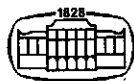


CRISIS IN ECONOMICS?

Studies in European Political Economy

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TABLE OF CONTENTS

PART ONE – PURE THEORY	7
I. Introduction: Why European Political Economy in an Americanized Discipline?	9
Reasons for a Copernican Change	10
Applications of the Americanized Theory in and on Europe	13
Do New Theories Emerge from the Applications?	14
Acknowledgements	16
References	17
II. Crisis in Economics?	19
Introduction.....	20
The Emergence of the Methodological Bias.....	21
Reasons for and Steps in Technicization	24
Challenges to the Technician Orthodoxy.....	30
A Gradual Penetration of Mainstream by Insights from the Fringes?.....	36
Beyond Newton	40
A Heterodox Revival.....	42
References	44
PART TWO – THEORIES OF POST-TRANSITION CHANGE IN OLD AND NEW EUROPE	51
III. Transition or Spontaneous Disorder?	53
Analytical Framework and Preliminaries	54
Why Is There Divergence in Transformation Experience?	59
Lasting Differences by Country Groups	63
Commonalities amidst Differences	75
Preliminary Conclusions	81
References	84
IV. Causes and Consequences of a New Variety of Macroeconomic Populism in Central Europe	87
Setting the Stage – Concepts and Categories.....	88
Varieties of Missing the Single Currency	92
Diagnosis: Major Factors Explaining Derailments	108
Preliminary Conclusions	111
References	113

PART THREE - THEORIES OF EUROPEANIZATION	117
V. A Premature Enlargement?	119
Mutually Reinforcing External and Internal Stagnation.....	122
On the Size and Structure of EU Transfers	125
Cohesion at the Crossroads.....	128
Stagnant Formal Europeanization.....	130
How Long a Consolidation Period Is Needed?.....	133
References	136
VI. The State of the EU: Revival or All Problems Swept under the Carpet? ...	138
Visionary Projects and Myopic Reality.....	139
The Nature of the Crisis - What Crisis?	141
Diversity, Stagnation or Consolidation?	148
Conclusion: The Limitations to Pragmatism	154
References	155
VII. The Future of the EU: Between High and Low Politics	157
The Lisbon Treaty - An Alibi or an Unused Potential?	158
Policy Reassessment in Each Relevant Area Is Already in the Making ...	161
Is There Life After Death? The Afterlife of the Irish Referendum	165
Is Hungary an Eternal Loser?	167
A Comedy of Errors?	171
United States of Europe or a Relapse into a Free Trade Zone?	173
References	176
VIII. Economic Transformation and the Process of Europeanization	178
Why Does Constitutionalism Matter for the Economy?	179
From Form to Substance and Disregard of the Economy.....	180
Europeanization - the Overrated Promise.....	184
Balance Sheet and Prospects	191
References	195
 PART FOUR - TOWARDS A THEORY OF SUSTAINABLE ECONOMIC	
GROWTH AND PROSPERITY	197
IX. Economic Conditions for Shared Growth	199
Global or Regional/Transition Specific Challenges?.....	201
The Reform Agenda in the Positive	206
Public Policy With a Focus on Bottlenecks	212
Overall Assessment and Prospects.....	215
Post Scriptum: Theory Strikes Back	218
References	221
 SUBJECT INDEX	225
 LIST OF ABBREVIATIONS	

I. INTRODUCTION: WHY EUROPEAN POLITICAL ECONOMY IN AN AMERICANIZED DISCIPLINE?

The financial meltdown of 2007–2009 has produced a series of claims inside and outside the economics professions maintaining that the currently available toolkit of the profession is next to no use. Calls for creating a brand new version of analyzing economic matters and discontinuing the tradition of the past 150 years of analytical social sciences can be heard at every corner and read in every paper. Therefore, we find it timely and legitimate to ask some of the fundamental questions about economics as a discipline, its methodology, relevance and applicability on subjects of the real world. In doing so, we first have to ask how we got there and where we are now.

The evolution of economic science has taken a resolute turn since the late 1970. This is reflected, *inter alia*, in the unprecedented geographical concentration of Nobel Prizes in Economics, having been awarded in about 95 per cent to scholars who have spent all, or at least most, of their productive years in the United States of America.¹ This cannot be taken as historically given, neither as a consequence of the enhanced financing and technological conditions of research, economies of scale and scope, which is the case in the natural sciences.

