

"From the 'Rescue of the Nation State' to the Emergence of European Spaces"

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1.Introduction: a Wish List from a Polisci-European studies scholar to the Historical Research

The aim of this paper is to make some contribution to the discussion on the future direction of the historical research on the European integration processes. I myself is not a historian, a political scientist specialised in Comparative and European Politics, and not in a position to make an inherent suggestion or a project declaration for further research. What I intend to do here is, broadly, to link the contemporary European studies to the Historical studies, and concretely, to emphasise the transnational dimension of European integration[1]. This exercise is not for criticizing current historiography on Europe with all the social scientists' arrogance. Rather, following comments and proposals should be read as presenting my "wish list" to the Integration Historians.

Here, it may be better to make my research interest clear, for following arguments are more or less coming from concerns and difficulties I have had during my own empirical research. Recently, I have been engaged in the research project on the European social policy, with some historical or developmental perspective covering the entire post WWII period. This means, first, that my concern is on the "soft" aspects of integration, most recently symbolised by the Open Method of Coordination. Second, social policy is a policy area where "pure" political consideration does not suffice and economic and sociological factors should be included. Third, social policy is a cornerstone of the "postwar settlement" or the "postwar regimes", whose integration has involved many actors other than governmental and political.

In the following pages, I first make some conceptual consideration and propose my first, methodological, "wish". Then, I propose a research wish list composed of three substantial items, namely, anatomy of transnationality, aligning policy history and institutional history, and the relationship between the postwar regimes and European Integration in general.

2.Conceptual Issues: Integration, Construction or Convergence?

First of all, some conceptual issues should be cleared. One aspect is about the "scope" of research. Most works have been done with the focus on the ECSC-EEC-EC-EU development, namely the current EU institutions and their predecessors. Some others try to keep

their perspective broader, and include other institutions like the Council of Europe, the OEEC (OECD) in the research. Endo (2008) puts what they call the "EU-NATO-CE" regime in the centre of description, and tries to illuminate changing relationships and dynamics among those institutions, which is generally successful. But this is not the issue I am raising here.

Rather, I would like to ask, which process, which effect we have been studying and we should investigate. Our research object is usually called "European Integration" and its history. Endo (2008) also appreciates a French expression "*construction de l'Europe*", or construction of Europe. But do those keywords cover the processes what we are interested in, entirely and properly? Not necessarily, I suppose.

Here, my contention is based on a trend in the current European politics research. One of the main research agendas in the political science has been the so-called 'Europeanisation' research paradigm for these ten years (Featherstone and Radaelli 2003). This paradigm seeks to find out what effect the European Integration project has brought about on the policy and politics in each member states. After an initial excitement and a flood of papers and dissertations, many researchers in the field have found various methodological difficulties. The immediate one is that the hypotheses do not work as expected. And it is also said that only to explore the so-called "downloading" from Brussels is not enough and we should investigate uploading at the same time. It is rather an obvious point for the Historical research and there is no problem.

Further issues are, however, more troubling. When we talk about the effect of European Integration, we usually have a *gross* impact in our mind. But it is problematic methodologically. Those *gross* changes might have occurred without Integration, influenced by the other factors like globalisation. We should somehow measure the *net* effect of Integration (Verdier and Breen 2001). This issue may be easier for historians to tackle, for they are more accustomed to identify complex environments of the central actors and their perceptions, then clarify why and how a specific policy or action was made. Handling of complex issues in concrete historical situation is one of the historians' comparative advantages, I believe.

But how about unintended consequences of an environmental change? More concretely, we have difficulty in the cases where clear approximation or harmonization of policy is occurring, but there is no trace of imposition or even learning from and via Europe. Can this be called Europeanisation or European Integration?

