The Study of Social Risks from the Perspective of their Collectivization and Privatization. Methodological and Conceptual Reflections.

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Abstract

The present paper discusses several methodological and conceptual issues confronting the study of recent welfare reforms from the vantage point of the ‘privatization of risk’. Looking mainly at the current American scholarly debate, it points at the lack of an effective measure of overall economic security and insecurity, and interprets it as a consequence of ‘risk’ being partly a social construction. This leads to reviewing two incumbent approaches to the study of risk: the economic-rationalist strand, which assesses risks with statistics, and the sociological strand, which focuses on the social processes selecting the goals on whose basis risks themselves are defined. Both, it is argued, need to be taken into account for a sensible analysis of the evolution of the welfare policy and politics. Whereas the rationalist school offers a sound analysis of the problems experienced by the beneficiaries and sponsors of welfare programs and of their potential preferences, the sociological school is better equipped to study the politics of risk collectivization/privatization, which by no means consists of a public debate on the optimal distribution of ‘volatility’. The paper concludes by presenting the approach adopted in my dissertation, in order to analyze the changing distribution of retirement risks in Italy, Germany, and the US.
Introduction

Over the last thirty years, most advanced industrial democracies have profoundly revised the social contract embodied in their post-war ‘welfare state structures’ (Pierson 2001, Gilbert 2002, Taylor-Gooby 2004). Recent scholarly contributions explain this international trend as a result of a new ‘logic of international competition’ as well as of socio-structural transformations and their bearing on electoral dynamics (Taylor-Gooby 2002; Schinkel 2009; Häusermann 2010). Although the noticeable amount of structural change occurred in the last decades has not (maybe not yet) produced policy changes of comparable magnitude, not only expansionary welfare trends are now a thing of the past, but the generosity of individual entitlements has shrunk. In other words, expectations of a sudden fall of social expenditure or of a substantial stability of the old welfare arrangements have both been disappointed, while voters and policy-makers have learnt to cope with the new scenario in ways previously unexpected (Alcock and Craig 2009, Giger 2011).

Even if it is safe to claim that total welfare spending has not decreased as much as conservative intellectuals and politicians campaigned for in the early Eighties, more nuanced analyses have shown that traditional welfare regimes have undergone both quantitative and, most notably, qualitative changes of undisputable magnitude (Esping-Andersen 2009; Palier 2010). A recent and influential stream of literature addressing similar social policy changes interprets them as a process of so-called ‘privatization of risk’ (Hacker 2004, 2006, 2008; Warren 2006; Orenstein 2009; on Italy see Marano and D’Antoni 2008). Namely, the approach suggests, social risks that were previously dealt with collectively are increasingly transferred on individuals and households, through an indirect, subterranean, form of privatization. Not surprisingly, the claim results in a broad research program, addressing (or at least involving) not just adequacy concerns for existing programmes or the emergence of previously unaddressed social risks, but also the changing notion of ‘public responsibility’ and the central role of ‘risk’ in post-industrial societies (Calhoun 2006).

The great relevance and scope of the risk privatization agenda involve, however, the cost of dealing with a much more complex and contentious ‘dependent variable’. Evidence in favour of its basic thesis is commonly provided with survey data collecting individual perceptions of economic security, as well as with ‘hard’ measures of economic wellbeing. Among the latter, one can find various disaggregated per capita spending figures, social security replacement rates (current or foreseen), income mobility and volatility trends, or the likelihood of experiencing income shocks of a given magnitude. But, considering the changing nature of socio-economic risks and the policy trade-offs they pose, this fragmented and sometimes contradictory wealth of figures is unable to point at clear policy implications. Subjective perceptions are generally considered unreliable and
inaccurate, if not biased, while measures are often ambivalent and inevitably dependent on counter-factual reasoning grounded in economic or normative assumptions. Partly as a consequence of this impasse, the risk privatization approach has not (yet) provided comparative political economists with a convincing narrative – not to say an explanation – of recent welfare reforms.

