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Non-Events in Macro-Comparative Social Research Why We Should Care and How We Can Analyze Them

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Abstract: This paper addresses the role of non-events in macro-comparative social research. Non-events are defined as critical junctures during which actors do not alter the policy path although the counterfactual case of policy change was a likely possibility. Macro-comparative researchers often overlook non-events. The reason for this is simple. Critical junctures are often identified on the basis of change that took place during these critical junctures. In contrast, there is no approach to identify critical junctures in the absence of change. Thus, although more or less explicit decisions to not change policy paths can be very informative, comparative researchers overlook these non-events because they do not realize that there were considerable possibilities for change during these critical junctures. In this paper, we discuss the role of non-events in macro-comparative social research. First, we explain how counterfactual theorizing can be used to integrate non-events in explanatory statements. Second, we demonstrate, using an example from our own research, how the consideration of non-events can advance our knowledge. Finally, we suggest a procedure, which can be used to analyze non-events and which is based on the combined usage of fsQCA to identify not-consistent cases, process tracing to determine the relevant critical junctures and disciplined counterfactual theorizing to probe whether change was really a possibility.

1 Introduction

This paper addresses the role of non-events in macro-comparative social research. Following Elster (2007: 9), the main task of the social sciences is to explain social phenomena, i.e. events, using other events. Thus, we attempt to explain why something happened by citing an earlier event as its cause. For instance, somebody might explain the outbreak of World War I by pointing to the assassination of Archduke Franz Ferdinand in Sarajevo. Both the outbreak and the assassination are observable phenomena and the causal claim is that the earlier event (assassination) caused the subsequent event (outbreak of war).¹

However, sometimes, we are more concerned with *non*-events. For instance, why did Switzerland not introduce an old-age pension scheme before World War II? An expert on the historical development of old-age policies in Switzerland would know that a first act, the so-called Lex Schulthess, had been rejected in a popular vote in 1931. Thus, the reason for the fact that Switzerland did *not* have a public old-age pension scheme before World War II is the rejection of the Lex Schulthess in a popular vote.

Of course, rejecting an implementation act in a popular vote is an *event*: the conscious decision to not implement an old-age pension scheme. Consequently, such an explanation does not violate Elster's request to explain events by citing earlier events. However, from today's point of view, the rejection of the old-age pensions schemes can be understood as a non-event. The implementation was a real possibility, but it did *not* happen.² Thus, non-events are a subset of the set of events, i.e. events that did not lead to change.

Non-events can be very important. For instance, Obinger (1998) considers Switzerland a welfare state laggard because most social insurance schemes have been introduced relatively late. However, had the voters not rejected the Lex Schulthess, Switzerland would have introduced an old-age pension scheme much earlier. Moreover, the 1931 draft law envisioned in fact a quite different pension scheme than the one accepted in 1946. Finally, the Swiss government in fact anticipated a potential rejection, when it began discussing the creation of

an old-age pension scheme in the 1880s (Pellegrini 2002: 297). Thus, rather than a welfare state laggard, Switzerland could have been a welfare state pioneer. This conclusion does not change the fact that Switzerland *is* a welfare state laggard, but it calls the *inevitability* of this outcome and some of the conclusions we base on it into question.

Non-events may be defined as critical junctures during which the policy path is not changed although the counterfactual case of policy change was a likely possibility. They become interesting when the absence of change is either rare because all other cases in the scope experienced change (e.g. not enacting a reform all other countries have enacted in this period), or theoretically surprising given that change was to be theoretically expected (e.g. not enacting a reform even though the reform is supported by an overwhelming majority of the population).³ Thus, to reiterate the above example, the failed attempt (non-event) to introduce an old-age pension scheme in Switzerland before World War II is interesting because its failure was in fact quite rare. All surrounding countries introduced an old-age pension scheme before 1931 (Germany in 1889, Austria in 1906, France in 1910 and Italy in 1919, see Obinger 1998: 14). In this particular case, the analysis of the non-event has brought to light the role of a set of particular political institutions that characterizes Switzerland (direct democracy and federalism).

Macro-comparative researchers often overlook non-events. The reason for this is simple. Critical junctures are often identified on the basis of changes that *took place* during these critical junctures. In contrast, there is no approach to identify critical junctures in the absence of change. Thus, although more or less explicit decisions to *not* change policy paths can be extremely informative and important, comparative researchers often miss them because they do not realize that there were considerable possibilities for change in this particular period. This is an unfortunate situation because the usage of counterfactual theorizing could inform us about what could have been had a certain event occurred and what has caused the attempt to fail.

As Lebow (2010: 8) notes, the social sciences suffer from a ‘hindsight bias’. We tend to upgrade the probability of events once they have occurred (‘it could not have come differently’), while we consider the future to be highly contingent (‘who knows what the future will bring’).⁴ Thereby, we ignore the fact that when crucial decisions in the past have been made, decision-makers faced considerable uncertainty. However, “once we know what has happened, it is difficult to recall how unsure we used to be about the future” (Tetlock et al. 2006: 3). Thus, while there might be good reasons for the decisions that have been made, we have to acknowledge that policy-makers could have decided differently. Lebow (2010: 10) reminds us that “if major historical developments are so inevitable, [...] the underlying conditions responsible for these events should have been apparent at the time to scholars and policymakers alike, making them – although not their timing and specific expression – to some degree predictable.” However, research has shown that we are not very good at making predictions about future events (Almond and Genco 1977; Tetlock 2005). Thus, we need to realize that things could have turned out differently, and by reflecting about these alternative worlds, we might learn something about the world we live in.

Reflecting about alternative scenarios is no free pass to engage in undisciplined speculation. The goal is not to speculate about how the world would look like if only Cleopatra’s nose had been shorter (and Marc Antony less attracted to her).⁵ Rather, counterfactuals should be used to “probe the causes and contingency of the world we know” (Lebow 2010: 6). Was the given outcome inevitable, very likely or could it have easily turned out quite differently? Whether the assassination of Archduke Franz Ferdinand in Sarajevo caused World War I is an open question, which is intensively discussed in the literature (Lebow 2007; Schroeder 2007; Thompson 2007), but it is *beyond doubt* thinkable that the assassination could have failed.⁶ Considering the importance of World War I, it is interesting to know whether the assassination was the cause of World War I (Lebow 2007) or whether war would have been unavoidable even in the absence of the assassination (Schroeder 2007).