Take an example of pension reform. Since 1980s, many European countries have begun to remodel their old age pension system into the three layered one, composed of the compulsory, the occupational and the private layers. This is so even in Germany, where traditional Bismarckian social insurance model has been firmly entrenched. Sweden, long famous for its tax-funded public pension scheme, now introduced a private pension scheme. The EU

has been also active in this field. It publishes reports and policy statements for a sustainable pension scheme and encourage learning from other countries. If we take these changes at their face value, it is easy to say the Europeanisation or Integration is in process. But how about Japan? It is also moving in the direction of the three-layered scheme, by the introduction of the common minimum pension layer, overcoming traditional occupational division. Then, is Japan in a process of Europeanisation? Obviously, not. True, the Japanese government has learned the lessons of pension reform in Europe, but it is rather a rational response to the environmental change. What is issue here is that approximation and harmonisation is possible even without overarching institutional frameworks or explicit agreements, and how to grasp and characterise it.

Therefore, some scholars have broadened their perspective and now engage themselves in the "convergence" research (Knill 2005; Holzinger, Jörgens and Knill 2007). The keywords there are "transfer", "diffusion", "learning" et cetera, which are trying to describe horizontal, mutual effect among the nation states. The fact that there are various types of "convergence" and it may happen even without mutual influence is also discussed. Echoing this, Gloom (2005) distinguish three processes, namely coordination, harmonisation and convergence.

So, my first "wish" to the historians is to pay more attention to the horizontal dimension among the Member States (or even beyond). Historiography on European Integration has done much in regard to the European Institution building or the policy making processes toward the European Institutions at the member country level. But there are more about the making of a "Europe". Therefore I put the words like "emergence" and "space" in the title of this paper. What I mean by "emergence" is to investigate not only intentional and purposive rational processes but also approximation and harmonisation as a consequence of unintended actions and responses to the environment. By "space", I want to emphasise that not only institutions but also shared norms, policy paradigms or perceptions could sometimes matter [2].

Interestingly, some historians are using the concept of "Europeanisation" but try to explore the field which overlaps my suggestion above. The "Europeanisation History Network" is composed of historians mainly from Britain (majority in Oxford) and Germany, who declares;

By focusing on developments of transfer and exchange, emulation and delimitation, the concept of (de)Europeanisation moves beyond national and comparative history. Building on existing notions of Europeanisation, it will advance historians' ability to analyze continuity and change, convergence and delimitation in modern Europe (<http://www.europeanisation.org/>).

Their research topics include "the Europeanisation of Economics and the Economics of

Europeanisation", which deals with community of economists, "Europeanisation through Culture: 'European Music' in the Twentieth Century", or "Memory and (de)Europeanisation - the Holocaust as a European 'lieu de mémoire'". We may further add the recent surge of interest in "transnational history" to that current (Dülffer 2005; Hoffmann 2003; Ther 2003).

As is already clear from the research topics above, the "emergence" of a European space is closely linked with "transfer" or "diffusion", which usually need a "carrier" of an idea or a "channel" of transaction. Here, we are already entering my second topic.

3. Anatomy of transnationality

To analyse the horizontal dimension of Integration, one of the most obvious ways is to investigate the role of transnational actors. Theoretically, such transnational actors as industrial groups, trade unions and interest lobbyists has occupied a centerpiece since the work of Haas (2004 [1958]), whose activity has been attracted attention of political scientists (Greenwood 2007; Amiya-Nakada 2004). Historians have also directed their attention to those actors as European trade unions (Pasture 2001; 2005; Rumpf 2001; Suzuki 2007. cf. also Dølvik 1997) and transnational European party networks (Kaiser 2007; Mittag 2006). Although there are some inherent difficulties for such research, especially lacking systematic holding of historical sources (in comparison with the research on governmental policies), such research will no doubt be advanced in the near future.

Still, there are some lacunae in the research. First, even the supranational institutions, which have been a focus of scholarly interest, have not explored fully. Although several historical works are now illuminating several aspects of internal working the European institutions (Varsori 2006), we do not have a clear picture of each institution.