Aim of the present paper is to provide an overview of the current problems in approaching the comparative study of welfare reforms from the risk privatization perspective, focusing on those related with defining the risk shift and then assessing its extent. Four problems seem particularly relevant. First, no standard indicator of risk socialization/privatization has been devised to facilitate its empirical assessment. Second, the most sophisticated figures capturing overall economic insecurity belong to the American scholarly debate and originate from a wealth of data that has little equivalent in the other advanced political economies. Third, indicators of ‘economic insecurity’ are unable to measure risk privatization whenever intended as a risk shift from the public to the private sphere, unless one assumes ‘insecurity’ (or at least any increase thereof) can be fully displaced by public means. Fourth, proxy indicators of risk privatization do not play out in the policy debate in the same way as expenditure levels, the consequence being some kind of hiatus between, on the one hand, the analysis of the risk shift policywise and, on the other, the analysis of the underlying politics. The existence of these four problems, the paper argues, urges for a new ‘qualitative reorientation’ based on the following rationale: a closer focus on single policy domains; a contextualized definition of ‘risk privatization’ calibrated on the specific policy goals of real-world case studies and their variation over time; a framework able to accommodate insights from the sociological study of risk alongside indicators of income volatility.

A very cursory and introductory reflection on the issue, the paper is structured as follows. The first section deals with the first two problems and provides a very short literature review, with particular focus on the recent scholarly debate in the US. The second discusses the last two problems, considering potential solutions by taking a step back and looking at the dichotomy between rationalist and sociological approaches to the study of risk. The third section presents a potential avenue to a ‘qualitative reorientation’ of the debate by discussing some of the conceptual and methodological choices I have made in my thesis. The final section offers, in a provisional conclusion, a reassessment of the main line of argument.

1. How do you recognize a risk shift when you see one?

Although the field of comparative political economy welcomes any kind of methodology, it is fair to say that most researchers on the subject use to start their inquiries with a so-called ‘empirical
puzzle’. Many even start by running a substantial number of cross-tabulations or scatter-plots, filled with (rough) indicators relative to their favoured topic, looking for distributions, trends, or correlations that manifest cross-national variation within a recognizable global pattern. This is indeed the case of most research on the expansion, retrenchment, and (more recently) recalibration of social expenditure: in so doing, various indicators of the national ‘welfare effort’ in specific spending items can, with due care and the due caveats, be visualized and compared, while the most notable trends and outliers are highlighted and eyeballed. When the latter look at least partly at odds with state-of-the-art theories and explanations, the researcher has moved a first, tentative, step in the direction of his/her ‘empirical puzzle’. Questions such as ‘Why expenditure on a given item X is increasing (decreasing) in Y countries (but not in Z)?’ or ‘Why country K, that we know for epitomizing (being resilient to) phenomenon A has assumed this unexpected position/trajectory?’ are common tokens of this category.

Unfortunately, the task of the scholars interested in the privatization of risk is not so straightforward. Contrary to the examples above, it is not (or at least not yet) possible to rely on a set of indicators of choice, acknowledged for their ability to operationalize the risk shift or assess its extent. This is not to say that addressing the issue from a sound empirical standpoint is impossible: many pieces of relevant information can in fact be drawn from the huge wealth of secondary literature already produced to answer different research questions. The implication is instead that the assessment of risk privatization becomes more contentious and interpretative (in sum, less ‘hard’) than the typical measurements of traditional welfare studies. In this section, I will specifically address two aspects of such question. First of all, I discuss recent attempts to quantify the amount of risk privatization and show that no measure of choice and no methodological standard have yet emerged. Second, I focus on the best indicators emerged in the US debate, showing that the chances to replicate them with the data collected in other contexts are, at the present stage, fairly limited.