In any case, without disciplined counterfactual theorizing there is no way we can answer this question (Fogel 1964; Fearon 1991: 474-84; King et al. 1994: 76-82). In fact, all causal inference from history ultimately rests on counterfactual claims about what could have been in hypothetical worlds (Lebow 2010: 137).

The goals of this paper are both modest and ambitious. First, we want to make a case for the importance of non-events. We argue that non-events are often overlooked, but potentially very important sources of information. Second and more ambitiously, we suggest a procedure that can be used to detect and analyze non-events in macro-comparative social research. This procedure is based on the combined usage of fsQCA (Ragin 2000) to identify not-consistent cases, process tracing (George and Bennett 2005) to determine the relevant critical juncture (that did not lead to change) and counterfactual theorizing (Levy 2008) to discipline our reflections about ‘what could have been’ given small changes in the antecedents.

The procedure is illustrated using an example from our own research (Emmenegger 2010ab), which focuses on the historical development of job security regulations in Denmark. We demonstrate how the consideration of non-events can advance our knowledge. As this discussion shows, the disregard of a critical juncture in the years before the first oil price crisis led scholars to misunderstand the historical development of job security regulations in Denmark and, consequently, the development of the popular ‘flexicurity’ model.

2 Why care about non-events?

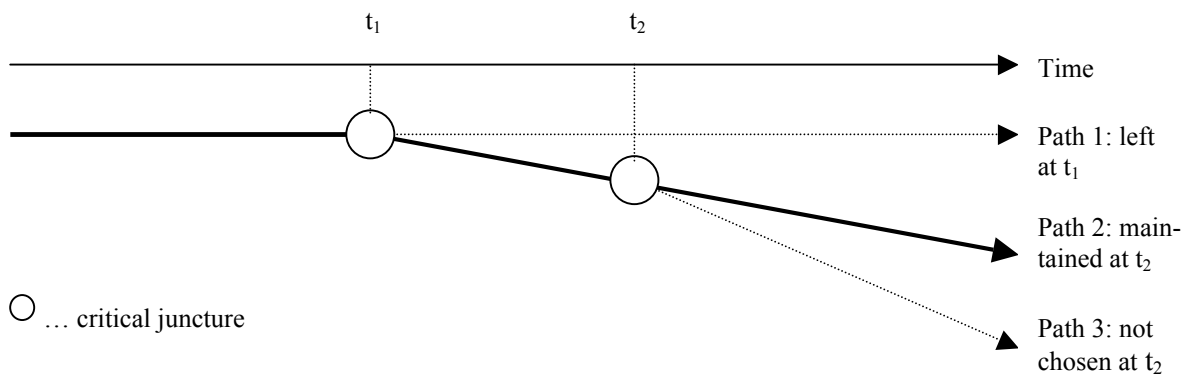
In this section, we discuss the role of non-events in explanatory statements. We define non-events as critical junctures during which the policy path is not changed although the counterfactual case of policy change was a likely possibility.⁷ This understanding of critical junctures implies that one can reach a choice point and decide to stay on the path (Capoccia and Kelemen 2007: 352). This differs from Collier and Collier’s (1991: 29, emphasis added) account, which stresses change: “A critical juncture may be defined as a *period of significant*

change, which typically occurs in distinct ways in different countries (or in other units of analysis) and which is hypothesized to produce distinct legacies”; or Slater and Simmons’s (2010: 888, emphasis added) definition, which defines critical junctures as “periods in history when the presence or absence of a specified causal force pushes multiple cases onto divergent long-term pathways, or pushes a single case onto *a new political trajectory that diverges significantly from the old.*”

In contrast, we understand critical junctures as situations during which significant policy change is *possible*. Thus, we use the concept of critical junctures to demarcate the relatively short historical period during which an established path may be left and a new path may come into existence. Following Capoccia and Kelemen’s (2007: 348), we understand critical junctures as “relatively short periods of time during which there is a substantially heightened probability that agents’ choices will affect the outcome of interest.” Thus, during critical junctures, political actors are able to exert considerable influence on institutional developments (Katznelson 2003). A critical juncture is then often followed by a period of self-reinforcement and institutional stability (Mahoney 2000: 514).

Figure 1 illustrates a possible non-event in a simple setting, assuming change only during critical junctures in a developmental process, which is otherwise characterized by path dependency. In Figure 1, a political unit follows a path 1 up to the critical juncture at t_1 . At t_1 , path 1 is left and the political unit enters path 2. Such a development can be described as a path departure. After t_1 , the political unit follows path 2. At t_2 , the political unit is again entering a critical juncture. However, this time, we can observe no path departure. Rather, the political unit remains on path 2, although the political unit *could have left* path 2 and entered path 3 (counterfactual case). For reasons that should not concern us here, the political unit did not enter path 3 despite the possibility to do so. This development at t_2 is referred to as non-event.

Figure 1 *Events and non-events in macro-comparative social research*



If we trace the development of policies within one country, it is quite natural to focus on critical junctures and important changes. If we rely on a ‘branching worlds theory’ (Elster 1978: 180), we can analyze each branching point and analyze the new and the old policy paths as well as counterfactual paths. However, as highlighted by Lukes (1980: 151) in his discussion of Elster’s contribution to counterfactual theorizing, we often do not know the branching points. If these critical junctures (branching points) are quite obvious, we normally incorporate them into the analysis. For instance, Thelen (2004) has discussed the lack of change in German vocational training after World War II as compared to the situation before World War II. World War II is an obvious critical juncture. The fact that relatively little has changed with regard to vocational training has spurred many insights. However, not all critical junctures are so obvious.