One of the most influential authors, Alan Blinder (1999), also highlights the growing Americanization and equates it with professionalization. For him, this is a synonym for the extensive – or perhaps often exclusive – use of econometric and mathematical techniques, which are considered *sine qua non* features of scientific analysis. The quality of their mastery is, at least in the mainstream form, decisive over its publishability, respectability and, indeed, its seriousness. In terms of language, sophistication, presentation and even issue areas, it has demonstrably narrowed the field in geographical terms. This cannot be *overshadowed* by the increasing presence of authors of previously less appreciated background, such as Chinese, Spanish Indian or Greek origin, as they tend to work, or be organically integrated to, the research networks of the big US schools.

¹ Nobel Prizes, either in economics, literature, peace building or in the natural sciences, certainly never constitute an undisputable standard, the final say of history. Still less in economics, an area in search of its identity and thus riddled with intellectual intolerance and animosities. Relying on a series of formal and independent appreciation of an impartial body, however, may help us find impartiality and thus strike a balance among competing – often mutually exclusive – interpretations.

Others, most notably József Móczár (2008: 18–24, 286) in his broad summary of mathematical economics, attribute this development to the economies of scale and scope of the academic market in the United States, which reflects the generally competitive economic arrangements of the country. While we shall try to get into the nitty-gritty of both sweeping claims, it still remains *puzzling* that of a total disciplinary history of around 250 years, the past ten per cent of the track should have produced such an *all-encompassing change* in terms of geography, genre, focus, interest, methodology and the potential circle of authors who may aspire to be taken seriously and recognized as school molding personalities.

This book, in its *first part*, is an attempt to provide at least a partial and preliminary answer to this *first puzzle*. It also tries to address and remedy some of the ramifications of this development in terms of subject matter, methodology, social relevance as well as applicability.

REASONS FOR A COPERNICAN CHANGE

One of the side effects of this all-encompassing change is the uncontested rule of English as *the singular means of communication and academic interchange*. Thereby Anglo-American publishers, journals and universities have taken the lead, not only in terms of setting the standards, but also in shaping tastes, values, approaches and even the choice of subject matter. A mere glimpse into the flagships journals of the economics profession, such as the *American Economic Review*, the *Quarterly Journal of Economics* or the *Journal of Political Economy*, may convince anybody in doubt about the validity of our claim.² It is particularly obvious that empirical issues, which may be relevant or fashionable in the US

² Tellingly, the German quality business paper, *Handelsblatt* in its July 2008 ranking of institutions and individuals, relies on the lists of the Tinbergen Institute in Holland and adds to it the contributions to the *Journal of the European Economic Association*. One may wonder why the editors opted for the TI against the previously used similar list made by the Kiel Institute of World Economics. This choice was never explained, although it clearly influences certain outcomes. While we share the editors' enthusiasm for the influential colleagues editing the *JEEA*, tellingly published by the MIT Press in the USA, we must note that this journal is currently merely in its sixth volume. For this reason, it has yet to prove if it will join, say the *Quarterly Journal of Economics*, or other leading academic forums, in carrying the most frequently cited articles in the profession. Likewise, we still have to see if it will graduate to publishing the printed version of the Nobel Lectures, let alone carrying any of the papers that deserve – usually with a delay of three to five decades – this decoration. Incidentally, all the three most recent Nobel lectures were printed in *The American Economic Review* (vol. 98, no. 3, May 2008). The alternative could have been *The Journal of Political Economy*, currently in its 116th volume. In short, the option chosen by a major opinion molder as a point of orientation for employers is a rather arbitrary option, disregarding the minimum of scientometric professionalism. Since selection criteria to a large degree predetermine the outcomes, this is not an innocent error – generally, more funding follows the rankings (as in many bureaucratic procedures including competitive bidding at governmental and international agencies as well as foundations). For a similar assessment of the ever more fashionable ranking of universities with a science appeal, see the critical assessment of Ádám Török (2006).

economy and society – be that gender, race, local elections, China or the su prime crisis – stand a disproportionately better chance in obtaining space an attention than other *per se* equally interesting and important topics, which m matter more for other sections of the globe but are less interesting for American. The latter include a number of traditional issues in European studies, such as t history, development and the intricacies of the operation of the single curren without a political union, or the still troubled lot of the economies that have (r emerged from the doldrums of communism.