1.1 Measuring risk privatization/income instability within countries

In the eight years passed since the publication of Jacob Hacker’s influential article on ‘privatizing risks’ (2004), the seemingly intuitive idea of a risk shift (i.e. that risks and responsibilities are being transferred between social actors) has posed harder measurement challenges than initially expected. The persistent lack of a transparent, readily observable, empirical proxy for the concept of risk privatization has provided ground for questioning whether a risk shift has actually taken place (see Winship 2009) or whether the definition of risk shift is a case of ‘concept stretching’ (on the latter see Sartori 1970).
Indeed, as one moves beyond the catchy intuition of a “redistribution of risks” the concept of risk privatization is rather loosely defined. The recent popularity of the topic among academics may suggest its being still ‘unsettled’ (‘quasi-concept’), as it moves between the public and the academic debate.¹ The term ‘privatization of risk’ may have been borrowed from outside political science, namely from epidemiology. In a 2001 article on the American Journal of Public Health, Beverly Rockhill denounced the “privatization of risk (factor)” as a value turn in the field of epidemiology and public health, connected with the development of preventive medicine: a system based on the centrality of the ‘personal risk factor profile’ and the primacy of personal autonomy (and the correction of personal behaviour) rather than the promotion of public intervention (Rockhill 2001).

In turn, Jacob Hacker’s original definition (Hacker 2004, but see also Hacker 2002) was developed within the broader attempt of refining theories of welfare retrenchment commonly associated with the predominant New politics of the Welfare State approach. Advancing a very well known conceptualization of incremental modes of policy change (layering, revision, drift, and conversion) Hacker argued that the literature on retrenchment, with its exclusive emphasis on the budgetary effects of formal policy changes, had ignored processes of ‘bureaucratic disentitlement’ and lack of adaptation to new social risks, thus disregarding the question of adequacy and of the consistency between the current setup of social programmes and their long-standing ambitions. He therefore suggested taking a closer look at policy implementation and outcomes: the latter could then to be traced back to ‘subterranean’ forms of scaling back the welfare state, given that “…in principle, U.S. social policy could have adapted to changing realities.” (Hacker 2004: 251)

In other words, “risk privatization” appeared as a bold theoretical construct, encompassing retrenchment and ‘bureaucratic disentitlement’, apparently minor moves towards marketization (for instance, the establishment of supplementary pensions) and even the inadequate or foregone socialization of new socio-economic risks. The new concept relied heavily on counterfactual reasoning and on very strong normative expectations on further expansions of social expenditure: both the lack of new programmes to address the new challenges of a post-industrial society and the introduction of new programmes, whenever interfering with more traditional ones, could in principle be instances of risk privatization. Rather than being observed through some empirical proxy, whose behaviour could give raise to a classic empirical puzzle (why more or less X than expected?), risk pri-

¹ The introduction of term is also quite recent: searching Thomson Reuters’ Web of Science Citation Database for publications whose topic contains both “privatization” and “risk” returns less than 400 results (certainly including a number of false positives) and shows just one reference before 1992. The average occurrence of a published item is 10 to 15 a year throughout the 1990s and early 2000s, rising to an average of 25 a year since 2004. As far as quotations connected to the coupled terms are concerned, they rise smoothly but steeply from 0 in 1993 to more than 400 in 2011.
vatization has to be traced and ‘understood’ in each specific case before explaining or understanding the causal process behind it. This is, to a large extent, the rationale of Hacker’s historical analysis of the US case and of his interpretation of the conservative ideology promoting the risk shift.

The large scope of the concept, however, is a heavy burden for cross-country comparisons: in principle, the analyst has to carry out an in-depth preliminary assessment of risk privatization in each country, producing a counterfactual reasoning able to assess both the disruptive potential of new welfare schemes and the fiscal room theoretically available for addressing new social risks and demands. Such a complication might explain why, notwithstanding the interest in the topic, the comparative study of risk privatization is far less developed than the study of, say, welfare dualization (Emmenegger et al. 2012) or recalibration (Ferrera et al. 2000; Hemerijck 2006). Further attempts to refine the concept have offered improvements for its quantitative measurement in the US case, rather than sharpening the theoretical framework with a view to the comparative analysis of risk privatization across policies or regimes. The goal, instead, has been to produce a synthetic indicator of the level of economic risk experienced by individuals and families, in order to study its evolution over time.