As highlighted by Capoccia and Kelemen (2007: 352), change is not a necessary characteristic of critical junctures. Rather, developments during a critical juncture may lead to the reinstatement of the status quo ex ante. In terms of Figure 1, this would mean that the political unit does not leave path 2 at t_2 although the possibility of path 3 exists. But if the political unit does not leave the path, i.e. if there is no change, how can we identify the critical juncture at t_2 ?

According to Capoccia and Kelemen (2007: 343), many researchers treat critical junctures as ‘deus ex machina’. Put differently, change induces us to assume the existence of a critical juncture. We do not start from the identification of critical junctures and then carefully evaluate whether change has taken place. They write: “Most researchers, following Pierson’s advice ‘to go back and look’, trace the roots of institutional change back to the origins of a ‘path’. We certainly do not disagree with this approach but contend that, if used exclusively, it overlooks the fact that some critical junctures may result in re-equilibration of an institution” (Capoccia and Kelemen 2007: 352).

Focusing on change instead of critical junctures biases our findings. Take the example of wars. If we focus exclusively on wars that took place and on a very small set of famous international crises that almost led to war (e.g. the Cuban missile crisis), we would struggle to learn much about the causes of war. Most importantly, we would be unable to define our population of cases (Mahoney and Goertz 2004).

Similarly, macro-comparative research often suffer from a hindsight bias because researchers tend to treat the real world as the only possible outcome (Fischhoff 1975; Tetlock 2005; Lebow 2010), rather than acknowledging that some things might have turned out quite differently. For instance, welfare state researchers often consider Switzerland a welfare state laggard; among others because a majority rejected the proposal for the implementation of an old-age pension scheme in a popular vote in 1931. However, an even bigger majority accepted the 1946 proposal, which led to the implementation of a quite different old-age pension scheme. What has changed during these 15 years?

Leimgruber (2008) has shown that rather than focusing on political institutions such as direct democracy, researchers should concentrate on the design of the reforms. More precisely, he has highlighted the role of the public-private mix, i.e. the interplay between the private pension funds, which were providing pensions to a considerable share of the working population, and public schemes, which seemed to threaten the viability of private schemes.

From this point of view, it might be not surprising to hear that numerous voters rejected the 1931 act because it challenged their existing private pension funds, while the 1946 act did not face such criticism. The important point here is that the voters might have accepted a differently designed act already in 1931.⁸

Once we accept the important role of non-events, we face a new challenge. How can we identify non-events? And how can we make sure that the analysis of non-events does not turn into ‘anything goes’? In the following section, we answer this question by suggesting an approach that can be used to detect and analyze non-events in macro-comparative social research. We then demonstrate this procedure using the case of job security regulations in Denmark.

3 How to analyze non-events

In the following, we discuss an approach that helps detecting and analyzing non-events. The procedure is based on three steps:⁹

1. Use fsQCA to identify cases, which are characterized by the absence of a given outcome (not-consistent case), despite the presence of configurations of conditions that almost always lead to the presence of this outcome. We argue that in such situations a non-event *might* be the cause of the absence of the outcome of interest.
2. Pair this not-consistent case with a consistent case (i.e., a case characterized by the same configuration of conditions as identified by the fsQCA analysis *and* the presence of the outcome of interest) in a most-similar systems design. The comparative analysis of the historical development of the outcome of interest in the consistent and the not-consistent case through process tracing can inform us about the existence of critical junctures that led to change in one case (consistent case), but to no change in the other case (not-consistent case). The critical juncture in the not-consistent case indicates the possible existence of a non-event.

3. Use counterfactual theorizing to check whether the hypothesized event (the counter-to-fact) was indeed possible in the not-consistent case. As the universe of imaginable (but not necessarily reasonable) counterfactuals is infinite, criteria for good counterfactuals are needed in order to discipline our counterfactual theorizing and to help establishing whether the counterfactual was a likely possibility. Only when an event was indeed possible, does it make sense to speak of a non-event.

In this procedure, the consistent case plays four important roles. First, the consistent case helps identifying the time when and place where we have to look for a potential non-event in the not-consistent case. Second, the consistent case serves as an illustration of how the process might have unfolded in the not-consistent case. Third, the consistent case illustrates that an alternative development was indeed possible. Finally, a comparison of the consistent and the not-consistent case helps identifying the crucial differences that may help explaining the unequal outcome.

To illustrate the procedure, we discuss a finding from our own research on the historical development of flexicurity in Denmark (Emmenegger 2010a). Liberal hiring and firing rules are one of the three pillars of the Danish version of the flexicurity model, as are active labour market policies and generous unemployment insurance systems. But while we know quite a bit about the historical development of the other two pillars of the Danish flexicurity model (e.g. Nørgaard 1997; Larsen 2004), there is little research on the historical development of job security regulations. The reason for this is straightforward. The flexicurity strategy is based on generous unemployment benefits, training-and-employment-based active labour market policies and liberal hiring and firing rules. Thus, when analyzing the historical development of flexicurity, researchers tend to focus on how unemployment insurance systems got generous and how labour market policies got activating, but rarely on the question why job security regulations *remained* liberal.

The few studies that address the issue of the development of job security regulations in Denmark have highlighted three arguments (Scheuer 1998; Madsen 1999, 2005; Estevez-Abe et al. 2001). First, it is argued that the tradition of liberal hiring and firing rules goes back to the 1899 September Agreement. Thus, it is easy to fire people in Denmark because it has always been like that. That is an odd argument because in 1899, there were no serious restrictions on hiring and firing in any Western democracy. However, all Western democracies have experienced significant change with regard to job security regulations since 1899. In some countries, these changes led to very restrictive hiring and firing rules (e.g. Sweden), while in others the rules remained rather liberal (e.g. Denmark).

Second, several studies have argued that there is a trade-off between unemployment insurance generosity and strictness of job security regulations. Employees are compensated for low levels of job security regulations by high levels of unemployment benefits. This argument is based on what we like to call ‘grand design’ assumptions. It is assumed that when reforming unemployment insurance and job security regulations, all relevant political actors, independent of political colour, had the ‘big picture’ in mind and took reforms in other labour market policy areas into account. From the perspective of a historically informed political science, these are heroic assumptions. Moreover, this thesis is not supported by cross-case empirical evidence. Table 1 shows the bivariate correlation between the average of the replacements rates for an average production worker living alone and a family household with one main earner (type average production worker), a dependent spouse and two children in the period 1998 to 2002, and the indicator of employment protection legislation in the same period. Following the trade-off hypothesis, we would expect a negative relationship. However, as the evidence presented in Table 1 shows, we can observe a rather strong positive correlation. Only when we remove the liberal market economies from the sample, the correlation turns negative (but remains insignificant).

