotheses posing as a serious attempt to construct a causal model, we preferred to specify the subject of our study and our so far naturally somewhat vague theoretical considerations of the ongoing transformation process rather by the enumeration of both empirical and analytic basic parameters in the framework of which the comparison of social structures in the year 1984 and 1993 should have been carried out (see pp. 151-152 of our article). The reason for this was simply the fact that our attempt to receive some preliminary answers to a complex of crucial questions in an inadequately empirically described and rapidly changing space could not take the naive form of a series of simple hypotheses. If we declare that the aim of our article is to present the first results of a new stage in our empirical work, we mean it. It was high time – nearly two years after the data collection in the Czech lands for international comparative research – to publish information concerning the crucial points describing changes in vertical social differentiation in the course of the post-communist transformation. However, it does not follow from this, as Matìjù insists, that we abandoned the theoretical elaboration of transformation processes. The careful reader of our paper will find (pp. 149-151) a brief formulation of our theoretical background, including references to papers published in English and French. It is easy to see that Matìjù's 'reproduction' or, better said, 'reduction' of our theoretical approach to banalities comes more from his imagination than from reality. (Besides the already mentioned papers, see also [Machonin, Tuèek 1994].).

In determining the basic parameters of our comparative analysis, we did not consider the formulation of such a naive hypothesis as Matìjù holds to be our 'core question'. Our position is substantially different. Being aware of the complex and conflictual character of the investigated processes, we inquired what changes in the vertical dimensions of education, occupational and managerial positions occurred under the assumed influence of privatisation, liberalisation and limited modernisation. We pose the question as to what metamorphoses the factors determining the earnings distribution have undergone, with the assumption that the influence of both meritocratisation and privatisation should be visible. We declared our intention to describe both the class changes and the eventual progress of social status consistency and meritocratisation. And we assume that we will discover increasing associations between objective social positions and the subjective perception of the progressing vertical differentiation. Thus, one can see that for us the progress of status consistency and meritocratic principles represents only one of the ongoing processes. Progressing parallel to this is class differentiation based on changes in ownership and managerial structures. And it is the existence of and relationship between these two processes which lies at the core of our research.

Due to space constraints, we stated at the end of the article that the conclusions were to be found in the abstract. It was for this reason that our critic could not find them 'at the end of the whole exercise'. However, in the abstract published 'at the beginning' of our paper, the reader finds eight crucial statements which address the questions introducing our text. They provide as much and as detailed new information as one would expect from the first analysis of a new data set. We stated very clearly that the article presented the first portion of data analysis alone and was strongly oriented towards the most general issues of vertical social differentiation. Thus, no experienced sociologist can expect extremely concrete and detailed conclusions concerning the extraordinarily complex processes in question.

In spite of the attention devoted by P. Matìjù to the above-analysed groups of objections connected with rather theoretic questions, our opponent devotes most of his concrete comments to questions of methodological and technical nature, connected with his third 'pitfall'.

He presents four concrete methodological and technical objections against our paper, making almost no comments on the contents of our analysis and its clearly presented conclusions. His objections concern the operationalisation of the variables 'managerial position', 'class-occupational position' and 'cultural activity' – the last in connection with the application of the multidimensional status concept and the use of cluster analysis. Being, once again, constrained by limited space, we can only briefly react to these four issues.

1. In his first critical comment, Matìjù rejects our construction of the variable 'managerial position' as peculiar and confusing: in his opinion, it is based on an inappropriate combination of two dimensions: the number of subordinates and the distinctions between self-employed and employees. From the theoretical point of view, he argues from the assumption that this 'confusion' does not allow "the possibility of capturing the differences between the two segments of the economy". However, in 1993, that is in the

first phase of the privatisation processes specific to the Czech situation, the group of self-employed in primary jobs – this group representing about ten percent of the economically active, three quarters of them tradesmen without employees – were by no means the only representatives of the private sector of the economy. The principal privatisation processes did not, however, concern this group of people (which produced only a very small part of the national product), but the large corporations, which, at that time, had, for the most part, the formal status of joint-stock companies. The up-and-running privatisation (or privatisation being prepared in the atmosphere of the so-called pre-privatisation syndrome) resulted in an extremely important social differentiation among people working in enterprises. The core of this differentiation was the hierarchy of various degrees of managerial positions and the rank-and-file employees. The top and medium-scale management had far more opportunities to secure material advantage from the ongoing privatisation changes than the other employees. Thus, the principal differentiation line ran not only between those people declaring themselves self-employed and the employed, but also between those who had more opportunity to dispose of capital due either to ownership or to managerial positions. Simultaneously, this differentiation created a link between the situation under state socialism (when managerial position, combined, as a rule, with a corresponding political position, constituted one of the main status dimensions) and the transformation period.

As far as the empirical proof of our critic's assumptions is concerned (Matìjù, Table 1), he knows very well that as early as the end of the data-collecting year, we prepared a comparison of multiple regressions of individual earnings for the years 1984, 1991 and 1993, using the original (not yet 'reweighted') 1993 sample. They were published in [Machonin, Tuèek 1994], with a structure of independent variables very similar to that Matìjù used later in his critical comment written in 1995 and with analogous results. Thus, Matìjù's findings are not new to us. In no case do they indicate a split of the labour market into the state and private segments which would be defined by the difference between self-employed and employees in the primary job. In both segments, the average income conceals substantial differences between those entrepreneurs with employees and those without and between the employed as well as among various degrees of the scale of managerial position within the large majority of employed.