In his subsequent and widely known book The Great Risk Shift (henceforth GRS), Hacker (2006) restated his approach to the privatization of risk moving from a more traditional empirical puzzle. Looking at indicators of economic instability, Hacker showed an increasing trend in the volatility of household incomes since the early Seventies. The pattern unfolded in a number of ‘instability peaks’ associated with the slowdowns of the early 1970s, 1980s, and 2000s, but also presented a huge spike during the economic expansion of the early-mid 1990s. After each spike, household economic instability was higher than in the preceding period, summing up to an upward secular trend. The latter revealed, according to the author, an increase in the amount of risk carried by private individual and families. Such an increase was meant to show the exhaustion of the old welfare programmes and progressive reduction of their adequacy. A nice empirical puzzle followed from the figure: how could the trend be explained? In line with the counterfactual reasoning above, the process of increasing inadequacy was interpreted as evidence of a risk shift, which was then traced historically in a number of policies (labour market, pension, healthcare, and family policies) and explained as a result of the agency of conservative policy entrepreneurs. This suggested that tracing the volatility of household income over time could provide a satisfactory overarching measure of economic insecurity and a useful proxy of risk privatization.

No doubt, Hacker’s findings entered (and revitalized) the economic instability debate, sparked in the mid 1990s by the work of Peter Gottschalk and Robert Moffitt: two economists interested in
the relation between earnings instability and inequality in the US (Gottschalk and Moffitt 1994; Moffitt and Gottschalk 1995). Over the last five years appeared a new wave of contributions, mostly critical of Hacker’s conclusions about the magnitude of the change occurred in the Nineties. As a result, a revised version of GRS was published in 2008 (presenting figures more in line with the findings of the literature on earnings instability) and a new indicator was elaborated in 2010 by Hacker and a new team of co-authors. While an adequate discussion of the merits and methodological details of this literature vastly exceeds the limits of this paper (see Hacker and Jacobs 2008 as well as Winship 2011 for excellent and very accessible reviews), a quick look at the main points of agreement and disagreement will suffice two substantiate my first two claims that: (1) no standard proxy of risk privatization has been found; (2) the most sophisticated indicators devised so far can hardly travel outside the US.

As summarized by Winship (2009), economic instability can be measured by indicators of:

A) **absolute and relative social mobility** (on individual earnings see: Gittleman and Joyce 1995; Moffitt and Gottschalk 1995; Backer 1997; Daly and Duncan 1997; Fields, Leary and Ok 2000; Haider 2001; CBO 2007; Dynan, Elmendorf, and Sichel 2007; Jensen and Shore 2008; Kopczuc, Saez and Song 2010 ; on household income see also: Duncan, Smeeding and Rodgers 1993; Gottschalk, McLanahan, and Sandefur 1994; Gottschalk and Danziger 1999; Gittleman and Joyce 1999; Gosselin 2008; Gosselin and Zimmerman 2008; Hertz 2006; Carroll, Joulfaian, and Rider 2006; Nichols and Zimmerman 2008);

B) **dispersion of income (or earnings) across and within individuals** (see Moffitt and Gottschalk 2008; Dynan et al. 2008; Shin and Solon 2011; Dynarski and Gruber 1997; Cameron and Tracy 1998; Daly and Duncan 1997; Gosselin 2008);


Indicators of each type have featured prominently in the recent risk privatization debate. In both versions of GRS, the assessment of the extent of the risk shift rests on two main indicators, both depending on strong modelling assumptions. The first is a measure of **across-individuals dispersion of transitory income shocks**: namely, the **volatility of real household income** for individuals aged between 25 and 61 (adjusted for family size). This indicator conceptualizes economic insecurity as the unpredictability of income levels (high or low) and living conditions. Following Gottschalk and Moffitt (1994), the figure is calculated using a simple **variance decomposition**
**model**: total income variance is modelled as the sum of the variances of a permanent and of a transitory income component, which are estimated over a 5-year interval and assumed not to covariate. The transitory component measures instability, whereas the permanent one is meant to control for the long term effect of income inequality.