Table 1 Determinants of job security regulations: bivariate correlations I

| Independent variable | Dependent variable | Expected relationship | Correlation | p-value (two-tailed) | No. of obs. |
|------------------------------------------------------------|-------------------------------------------------------|-----------------------|-------------|----------------------|-------------|
| <i>Unemployment insurance generosity (1998-2002)</i> | <i>Employment protection legislation (1998-2002)</i> | - | + 0.405 | 0.095 | 18 |
| <i>Unemployment insurance generosity (1998-2002)</i> | <i>EPL (1998-2002) excl. liberal market economies</i> | - | - 0.255 | 0.423 | 12 |
| <i>Predominance of small and medium-sized firms (2002)</i> | <i>Employment protection legislation (2002)</i> | - | + 0.186 | 0.474 | 17 |

Notes: The analysis covers 18 countries (Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom and the United States of America. Data are from Scruggs (2004) and the OECD (2004, 2006). No data on economic structure for Canada available. Source: Emmenegger (2010a).

Finally, some authors have pointed to the predominance of small and medium-sized enterprises in Denmark to explain the low levels of job security regulations. Relying on theoretical arguments from dual labour market theory (Doeringer and Piore 1971), these authors argue that, per definition, small firms have to rely more on external labour markets. As they lack the capacity to adjust to exogenous demand shocks by redeploying redundant workers, small firms are more concerned with regulatory social policy's consequences for non-wage labour costs (Mares 2003). In contrast, large firms have a greater capacity to shift an increase in non-wage labour costs onto consumers in the form of higher prices. Again, this thesis strongly relies on an assumption of long-term system-level rationality. It is assumed that these social and economic actors know what they want, and that they and their political allies are powerful enough to implement their vision and strategy.

Table 1 shows again the bivariate correlation between Western democracies' economic structure and the corresponding levels of job security regulations in 2002. The indicator we use for operationalizing economic structure is the share of employees working in firms with less than twenty employees (as a percentage of total employment). Thus, if the 'firm size hypothesis' is correct, we would again expect a negative relationship. However, as Table 1 shows, we are observing a positive bivariate correlation.

Of course, a relationship between unemployment insurance benefits and the economic structure, on the one hand, and the level of job security regulations, on the other hand, cannot be ruled out on the basis of bivariate correlations. However, as the above analysis has shown, there is no straightforward association. In fact, we argue that all three explanations suffer from problems associated with non-events. The first explanation does not take into account crucial developments during the last 110 years. In terms of Figure 1, these scholars focus only on the critical juncture at t_1 , the 1899 September Agreement, and subsequently highlight path dependency. They do not allow for the possibility of some critical juncture (at time t_2), which was crucial for the future development of job security regulations (although it did not result in a path departure).

The other two approaches take contemporary institutions and assume – relying on a functionalist logic – that these institutions reflect the preferences of policy designers. They do not allow for inertia, path dependency, unintended consequences or political failure. Furthermore, they do not allow for non-events, that is, critical junctures during which no change took place. However, as our own account of the historical development of job security regulations in Denmark illustrates, it is exactly a non-event that tells us the most about the historical development of job security regulations in Denmark.

Accepting the importance of non-events, we are confronted with one important problem: How do we identify critical junctures in the absence of change? In the following, we present a procedure to identify and analyze critical junctures in cross-case settings. This approach relies on an inter-group comparison using fsQCA and an intra-group comparison using process tracing.

fsQCA (Ragin 2000) is based on set-theoretic relations and focuses on explicit connections between conditions (configurations). Unlike the commonly used correlation methods, which aim at the identification of tendential relationships ('net effects'), fsQCA attempts to discover configurations of conditions. As Ragin (2000: 66) notes: "By grouping

cases into a relatively small number of configurations of attributes, the researcher establishes a basis for specifying different ‘kinds’ of cases. In this way, the researcher can understand types of cases as different configurations of attributes.” Using fsQCA, researchers can identify whether certain combinations of conditions always or almost always lead to a certain outcome. In this way, fsQCA helps discovering relationships, understood in Humean terms as regularities.

However, association is not causation, as it is well known. Therefore, the results of the formal fsQCA are but a midpoint in the analysis. They need to be complemented by a qualitative discussion of the different paths. These discussions are to show whether the observed regularities indeed represent causal relationships. Thereby, the identified paths offer an analytical structure for the case-level interpretations. Thus, the formal fsQCA analysis does not substitute for the case studies “just as reading a detailed map is not a substitute for taking a hike in the mountains” (Ragin 2000: 283).

The case discussions are simplified by the fact that the fsQCA analysis allocates cases to paths. Put differently, the formal fsQCA analysis identifies paths leading to the outcome and allocates cases to the paths they are most likely to follow. However, fsQCA can also be used to identify cases, which do not correspond to the expected patterns, i.e. cases that are *not* characterized by the presence of the outcome despite the presence of configurations of conditions that almost always lead to the presence of the outcome. In QCA language, these cases are often referred to as contradictory cases because they do not show the expected outcome despite the presence of a configuration of conditions normally associated with the outcome of interest. While the existence of contradictory cases is sometimes considered a problem, we argue that they can also be considered a source of information because they force us to ask the question of *why* they do not exhibit the expected outcome. Quite possible, a non-event is the cause for the difference in outcomes.

Thus, we suggest to use fsQCA to identify not-consistent cases and to subsequently match these not-consistent cases with consistent cases. Thereby, we follow the recommendation by Ragin (2000: 60) and select not-consistent cases, possibly caused by non-events, in a way that they resemble consistent cases in as many ways as possible. However, rather than matching consistent (showing the expected outcome) and not-consistent cases (not showing the expected outcome) on *all possible* conditions, we restrict our focus to those conditions that have been identified by the fsQCA analysis as relevant.