Insightful analyses (e.g., Simonovits 2005) have called attention to the ve large dose of subjectivity and open favoritism in the selection process of the mc highly esteemed journals of the economics profession. But even more broadl other sources on the uses and misuses of scientometrics warn us about the lirr tations of the quantitative methods currently in vogue in evaluating scientil achievements.

The role of prejudice and other components usually considered to be outsid the scope of academic inquiry is shown to be at work even in such seeming policy neutral areas – or value-free fields, if you wish – as medicine or region studies (Braun ed. 2007). As the papers in the volume show, the most sophist cated Hirsch indices of several Hungarian physicists, for example, considerabl exceed the indices of physicists decorated with a Nobel Prize. The same is the cas in medicine. Self-references as well as foregoing the allocation of partial weigh- to multi-author pieces (currently ubiquitous in the natural sciences) result in ir flated impact assessments and non-comparability across disciplines and time pe riods. As the authors warn, and not only on the base of the above-cited extrem examples, one should be circumspect in making use of quantitative indicators an even positively suspicious of the proliferating listing of schools and individual: even though allegedly entirely merit-based.³ Therefore, it might make sense to ir vestigate components of merit, of trying to return to the *classical standards*, also i use across the natural sciences. This entails merits such as originality, lasting valu and lasting citation,⁴ in short peer assessment, which is perhaps more complex bu more reliable than the currently fashionable rankings.

Our endeavor should thus cover both the fields and methods of the econom ics discipline, as well as the outcomes of the analyses, in the broad sense. As . *second puzzle*, we shall address at least some of above concerns and show why and how it matters to economics, in terms of research area and academic per formance assessment alike. Both have undergone Copernican changes without

³ These listings are by no means innocent. They orientate good students as well as research funding in a number of areas just because of their science appeal.

⁴ Contrary to frequent journalistic claims, citation, in a sufficiently long period of time, does no follow fashions and personal relations, if for no other reasons, because of the changing nature o both. Thus, while it is a true observation that extreme positions arouse considerable interest an reflection, still those ebb out with the inevitable marginalization of the extremist position.

having been preceded by broad and consensual understanding, as was the case in arts or natural sciences.

A *third* factor of concern might be the increasing drift between the self-interpretation of the guild of academic economists, increasingly under the control of technicist orthodoxy, on the one hand, and the challenges of social, political and market expectations, on the other. E. Roy Weintraub (2002) of Duke University has gone perhaps the farthest in describing the mathematicization of economics, which has become established practice everywhere over the past quarter of the 20th century, and which means much more than the instrumental use of mathematical, statistical and econometric techniques of various sorts. What he criticizes is the tendency to take mathematics *as an independent structure*, determining the questions and thus the answers as well for the entire profession. For this reason, while the coevolution of academic economics and at least some trends in mathematics is rather straightforward, the relation to any of the many *real world issues* has become rather *coincidental*.⁵ True, this coevolution has rarely included feedbacks from economics to mathematics, with the potential exception of financial economics in general and risk assessment in particular.

Alan Blinder (op. cit.) is quite straightforward in claiming that the economics of the past two decades has produced relatively little of what he terms '*useful knowledge*'. Being a practitioner – as former member of the Council of Economic Advisors and also active member of the governing board of the FED – and also a prolific writer on policy relevant issues of various sorts, he might be particularly well placed to make this comment. In the view of several representatives of the mainstream, such as Gary Becker and Robert Solow, to be cited in the next chapter, economics has entirely lost its separate subject matter and has evolved into methodology, more precisely *a specific brand of modeling*. Advances that have been made in the area of quantitative analyses, modeling and forecasting have, notwithstanding the frequent claims to the contrary, contributed little to the management and the efficient solution of various *societal, corporate, financial, environmental and redistributory* problems. The latter have been multiplying over the past few decades, rather than fading away as the naive modernization theories of the 1950s and 1960s would have had it in East and West alike. The cumulative unresolved challenges have been calling for novel solutions for such traditional economic issues as the *financial* tremors, the stagnation of the universalist European *welfare state*, *global poverty*, the *resource curse*, the spread of macroeconomic populism and its ramifications or the stagnation of *European integration* at the time of globalization and the information and communications technology revolution (ICT).