The reason behind our decision not to repeat the 1993 exercise this time lies in the entirely different purpose of the later analysis. We were not seeking to identify factors determining earnings differentiation in general, but primarily among the relevant partial status dimensions such as education, work complexity and managerial (power) position, of course in competition with the demographic variables. The simple dichotomy of self-employed and employed does not represent a possible partial status hierarchy. However, the inclusion of the differences among self-employed with and without employed and the rest of the economically active population as an organic supplement to the managerial hierarchy can help construct an important partial status-forming variable. Our main interest was namely to operationalize the managerial position as the crucial dimension of power differentiation. From this point of view, it was impossible to ignore the qualitatively new fact that the self-employed, particularly those with employees, actually acquired higher decision-making autonomy than employees in otherwise similar positions. It seems to us that our method of operationalising the managerial position as one of the partial dimensions of the multi-dimensional social status was theoretically well-grounded and that it corresponded to real changes in society. To accept Matìjù's formal objections would mean to abandon the clarification of important qualitative changes in power distribution.

2. As far as the class-occupational differentiation is concerned, Matìjù's comments are prevailingly rational. Indeed, we did somewhat modify the EGP scheme for 1993 in one respect. In the classification of those higher or lower professionals who were concurrently entrepreneurs, we in fact preferred the second, 'ownership' aspect of their position. We naturally feel that this modification was very useful for the 1993 situation as it enabled us to identify some important characteristics of the newly created social groups of private owners. It also helped improve the predictive power of the class-occupational position which we consider to be a promising indicator of vertical social differentiation in the immediate future.

We were aware of the disadvantages of using two different classifications for a historical comparison. It is for this reason that, as early as late 1994, while preparing the typescript of a new study (at that time unaware of Matìjù's reservations), we made a new attempt to overcome this problem. We used the original Czechoslovak occupational classification created by the authors of the 1984 survey and rearranged it into a categorisation roughly similar to the one we applied to the 1993 data. On the basis of these data, we still believe that some fall in the percentage of non-agricultural manual workers occurred in the years 1984-1993. According to Table 3 of our paper, it was a decline from 58.5% to 45.3% (not from 47.3% to 45.3% as Matìjù insists in his footnote 9; he simply forgot to count our highly qualified workers and other manual in 1984). According to our last re-classification, which brought changes mainly in the category of 'other manual', it was the move from 50.6% to 45.3%, which is close to Matìjù's figures. The decisive increase concerned the emerging groups of self-employed with employees and without employees which, in our modification of the class scheme, includes self-employed professionals participating in non-manual work. All these changes, although not as robust in all dimensions as we originally assumed, certainly correspond to the interpretation presented in our paper and are, according the later statistical surveys moreover, continuing. However, our statement concerning the considerable shift in the percentages of skilled and semi-skilled or unskilled workers (which we only announced but did not interpret in our article) seems to be a consequence of the different categorisations used in the two analysed years rather than a true reflection of reality.

3. The third objection concerns the operationalisation of the partial status dimension, 'cultural activity'. The manner in which it was created is briefly described on pp. 153-154 of our article. After having read the critical comment of our opponent, we undertook another empirical revision of our construction carefully respecting the most possible coordination of the variables and scales used in 1984 and 1993. The result was highly interesting: no significant changes in our empirical results – even including the complex synthesising social status constructions and all relevant cross-tabulations – emerged. Now we are even more convinced that in spite of some minor differences in the wording of the questions, the construction of this 'latent variable' is quite acceptable and that, in principle, it measures equally in both years precisely what we want to involve in the multi-dimensional status constructions. We can plausibly explain the differences between the values of correlations of this variable with earnings in 1984 and 1993 just as we can in the analogous case of managerial position. Throughout the whole analysis, both variables operate in fundamental accordance with the theoretical presuppositions. This result

is, in the framework of our theoretical and methodological paradigm, relatively satisfactory, although it must be verified by a series of further analyses.

P. Matìjù thinks that his critical remarks concerning two of five partial status dimensions used by us for the construction of the multidimensional social status index are sufficient to shatter the reliability of this synthesising variable. We do not share this view. First, we already sought to waylay his objections here. Second, the main verification of any constructed variable consists above all in the demonstration of its explanatory power. It is quite curious that P. Matìjù has made absolutely no comment on our comparisons showing that in the Czech conditions, the multidimensional status index and the (partly revised) EGP scheme in this respect clearly exceed the standard status indices based on international data.

4. Finally, a brief statement on the issue of cluster-analysis application should be made. We are fully familiar with all the difficulties of applying this technique. However, we also are fully aware of the necessity of having a well-elaborated typology of multi-dimensional status patterns and corresponding social groupings. In our paper, we made new progress in this direction, as is explained on p. 155. It consisted in dividing the procedure into two steps: a) The hierarchical classification on a limited, but relatively large (1000 individuals) randomly selected subsample, which enabled us to choose a relatively stable and lucid cluster structure with a sufficient amount (from the statistical point of view) of individuals. This structure happened to encompass 10 clusters in both cases – the number of clusters was not determined 'a priori' as Matìjù suspects. b) The selection of individuals to the centroids. For this purpose, only the classification part of the procedure – formally referred to as a whole as "Quick Cluster" – was used. Thus, in this case, the disadvantages of the quick-clustering against which Matìjù argues did not arise.

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