An example may illustrate this procedure. Imagine an fsQCA analysis with four conditions (left party strength, union strength, corporatism, homogeneity) and the outcome ‘generous welfare state’. The analysis shows that the combination ‘strong left parties’ and ‘corporatism’ is associated with generous welfare states. This association can be observed in six countries. However, imagine that there is one country that is characterized by strong left parties and corporatism, but not a generous welfare state. This observation may lead us to conclude that our formal fsQCA analysis lacks an important condition. However, we may also conclude that there might be something very particular about this country, for instance, a critical juncture that did not lead to the expected change (non-event).

In our analysis of the determinants of high levels of job security regulations in Western democracies, we have started with five conditions, which had been previously identified as important determinants of job security regulations in the scholarly literature. These factors are (1) Catholicism (Esping-Andersen 1999) (2) a statist political economy tradition (Bonoli 2003), (3) a high level of coordination in a production regime (Estevez-Abe et al. 2001), (4) a powerful labour movement (Rueda 2005) and (5) few institutional veto points (Immergut 1992). Please consult Emmenegger (2010b) for a discussion of the hypotheses and the calibration of the conditions. Table 2 shows the fuzzy set scores for all five conditions and the outcome.

Table 2 Fuzzy set scores for five conditions and the outcome

| Country | Statist political economy | High level of non-market coordination | Strong labour movement | Catholic | Many institutional veto points | Restrictive job security regulations |
|--------------|---------------------------|---------------------------------------|------------------------|----------|--------------------------------|--------------------------------------|
| Abbreviation | S | C | L | R | V | JSR |
| Australia | 0.00 | 0.00 | 0.57 | 0.20 | 1.00 | 0.14 |
| Austria | 0.67 | 1.00 | 0.57 | 1.00 | 0.67 | 0.71 |
| Belgium | 1.00 | 0.67 | 0.43 | 1.00 | 0.67 | 0.57 |
| Canada | 0.00 | 0.00 | 0.14 | 0.20 | 1.00 | 0.14 |
| Denmark | 0.00 | 0.67 | 0.86 | 0.40 | 0.00 | 0.29 |
| Finland | 0.67 | 1.00 | 0.71 | 0.40 | 0.00 | 0.43 |
| France | 1.00 | 0.33 | 0.43 | 1.00 | 0.33 | 0.71 |
| Germany | 1.00 | 1.00 | 0.43 | 0.60 | 1.00 | 0.86 |
| Ireland | 0.33 | 0.00 | 0.14 | 1.00 | 0.00 | 0.29 |
| Italy | 1.00 | 0.33 | 0.57 | 1.00 | 0.33 | 0.86 |
| Netherlands | 0.00 | 0.67 | 0.29 | 0.20 | 0.67 | 0.71 |
| New Zealand | 0.67 | 0.00 | 0.29 | 0.00 | 0.00 | 0.14 |
| Norway | 0.00 | 1.00 | 1.00 | 0.40 | 0.00 | 0.86 |
| Portugal | 1.00 | 0.00 | 0.43 | 1.00 | 0.00 | 1 |
| Spain | 1.00 | 0.00 | 0.71 | 1.00 | 0.33 | 1 |
| Sweden | 0.00 | 1.00 | 1.00 | 0.40 | 0.33 | 0.86 |
| Switzerland | 0.00 | 0.67 | 0.14 | 0.20 | 1.00 | 0.29 |
| UK | 1.00 | 0.00 | 0.29 | 0.00 | 0.00 | 0.14 |
| USA | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 | 0 |

Note: Data taken from Emmenegger (2010b). For the purpose of simplification, the condition ‘strong religious parties’ has been omitted. Moreover, the fuzzy set for Spain on institutional veto points has been adjusted from 0.67 to 0.33 (see Emmenegger [2010b] for a discussion).

The result of the formal fsQCA analysis based on the data in Table 2 is displayed in Table 3. We will not discuss the analysis and the result in any detail here (see Emmenegger 2010b for a discussion).¹⁰ What is important in the context of this paper is the fact that the formal fsQCA identifies the configuration $s^*C^*L^*v$ to be generally associated with high levels of job security regulations (see Table 3). However, an inspection of the data displayed in Table 2 shows that Denmark – despite being characterized by a non-statist political economy tradition, few institutional veto points, a high level of coordination in the production regime and a strong labour movement – is *not* marked by high levels of job security regulations. Thus, Denmark is a not-consistent case. Assuming that our fsQCA analysis is correct, we would expect high levels of job security regulations in Denmark. As a result, we

need to address the question of why Denmark is not characterized by high levels of job security regulations.

Table 3 Analysis of sufficient conditions for the outcome high levels of job security regulations

| | | | | | | | |
|------------------|--------------|---|--------------|---|----------------|---|------------|
| Solution: | S*C*R | + | S*R*v | + | s*C*L*v | → | JSR |
| Consistency: | 0.97 | | 0.99 | | 0.84 | | |
| Raw coverage: | 0.44 | | 0.29 | | 0.28 | | |
| Unique coverage: | 0.26 | | 0.12 | | 0.21 | | |

Overall consistency: 0.92 – overall coverage: 0.77.

Note: The consistency threshold has been set at 0.87. The next highest score is 0.72. Source: Emmenegger (2010b)

The fsQCA analysis has helped us identifying a not-consistent case, but how do we find this potential non-event? If they only thing we know about the historical development of job security regulations in Denmark is the fact that the 1899 September Agreement did not restrict the employers’ right to hire and fire, then we are facing the proverbial search for the needle in a haystack. As a result, we need some help in getting ideas about where to look for non-events. We suggest using the fsQCA analysis as a starting point.