⁵ According to József Móczár (2008: 233): "Economic historians have offered a multitude of proofs that by nowadays there emerged such an abstract version of academic economics, which bears no relation whatsoever to empirically observable facts of the economy, but meanwhile it has become

Fourth, as an interesting, though by no means unforeseeable by-product of the standardization, over-technicization and Americanization of economics education, also at the PhD level, is that a certain mismatch has emerged between the *aspirations* and abilities of successful postgraduates, on the one hand, and the *needs and priorities of the labor market*, on the other. People coming from the top schools have difficulties in being integrated in their local academic and societal environment, which is a phenomenon not constrained to transformed societies. Analyzing this issue through a series of methods including surveys of the graduates, David Colander (2008) has found that the internal evolution of PhD training has created a *mismatch* by eroding some of the traditional pluses of the traditionally much *broader European approach to postgraduate education*, while disregarding some of the rather fundamental *differences in the institutional setup* of European and American labor markets, academic and non-academic alike.

APPLICATIONS OF THE AMERICANIZED THEORY IN AND ON EUROPE

Fifth, although in Central Europe accession to the European Union has contributed to solving some major problems that have been bequeathed by the cold war and the collapse of the Soviet Empire, it has clearly fallen short of triggering a series of structural reforms which could have laid the foundations for lasting *financial solidity* and *sustainable development* in the broad sense, i.e., more than mere quantitative growth. At the time of writing, it seems that both major development issues have remained unattended as yet.⁶

Sixth, the European Union itself has been emerging, even though quite slowly, from its self-created political and institutional stagnation. The latter has evolved with the derailment of the project of political deepening launched at the Constitutional Convention of Laeken back in 2003. With its relatively smooth ratification in 23 out of 27 member states by September 2008, the overall paralysis and the climate of deep mutual distrust seems to be over. The Irish veto of the Reform Treaty in June 2008 somewhat slowed this process but by no means reversed it – a claim we shall substantiate in a separate chapter. This new opportunity can be seen as the carrot for reformers while the challenges emanating from globalization in general and the conclusion of the Doha developmental round in the WTO in particular, will create the much needed stick (Sally 2008). The interaction of the two is likely to *galvanize changes* and thus help to *institute some of the overdue reforms, rethink enlargement* as well as *alternative neighborhood policy*

⁶ In our analysis, we shall abstract from the trends that have evolved in the New Independent States, as these seem to fit better to developmental studies. On their core, Russia, a recent survey by Anders Aslund (2008) might serve as a point of reference.

options. The need to remain active in Turkey, the Western Balkans, Ukraine and the new independent states, as well as in the Mediterranean remains a top priority for the EU and for its common foreign, security, trade and financial policy. The formation of the Euro-Mediterranean Union in July 2008 is a clear indication of the centrality of these issues for strategy- and policy-making alike.

As the fifth and sixth issue areas to a large extent overlap, it might be legitimate and even necessary for us to devote some time and space, as a *seventh issue area*, for covering the *common ground*. Was the process of eastward enlargement delayed or rather premature, and has it been, on balance, a success or a failure? What should we think, with the benefit of hindsight, about 'transition strategies' that have culminated in EU accession in some cases, but led to being left out in the cold in others? How efficient has social engineering – materializing in constitutional development and leveraged institution building during the accession process – been in terms of transplanting and internalizing EU values and procedures in post-communist societies?

DO NEW THEORIES EMERGE FROM THE APPLICATIONS?

Finally, as *eighth*, the question arises whether we can draw any more *general theoretical lessons* from the diverse regional experiences in Central and Eastern Europe over the past two decades for broader global economic analysis. Such lessons might, inter alia, relate to the roles of public policy, path dependence, the role of *spontaneity versus* constructed projects/policy plans, the role of the *social acceptance* of economic concepts, the pluses and minuses of institutional *shortcuts* or the role of fiscal responsibility in bringing about lasting growth and development in historically backward or medium developed economies.