For example, in analysing the role of the DG bureaucrats in the social policy formulation, I try to find out career patterns of the bureaucrats, based on the Annuals and the Organigramms of the Commission (Amiya-Nakada 2008). Now it is clear who was where during which period, but I do not have a measure against which my findings should be evaluated. Even as for the nomination procedure or personnel decision concerning the director-general, I could not find clear pattern. For example, Jean Degimbe, a veteran in the Commission recruited by the Roger Raynaud, served for sixteen years. But the next two Directors, served only for five and six years, respectively, although It is highly possible that Degimbe is an exception. Concerning career background, Degimbe and the other two Directors are internal promotion within the Commision, but the successor of Degimbe, very influential Alan Larsson, had been Swedish Minister of Finance before coming to Brussels. The Next Direct-

or, Odile Quintin, had spent most years in the DG Employment before promotion, but the current Director, Nikolaus van der Pas, had no previous experience with the DG employment. Further down the hierarchy, it is more than unclear who comes to Brussels when and why. Some join the Commission early and advance internally, the others come sideways from the Member States ministries to relatively high-level posts of the Commission. In the latter cases, was it a personal decision or was s/he sent to the Commission by their "home" governments?

As far as the Commissioners are concerned, there is a study analysing their composition and background (Döring 2007), but the overall pattern of recruitment in the Commission is yet to be investigated. Historical research has appropriate approaches to this issue, biographical study which concentrates on a specific person and describe various circumstances and dynamics influencing her/his life, or prosopography.

As for the ECJ Judges, Alter (2001) uses the personal ambition of (national) judges to be sent to Luxembourg as a part of explanatory variables for specific judgements. The personnel decision on a Commissioner is sometimes reported in the national press, but that on an ECJ Judge is hardly a news. But potential influence of a Judge may be greater than that of a Commissioner, for there is no European Institution which can directly intervene in the specific case. Now we know much about creative interpretations of the ECJ, which is sometimes issued against the will of the Council or the Commission. So, the composition of the ECJ Judges is as important as the US Supreme Court Judges and deserves much attention.

This research topic, personnel policy of supranational Institutions, relates to my second point of this section. "Socialisation" is a term frequently used in recent European studies, especially those ones standing on the constructivism and emphasising learning (Egeberg 1999; Trondel, Marcusson and Veggeland 2005). These studies, explicitly or implicitly against inter-governmental explanation which take the governmental position fixed beforehand, investigate the perceptions of the supranational bureaucrats from various countries or the national bureaucrats sent to Brussels as a member of the delegates.

If such "socialising" effect is negligible, we do not have to talk about "Brussels bureaucrats", for all there is in the Commission is just negotiation games played by the Member States. To the contrary, If the socialising effect have an enduring impact, not only on the supranational bureaucrats, but also on the national delegates, it has further implications. In this case, the more the bureaucrats acquire experiences of work in Brussels, the further their perceptions and preferences are "Europeanised". As a result, the "preference" of a given Member State may be subject to change in the long run. This socializing effect is especially important when we deal with transnational aspects of Integration or the so-called "soft mode of governance", represented by the OMC.

My third point in this section is an extension of this perspective. Hartmut Kaelble has recently stressed the existence of "European public sphere," which has direct political implications for the "democratic deficit" debate. One uniqueness of his (and his students') research project is to treat various transnational networks of bureaucrats and policy experts as "public sphere" and trace them back into the beginning of the twentieth century (Kaelble, Kirsch and Schmidt-Gernig 2002; Kaelble 2002; 2004). Then, who is the member of the community in a given period and a given policy area, and what is the relationship between those transnational networks and the building of the European Institutions? How the Institutions facilitates the networking and community building?

Further, a more difficult question will be when and how such transnational public sphere of experts exercises an influence on actual political decisions? At least from the political science perspective, there is little use just saying "there is such unique thing as transnational network" without identifying its independent effect on a specific decision or the general development of Integration. Historical research can identify specific cases and periods where those transnational "public sphere" did matter, I hope.

In sum, I have argued in this section that transnational actors and transnational relations of policy makers should be analysed more intensively. This is not only about studying the history of the European Trade Union Confederation or the European People's Party, but also about making clear the transnational effects of the supranational institutions and the transnational connections of the national actors.

4. Aligning Institutional History and Policy History

As is already discernible in the discussion of the previous section, study of transnational relationship, especially that of bureaucrats and experts, has rather narrow focus on a specific policy area, although there are often broader background assumptions or theories behind. But the centre of the Integration Historiography has been always occupied by the story of building the European Institutions and the related drama of intergovernmental negotiations.