The fsQCA analysis provides us with the possibility to match cases that are as similar as possible on all *relevant* conditions. As can be seen in Table 2, Sweden is – like Denmark – characterized by a non-statist political economy tradition, high levels of non-market coordination, strong labour movements and few institutional veto points. However, unlike Denmark, Swedish workers enjoy high levels of protection against dismissals. Therefore, Sweden is a consistent case because it features both the conditions expected to lead to the outcome *and* the outcome. Thus, we have now two cases that are very similar with regard to the identified relevant conditions, but differ with regard to the outcome.

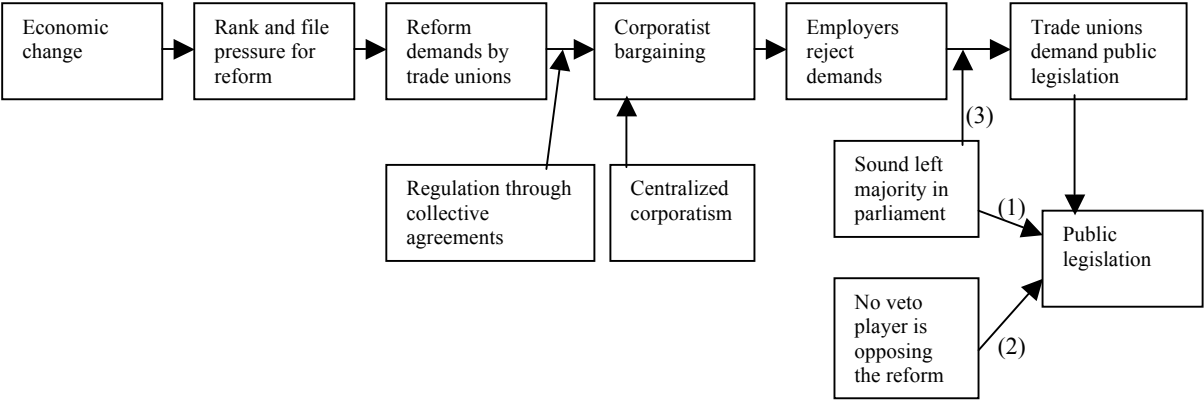
In a second step, we thus suggest to systematically compare the historical development of job security regulations in the consistent (Sweden) and the not-consistent case (Denmark) using process tracing. This systematic comparison can help us identify the critical junctures during which the paths of Denmark and Sweden diverged. Although such a comparison need not necessarily lead to the identification of a non-event, it is our best chance for finding something that did not happen.

For illustrative purposes, we use a causal narrative, which validates cross-case associations by splitting conditions into constituent sequences of disaggregated events and comparing these sequences across cases, and by illustrating these sequences using an event-structure diagram (Mahoney 2003). Below, we quickly summarize the historical development of job security regulations in Denmark and Sweden (see Emmenegger 2010a for a detailed discussion). In brief, we argue that the counterfactual of an enactment of a law on job security was a likely possibility in Denmark during the critical juncture 1966 to 1974, thus emulating the Swedish development, but that for reasons outlined below, the Danish labour movement failed to get tougher restrictions on hiring and firing enacted.

In Sweden, job security was regulated by means of collective agreements with rather low levels of job security regulations. In the course of the 1960s, dissatisfaction rose in Sweden among the trade union rank-and-file. As a result of economic restructuring and the increasing occurrence of dual-earner households, trade union members started to push trade unions to demand more job security regulations. However, the employers' associations proved unaccommodating. As a result, the Swedish blue-collar trade union federation (LO) turned to the social democratic government and demanded legislation. In 1971 the government established a labour law commission and in 1974 the Employment Protection Act was passed. The 1974 Employment Protection Act is important for two reasons. First, the act led to a considerable tightening of job security regulations in Sweden. Second, the act was a substantial departure from the Swedish model of "agreement in preference of legislation"

(Kjellberg 1992: 99). Figure 2 illustrates the developments in Sweden in an event-structure diagram.

Figure 2 Event-structure diagram of the historical development of job security regulations in Sweden (1966-1974)



The Swedish development points to a critical juncture in the years just before the first oil price crisis. Considering the similarity of the Danish development up to this point and the overall similarity of the two cases as established by the fsQCA analysis, we suggest asking the question of whether there might have been a critical juncture in Denmark, too, although one that did not lead to change (non-event). Thus, the combined usage of fsQCA and process tracing helps identifying the time when and place where we have to look for a non-event in the not-consistent case. In the case of the historical development of job security regulations in Denmark and Sweden, the suggested procedure points to the years just before the first oil price crisis and to the labour movement as a driving force.

In fact, case evidence shows that in the late 1960s, as in Sweden, the Danish blue-collar trade union federation started demanding tougher restrictions of hiring and firing. As in Sweden, the social democratic party supported these trade union demands. Moreover, as in Sweden, corporatist negotiations in the late 1960s and early 1970s did not lead to a significant

expansion of job security regulations in collective agreements. Thus, it is possible that Denmark might have entered a critical juncture in the late 1960s and early 1970s.

However, is this indeed a case of a non-event in Denmark? Put differently, was the counterfactual move from a regulation by collective agreements to public legislation and, simultaneously, the tightening of job security regulations *really* a likely possibility during this critical juncture? To answer this question, we need criteria, which help us evaluating counterfactual scenarios. These criteria are needed to discipline our counterfactual theorizing and to guarantee that the suggested counterfactual was indeed a real possibility.

Emmenegger (2010c) has reviewed the methodological literature in international relations for criteria for good counterfactuals. Table 4 provides a summary. Put simply, we need to ask three questions: (1) Could the Danish trade unions have demanded the public legislation of job security regulations (plausibility of the counterfactual antecedent)? (2) Would the Danish government have passed a law on job security (conditional plausibility of the counterfactual consequent)? (3) Are there observable implications (factuals) of this counterfactual argument that can be tested on other data (projectability)?

The Swedish example demonstrates that the move from collective agreements to public legislation could have been an option for the Danish trade union movement. In fact, the Danish trade unions were very well aware of the developments and discussions in Sweden (Eysell 1983: 478-481). Not only did the social democratic parties and trade unions regularly get together, the Danish blue-collar trade union federation (LO) also kept track of developments in most European countries, especially Norway and Sweden (Emmenegger 2010a: 284). Moreover, after the negotiations with the employers' association had failed in early 1973, the communist party and the socialist people's party demanded legislation (Pryds 1997: 74; Eklund Hansen 1998: 67). Within the blue-collar trade union federation the discussion emerged whether to stick to basic agreements or replace the basic agreements with public legislation (Eklund Hansen 1998: 67).