Even the mere listing of our eight major puzzles and possible angles of analysis should make it clear that this is not (even meant to be) a conventional economics book, confined to refinement of technical analyses. Its scope and approach, while firmly anchored in the standard economic paradigm, overstretch pure economic approaches and complement these with a series of insights from the natural and social sciences. We aim to *endogenize public choices, institutions, policy options and social feedback*, in line with decades-old traditions in the new political economy and the political economy of policy reforms.⁷ We thus join the *institutionally informed standard economic analyses* and apply these to a variety of *European topics, thus presenting-unique European perspectives and European concerns*. We hope to contribute, even if modestly, to the ongoing *change in emphasis*

⁷ Certainly, comparative economic systems' research, the Austrian school and German institutionalism, especially *Ordnungstheorie*, much of it originating in the 1890s and revived by the 1920s' and 1930s' debates on socialist calculation and the study of state-managed economies all count among the intellectual roots of this line of thought.

in the *global economics profession*. Meanwhile, we also wish to remain relevant to students of sociology, political science, international relations and even to analysts working for the business community. This position is in line with the traditions of European economic thinking, from Adam Smith through John Maynard Keynes, Friedrich August von Hayek, Walter Eucken and János Kornai.

When discussing systemic change, both in the "post-communist" context and in the analysis of the EU, we enter into the field of *new political economy*, where political processes, institutions and public choices in the broadest possible sense are being *endogenized*. As we stick to the standard economic paradigm and its means of analysis, we do not join the research project of those (e.g., Chang 2002) who interpret the inclusion of institutional analysis as an *alternative, rather than a complement*, to modern economic approaches. Their line of thought constitutes, at the end of the day, a return of pre-modern approaches such as the German historical school, where the economy was seen as a mere *subsection* of a much broader superstructure, dominated by culture and power considerations as well as morals and geographic factors. By contrast, we join the current academic majority in treating the economy as a *subject on its own right*, following the logic of its own, thus being liable to analytical investigations by means developed over the past 150 years or so. Thus, while being distinct to the 'institutional void' of technicist orthodoxy, we also reject – on theoretical and methodological grounds to be elaborated in the next chapter – any reintegration attempts of the competing 'old institutionalist' streams, including the culturalist school, strongly represented in the area studies and international relations literature.

Our study of institutions follows the mainstream economic logic and aims at *creating a productive feedback to it*, rather than at undermining or fundamentally revising its basic insights. These usually reflect broad and generalizable considerations that are formulated at a much higher level of abstraction.⁸ They represent the *common thread* over and above the individual observation (and case study level, overcoming coincidental and other ad hoc factors). What we hope to provide by the current analysis is two sorts of insights:

- a) *policy applications* on particular fields that normally lie outside the focus of mainstream analysis; and
- b) *generalizable insights* that may verify or falsify theoretical claims that were established in the mainstream literature.

Both endeavors may prove productive and open-ended. Time will tell whether a Kuhnian *paradigm shift*, or a mere – rather conventional – *incremental change*,

⁸ This is the well-known argument of Oliver Williamson (2000) explaining that institutions act at a different level of abstraction and analysis than the study of elementary facts, the focus of neo-classical analysis. This differentiation was widely known in the Hungarian economics of the interwar period, and formed the backbone of the economic concept of the leading academic economist of

which incorporates institutionally and historically informed approaches in the mainstream, will take place in the coming decades. What seems to be beyond doubt is that neither a return to story-telling narratives in the name of cultural and historical peculiarities, nor timeless, rigid, mechanic frame void of history, institutions and human beings, nor the customary disregard of advances in the natural sciences, in quest for methodological rigor developed particularly by the new classical and new Keynesian schools, is sustainable and productive⁹ in the long run.

For us, the eight major issue areas seem to form an *integrated topic* with mutual interrelationships and mutually reinforcing consequences. However, we also accept that their diversity does not yet allow forming a tight structure that would be required of a proper conventional monograph. While every effort is made to ensure the dovetailing of various chapters, the outcome, by necessity, can only be partial. Some chapters are, due to the complexity of the subject, somewhat longer than others, which would be inadmissible even in a graduate level textbook. This is reflected in retaining the word *Studies* in the subtitle. In turn, most of the individual chapters might well be read and used separately, also for reading lists for various graduate level and PhD courses. On occasions, the digressions are mostly confined to the numerous footnotes, which reflect the controversies of fine print, as well as complementary, rather than fundamental pieces of information. These can be omitted, at least in the first overall reading to capture the *major line of the argument*. Also, with the passage of time, while using the text in classroom, current commentary and most recent statistics can and should complement the arguments introduced in the main body of the text.