Such an academic inclination is only natural and justifiable given the importance of the European institutions themselves. In conducting the EU-politics analysis, however, lack of historically grounded and primary-sources based solid history of respective policy domain is regrettable. Moreover, the lack of solid historical knowledge sometimes leads to very much stereotyped, although not totally wrong, interpretations.

For example, in the usual description of the "European Social Model" discourse, the

1993 White Paper on Growth, Competitiveness and Employment is often cited as the origin of the discourse. I do not deny its importance as an important benchmark for the policy debate thereafter. But, after I have checked all the presidency conclusions of the European Council meetings and important social policy Communications since 1993, I found several details to be added to this usual narrative. First, it is true that the word "Model" itself appeared for the first time in this White Paper. But, the word then disappeared from the policy papers and the presidency conclusions and came back again after 1995. Second, policy instruments stressed in the papers are different from the current ones. In the mid-1990s, among the principal instruments were work-sharing and shorter working-time. Current emphasis on external flexibility or flexicurity, which means easier hire and fire with support of income protection, is of more recent origin. Third, mid-1990s papers always stressed interdependence of the macro and the micro aspects.

But I'm not completely sure about my findings, for the history of the European social policy is yet to be written. Geyer (2000) is often used for the description of the general development of European social policy, and more recent works like Johnson (2005) and Wendler (2005) are also valuable. But they are all written by the social science scholars. I only hope that solid history of European social policy would be written, from which I can start further research. There may be other policy areas with long history and awaits exploration by the Integration historians.

Policy history is also well suited for historical research beyond intergovernmental negotiations. In a specific policy area and under a specific circumstance, the working of the transnational networks of politicians and the "expert public sphere" would be illuminated more clearly, and policy convergence outside the European institutions could be included in the investigations.

A further interesting issue is the relationship between the institutional and policy histories. Is there isomorphism working and have the institutional changes rather directly affected the policy development? Or are they rather independent and does each policy domain develop autonomous dynamics unaffected by the political struggles over the Institutions? This issue has two important implications.

First, if the institutional and the policy level are independent, it is highly probable that the Member State governments do not or cannot co-ordinate their policies at the European level. In this case, there is no such thing as the "European policy" of a Member State but a bundle of sectoral policies made by each national Ministries and negotiated at the European level. In other words, prominent issues like supranationalism versus inter-governmentalism, or the relative strength of the Parliament, the Council and the Commission might be just superficial. Real life of the people might be affected more by the comitology and other negoti-

ations with in the policy community. This question is not theoretical but empirical one, to be answered with temporal and policy specification.

Second, one of the most famous quotes in the EU studies says more than thirty years ago;

"Several blind men approached an elephant and each touched the animal in an effort to discover what the beast looked like. Each blind man, however, touched a different part of the large animal, and each concluded that the elephant had the appearance of the part he had touched. [...] The total result was that no man arrived at a very accurate description of the elephant (Puchara 1972).

This "blind men" metaphor is repeatedly used to criticise the predicament of the Integration studies.

What if there is actually no elephant, like the painting of the Emperor Rudolf II by Giuseppe Arcimboldo.? The "EU" might be just a construction, and only policies might be real. This is a real possibility, at least seen from the current state of affairs, and theoretical engagement is already beginning. For example, John P. McCormick, scholar of normative political theory, characterise the EU as a *Sektoralstaat* or a state composed of sectors (McCormick 2007), and Philippe C. Schmitter devised a word *Condominio* in exploring the future shape of the EU (Schmitter 1996). The latter refers to the situation in which each policy sector has different membership, and some states join most frameworks but others few.

Therefore, I ask Integration historians to try to align policy histories and institutional histories more closely, for issues with theoretical and immediate implications are embedded there.

5.A Milwardian Perspective Lost?: Postwar Politico-Economic Settlement and Integration

My final point is rather broad one. The Integration historiography has advanced our understanding greatly, on the concrete political considerations of each national governments and their interactions under specific circumstances. Endo (2008) has done a very good job in this regard and lifted up high the level of historical narrative on the Integration processes in Japan.