Table 4 Checklist for counterfactual cases

1. **Clarity:** Specify the hypothesized antecedent, the consequent, the connecting principles and additional enabling conditions as clearly as possible
 2. **Plausibility of the antecedent:**
 - a. Make sure that the counterfactual antecedent is logically and historically possible (logical and historical consistency)
 - b. Specify antecedents that require the alteration of as few historical facts as possible (minimal rewrite rule)
 3. **Conditional plausibility of the consequent:**
 - a. The connecting principles should be consistent with well-established theoretical laws (theoretical consistency)
 - b. The connecting principles should be consistent with well-established statistical generalizations (statistical consistency)
 - c. Minimize the number of causal steps between the counterfactual antecedent and the counterfactual consequent (proximity rule)
 - d. Acknowledge the interconnectedness of events and consider the effects of second-order counterfactuals
 4. **Projectability:** Formulate other implications which can be tested against data
-

Source: Fearon (1991, 1996), Tetlock and Belkin (1996), Lebow (2000), Levy (2008) and Emmenegger (2010c).

In terms of power resources, there were two windows of opportunity for the enactment of laws regulating job security. From November 1966 to January 1968 and again from September 1971 to December 1973, social democratic minority governments, supported by the socialist people's party and some independents, governed the country. Together, they had a majority in parliament. Moreover, both parties supported a tighter regulation of hiring and firing (Emmenegger 2010a: 284). However, as long as the trade unions did not demand public legislation, the parliament is unlikely to pass a corresponding law, as the Swedish case demonstrates. Still, the political majorities were there. Considering the very good relationship between the blue-collar trade union federation and the social democratic party, the common goal of tougher job security regulations and the composition of the Danish parliament, it is very likely that the parliament would have passed an act on job security.

Turning to the criteria for evaluating counterfactuals, we can conclude that the above counterfactual is logically consistent. It needs only very limited rewriting of history. The

causal mechanism is short and simple as well as theoretically and statistically consistent. The Swedish example shows that such a course of events was indeed possible. Of course, as in Sweden, the relationship between the social partners and the Danish government would have suffered (second-order counterfactual). However, cross-case evidence and the Swedish example show that, once passed, the retrenchment of job security regulations is very difficult (Bonoli and Emmenegger 2010). It is very likely that, had the parliament passed such an act, job security regulations would have remained more rigid in Denmark than they are today.

Thus, we argue that the developments in Denmark can be described as a non-event. In terms of Figure 1, both Denmark and Sweden experienced an important path departure at the turn of the century (t_1 in Figure 1). This is often highlighted in accounts on the historical development of job security regulations (1899 September Agreement in Denmark, 1906 December Agreement in Sweden). At t_2 (early 1970s), Sweden experienced another path departure, moving from collective agreements to public legislation (path 3). In contrast, Denmark could have moved to public legislation but did not. As a result, Denmark remained on path 2. This development, we argue, should be described as a non-event.

Why did Denmark experience a non-event? Put differently, why did the Danish blue-collar trade union federation not demand the public legislation of job security regulations? There are several reasons for this. First, the Danish social democratic party, although very powerful in comparative perspective, is not as powerful as its Swedish counterpart. During the two windows of opportunity, the left coalition was constantly bothered by conflicts within and between parties. The first government collapsed because some parliamentarians left the socialist people's party to launch the left socialists, which did not support a social democratic government. Similarly, the second government collapsed because some parliamentarians left the social democratic party to create the centre democrats. Thus, despite majorities in the parliament, internal fragmentation caused by intra-coalition conflicts decreased the

governments' reform capabilities considerably (Emmenegger 2010a). In Figure 2, arrows 1 (fewer power resources) and 2 (presence of veto players) illustrate this difference.

Second, when the Swedish trade union movement started to demand public legislation, the Danish blue-collar trade union federation was still bargaining with the employers' association. Only in spring 1973 did it become clear that no extensive restrictions would be possible by means of collective agreements. However, here it is important to keep in mind that collective bargaining in the corporatist arena always takes place in the shadow of the political arena (Scharpf 1997). While the Swedish LO could rely on the social democrats as a very powerful partner, the Danish LO was not so lucky (Strøby Jensen 2002: 80-81). This fact, clearly visible after the failure of the first red-red coalition government in 1967, weakened the bargaining position of the trade unions in Denmark. Thus, it is very likely that the fragility of the red-red coalition governments was anticipated by the trade union federation and induced them to put more emphasis on the corporatist arena. In Figure 2, arrow 3 illustrates this difference.

With regard to projectability (see Table 4), the above discussion implies that rather than focusing on economic structure and a possible trade-off between unemployment insurance generosity and the level of job security regulations, we should look at the electoral strength of left *and far-left* parties in the period prior to the first oil price crisis. In these years, a 'red wave' (Mjøset 1987: 420) swept over Western democracies and in all countries a radicalized labour movement demanded the tightening of job security regulations. If we can generalize from the Danish and Swedish cases, then we should observe a positive relationship between the strength of left and far-left parties in this period and the level of job security regulations today.

Table 5 displays the bivariate correlation between the level of job security regulations measured as the average of the OECD's (2004) EPL indicator in the period 1985 to 2003 and the electoral strength of left and far-left parties in the period 1966 to 1974. As can be seen, the

two variables correlate strongly. If, however, we omit the electoral strength of far-left parties and focus exclusively on left (social democratic) parties, the correlation ceases to be significantly different from zero. Thus, this finding supports our argument that in countries in which left and far-left parties were not able to successfully collaborate in the period prior to the first oil price crisis (such as Denmark) lower levels of job security regulations can be observed.