ACKNOWLEDGEMENTS

I have presented previous versions of each chapter to several academic audiences and received critical comments, which I attempted to address and integrate in the final version of the text. Those contributing particularly to individual chapters are mentioned by name at the respective places. As far as the entire project is concerned, my special thanks are due to my four most diligent commentators providing regular and very critical feedback: Dr. hab. Dóra Gyórfy, Assistant Professor at the Department of Economics, University of Debrecen, as well as Prof. Julius Horvath, Chair of the Economics Department, the Central European University, and József Móczár, Professor of Mathematical Economics at Corvinus University of Budapest as well as my defending PhD student Deniz B. Mc-

⁹ Productive, in this sense, means the same as in the natural sciences, i.e., coming to previously not known (original) insights, finding new methods and being able to test those empirically. Moreover, in a best case scenario the entire exercise yields results that help to improve practical problems, in our case of economic nature, yielding a better quality life for individuals, communities and humankind in a sustainable fashion.

Donald who commented on various versions in terms of structure, substance and presentation.

The book could not have been conceived without the generous support of two of my schools, the Central European University and the Corvinus University of Budapest, both having granted me a year of sabbatical leave starting in September 2007. The then Head of the Department of International Relations and European Studies, Associate Professor Béla Greskovits and the long time Chair of the Department of Comparative Economics, Professor Balázs Hámori, President of the Hungarian Association for New Institutional Economics, both deserve my special thanks. Active members of the Committee on Economics of the Hungarian Academy of Sciences, an academic body I was honored to chair for two cycles in 2002–2008, have helped, through continuous exchanges and criticism to formulate and finalize many of my ideas.

Last but not at all least, the continuous support of my family must be appreciated. The love and devotion of my wife, Csia (Gabi), as well as of our son, Zoltán, and our daughter, Orsi (Hedgehog), have been a fountain of lasting inspiration during this challenging period of academic and public life.

Budapest–Debrecen, 30 September, 2008.

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II. CRISIS IN ECONOMICS?¹

"God put macroeconomists on earth
not to propose and test elegant theories,
but to solve practical problems."

Gregory Mankiw (2006: 29)

This chapter discusses four main issue areas. *First*, through reverse problematization it addresses the *disconnect* between global economic issues and modern economic theory that has evolved around a definite and reductionist methodology, based on assumptions that no longer hold in light of the new insights in the natural sciences. *Second*, it tries to develop an understanding of how and why methodological exclusionism in the mainstream has emerged. *Third*, it attempts to forecast if a Kuhnian *paradigm shift* is in the making through the *gradual revitalization of historically and institutionally informed analyses*. Finally *fourth*, whether a *moderate and co-operative approach and the ensuing methodological pluralism* could be suggested, basically following the nightposts of Nobel winners in economics,² complemented with the output of the leading journals of the profession. Being a leader implies first tradition, second being the forum for the publication of the most frequently cited contributions to economics, third, being the forum of publishing the ideas of school molding personalities over the past five to seven decades, when contemporary economics has emerged. In checking our propositions, the two journals of the American Economic Association, specifically devoted to covering the entire field and orienting end-users, i.e., the *Journal of Economic Literature* and the even more end-user oriented and thus educational *Journal of Economic Perspectives* serve as our points of reference.

¹ This is a substantially revised and extended English version of the inaugural lecture to the Hungarian Academy of Sciences, Budapest, 8 March 2008. Useful comments by Professors György Bazsa (Chemistry), Rezső Lovas (Nuclear Physics), Huricihan Islamoglu (Economic History) and Katalin Szabó (Comparative Economics) on previous drafts are appreciated, with the usual caveats. A shorter version of the chapter appeared in: *Zeitschrift für Staats- und Europawissenschaften* vol. 7. no.1. pp. 4–31.

² As the longtime Chairman of the joint selection committee for the Swedish Academy and the Riksbank (Lindbeck 1985: 46) explained, their "suggestions tend to be concentrated on the well-known and highly respected economists, particularly scholars in the field of 'central' economic theory as traditionally understood". Furthermore, they award "in particular originality of the contribution, its scientific and practical importance, and its impact on scientific work and society in general, such as government policies" (ibid. 50).