But how about more broad relationship between internal and external dimension of the "rescued" European nation states? If what Endo calls the "Milwardian agenda" is just about showing that the intention of the national governments were rescuing the nation state and European integration has been advanced as a result of such self-preserving decisions, it is safe to say that the work is well done in Endo (2008). But it is another thing whether the European nation states have really needed Integration for rescue. In other words, how far the

so-called "postwar settlement" or "Keynesian welfare state" had depended on Integration? Aren't the Marshall Plan or the Breton Woods Regime more important for the "rescue"? This is what I call here the "Milwardian perspective lost."

We have to remember that the domestic settlement within each nation-state and the reconstruction of the European international order were concurrent processes. It is not that they consolidated their domestic regimes first and then stepped in the European arena to negotiate the European order. Of course, we have many detailed accounts of interdependence or interpenetration of domestic and international politics in the monograph, especially concerning the postwar reconstruction period. The question I am asking here from an actual concern is what has been the concrete relationship between the variety of postwar politico-economic regimes in Western European nation-states and the actual trajectory of Integration.

To highlight current difficulty of social policy co-ordination at the EU-level, Scharpf (2002) refers to the effort made by Guy Mollet, then French Prime Minister, during the Rome Treaty negotiations and his failure to harmonise social policy. After that, the national welfare states expanded and consolidated their own national "regimes", which makes current co-ordination difficult. Some German economists or law scholars highly appreciate the Rome treaty as an embodiment of *Ordoliberalismus*, which corresponded to the German domestic politico-economic regime of the 1950s. According to a story of this line, the West German government, especially its Economic Minister Ludwig Erhard, has successfully installed economic order with stress on market competition, almost unnoticed by other governments (Mestmäcker 1993). These are stories told with clear political intentions. Such narrative should be revised, based on solid historical research.

But not all the burden should be borne by the Integration historians. At least, the same amount of responsibility should be put on the shoulder of scholars of domestic history. In spite of the growing amount of historical works dealing with the postwar era, most of them are concentrated on the social and cultural aspects of the postwar order. Actual working of the postwar political economy, its diachronic change, and its overall characterisation is not elucidated. This is further exacerbated by the recent development in political science, moving away from history and leaning toward politometrics and game theory. As a result, only a few scholars have an interest in the investigation of the postwar regimes.

Recently, it is often lamented that real party competition was lost or it is the time of "post-democracy" (Crouch 2004). But such misgivings are too often based on *cliches* about good old days and on no grounded knowledge. It is more than questionable that we had a real democratic choice, which we do not have now. Just remember the fact that the famous article "waning of opposition" was published in 1957 (Kirchheimer 1957)!

We should know more about the postwar regimes, the world where we lived in. In this

regard, it is interesting to note that Martin Conway, an Oxford Historian, is advancing the historical research of the postwar democratic regimes. Within the framework of the "Europeanisation History Network" mentioned above, his project is on "Discourses of Democracy in Western Europe 1947-73: Cause or Symptom of (de)Europeanisation?" (<http://www.europeanisation.org/for/conway.html>). The focus on discourse is a promising way, for we already have interesting works on the inter-war periods from discourse perspective in the Integration Historiography, which deal with both the domestic and the European dimensions (Sick 2002; 2003; Wegeman 2003).

To summarise this section, what we need now is to analyse the co-evolution of the domestic and the international regimes in Europe simultaneously. It will shed new light on the diagnosis of the current situation and the future prospects of the European Integration.

6. Conclusion

These are my wish list to the Integration historiography. It is summarised as follows;

1. more attention to the transnational actors and relations,
2. more policy histories and their alignment with the main-stream Integration history, and
3. true "European" history dealing with the interdependence or the co-evolution of the domestic and the international aspects.

It is disputable if political science has something to offer to the historical research, but I'm sure that Integration historians are standing on a fertile ground and have much to offer. I'm really expecting it.

Notes

[1] In this regard, I have the same concern with Wolfram Kaiser, a German Integration historian working in Britain, although from the opposite direction. It is recommended to consult his various review articles for more historically grounded arguments (Kaiser 2005; 2006; 2008).

[2] As examples of works employing the "space" metaphor, see Delanty and Rumford (2005) and Misa and Schot (2005).

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