Table 5 *Determinants of job security regulations: bivariate correlations II*

| Independent variable | Dependent variable | Expected relationship | Correlation | p-value (two-tailed) | No. of obs. |
|-----------------------------------------------------------------|------------------------------------------------------|------------------------------|--------------------|-----------------------------|--------------------|
| <i>Electoral strength left and far-left parties (1966-1974)</i> | <i>Employment protection legislation (1985-2003)</i> | + | + 0.49 | 0.038 | 18 |
| <i>Electoral strength left parties (1966-1974)</i> | <i>Employment protection legislation (1985-2003)</i> | + | + 0.12 | 0.631 | 18 |

Notes: The analysis covers 18 countries (Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom and the United States of America). Data are from Armington et al. (2008) and the OECD (2004).

As this example illustrates, non-events can be as informative as actual policy changes. However, when analyzing non-events, it is imperative to ensure that the used counterfactual case allows for causal statements. More precisely, the counterfactual case has to satisfy the following criteria: First, the hypothesized antecedent and consequent have to be specified. Second, the plausibility of the antecedent has to be demonstrated. Third, the conditional plausibility of the consequent has to be shown. Finally, further implications of the causal argument should – if possible – be formulated and tested against new data.

4 Conclusions

This paper agrees with Pierson’s (2004) advice ‘to go back and look’ but it raises the question of ‘where to look?’ Tracing the roots of institutional change back to its origins is

certainly helpful, but it comes at the risk of losing sight of events that could have led to change, but ultimately did not. In this paper, we suggest calling these events *non-events*. Not all non-events have a lasting influence on the way of the world, but some non-events have been extremely influential. If the Danish labour movement had managed to enforce stricter job security regulations in the years prior to the first oil price crisis, we might not be talking of flexicurity today. If not for the left's failure in the early 1970s, Denmark would not be an exception among the Scandinavian countries. Rather, like its Norwegian and Swedish brethren, Danish workers could not be easily fired (although some researchers might be out of work, or at least be looking for a new research theme).

Looking for non-events is the first step, finding them the second. In this paper, we have suggested a procedure that can be used to detect and analyze non-events. It is based on the combined usage of fsQCA to identify not-consistent cases, process tracing to determine the relevant critical junctures and counterfactual theorizing to discipline our thinking about 'what could have been' given small changes in the antecedents. We argue that minimum requirements for counterfactual theorizing are of particular importance for this kind of analysis in order to safeguard us from the 'anything goes' problem. Put differently, strict rules are needed in order to make sure that the non-event at hand is indeed a non-event. If we cannot convincingly show that the counterfactual event would have been possible, there is no use speaking of a non-event.

How important are non-events in macro-comparative social research? It is our firm belief that comparative researchers have missed many important non-events, especially in areas that so far have been neglected by the social sciences. Because macro-comparative social research often relies on secondary literature, we are often able to identify non-events in well-documented areas. Just take the example on the introduction of an old-age pension scheme in Switzerland discussed in the introduction. In other areas, such as the historical development of job security regulations in Western democracies, non-events are sometimes very difficult to

identify. However, as the discussion of the historical development of job security regulations in Denmark and Sweden has illustrated, the identification and analysis of non-events holds great potential to advance our knowledge.

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Endnotes:

¹ Next to events, the social sciences often use facts in explanatory statements. However, according to Elster (2007: 9), events should be preferred. The fact ‘the road was slippery’ might be used to explain why a person lies in the hospital. However, it would be more precise to use the event ‘the car went off the road’ to explain why a person lies in the hospital (after all, most cars do not go off the road).

² There is an important distinction between failing to attempt and failed attempts (Elster 2007: 9). Only the latter can be described as non-events. However, if a conscious decision to not attempt precedes the failure to attempt, the decision can be considered an event.

³ A simple way of thinking of an event is a specific value on a variable. A non-event is then a continuation of particular value on a variable, even though the probability of a value change was very high. We are grateful to James Mahoney for suggesting this illustration.

⁴ The current economic crisis is a good example of this hindsight bias. How many did see it coming? And how many are now arguing that the crisis was inevitable?

⁵ Although often cited as a ‘miracle counterfactual’ that makes implausible changes in reality, the original statement, expressed by the French philosopher and mathematician Blaise Pascal, referred to the length of the nose as a symbol of her strength of character (a popular belief in the 17th century) rather than as a sign of Cleopatra’s beauty. URL: http://penelope.uchicago.edu/~grout/encyclopaedia_romana/miscellanea/cleopatra/bust.html (access July 14, 2010).

⁶ The list of reasons why the assassination could have failed is in fact impressive: The Archduke had been warned about the dangers of visiting Sarajevo; his wife suggested to cancel the visit; there were fewer bodyguards during the procession than originally planned; the procession could have been cancelled after the first attack on Franz Ferdinand’s life had failed; there was a misunderstanding with regard to the route with the two cars before Franz Ferdinand (including the police guard) turning into the wrong street while Franz Ferdinand’s

car remained on the correct street; the policeman next to the assassin could have been able to stop him; the assassin could have simply missed Franz Ferdinand; and the assassin could have obeyed the order, which had been sent to him by the military conspirators in Belgrade, and aborted the mission (Lebow 2010: 86-87).

⁷ Note that our understanding of non-events differs from what Mahoney and Goertz (2004) call ‘negative cases’ insofar as they are concerned with the definition of the population of cases and the establishment of causality in terms of covering laws/constant conjunction, while we are concerned with the identification of not-consistent cases and the establishment of causality in terms of individual cases. See Goertz and Levy (2007: 10-15) for an excellent discussion of the distinction between these two understandings and the role of counterfactuals therein.

⁸ Of course, this raises numerous interesting questions. For example, why was the 1931 act designed the way it was?

⁹ It goes without saying that the suggested procedure is strongly influenced by the work of Ragin (1987, 2000).

¹⁰ The data has been analyzed using the software fsQCA 2.5 (Ragin et al. 2009). We set the consistency threshold in the truth table at 0.87. The next highest score is 0.72. The solution corresponds to the ‘intermediate solution (Ragin and Sonnett 2005). For a detailed discussion of the procedure, please consult Ragin (2008) or the detailed analysis in Emmenegger (2010b).