The second indicator is a measure of **absolute downwards social mobility**: namely, the likelihood of experiencing a 50 per cent or greater drop in real household income. Differently from the volatility estimates above, this indicator looks at economic insecurity as the chance of losing one’s own habitual standard of living. It is calculated as the result of a logistic regression estimating the average individual’s probability of incurring such a loss over a two-year interval, controlling for socio-demographic characteristics (such as the five-year moving average of family income or the exposure to risks such as single motherhood or divorce), individual fixed effects, and a time trend variable.²

Other than forcing a revision of Hacker’s 2006 figures, the debate (see footnote 3 below) on how to measure economic insecurity and/or assess the risk shift called into question the theoretical assumptions and the methodological practices used to produce the indicators above, favouring also a more critical assessment of the pros and cons of the available datasets. While no stable consensus has emerged yet, four points at least are widely acknowledged:

1) Most studies indicate that individual earnings and (individual) family incomes have become more unstable over time, even if less than Hacker suggested, with wide variations across socio-demographic groups, and along a somewhat different pattern. Downward absolute mobility has increased for male earnings during the 1970s, but stabilized since the early 1980s, while consistently decreasing for women over family. Turning to family income, the upward trend in variability has been slightly more pronounced.³

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² The first indicator shows a mild but steady increase along the 80s with a return to mid 1970s levels later in the decade; in the early 1990s, it skyrockets and scores a fivefold increase (before taxes) between 1974 and 1994, swinging back and forth towards its second-highest peak in 2002. Between 1974 and 2002, the figure after (before) taxes doubles (triples) in value. The trend of the second indicator slows down at the end of the 80s only to peak in the early 1990s; later on it experiences a substantial drop and then a reversal to a new high level, close to the historical maximum. The share of individuals experiencing the aforementioned shock rises from above 7 to above 16 per cent between 1970 and 2002. In Hacker 2008, income volatility in 2004 is less than double its 1973 value, with a peak in 1994. Values for the likelihood of a 50 per cent or greater income drop are halved, ranging from 4 to 8 per cent in between 1971 and 2004.

³ The dispersion of earnings changes, measured by changes in the variability of income between one year and the next, has followed countercyclical patterns with a secular increase in the 1970s. Finally, earnings instability, measured by the dispersion of transitory earnings shocks, has increased, with estimated increases in standard deviation ranging between 15 to 50 per cent. Instability grew mildly during the 1970s, accelerated in the first half of the 1980s, stabilized throughout the rest of the decade, moved up again during the 1990s, and assumed a less interpretable trend in the early 2000s.
2) Differences among results depend to some extent on the selection of different datasets. Concerning the first point, income or earnings volatility can only be observed using longitudinal (panel) datasets. Three have emerged as the most widely used: the Current Population Survey (CPS, a quasi panel in which different groups of respondents are rotated over time), the Panel Study of Income Dynamics (PSID, the dataset of choice for most analyses), and the Survey of Income and Program Participation (SIPP, sometimes merged with administrative data from the record of the Social Security Administration). Each presents distinctive pros, cons and methodological challenges (in terms of the representativeness of the sample as well as of the consistency and accuracy of the data) which in turn influence the results in largely predictable ways.4

3) Many differences in the results also depend on operative choices, often connected to the peculiar characteristic of each dataset. Differences in matching (mostly between subsequent CPS waves and between the SIPP and SSA datasets) and trimming strategies (related to different top and bottom coding standards, as well as to the large impact on percentages and logged measures of minimal absolute changes in the lowest incomes) are held accountable for the main inconsistencies in the literature (see Gosselin and Zimmerman 2008, Hacker and Jacobs 2008, and Winship 2011 for discussions, or compare Dahl et al 2010 and Hacker et al 2010 for an example).5 A methodologically weak treatment of very low income observations (together with unaccounted changes in PSID methodology) is commonly called upon to make sense of the abnormal 1992 peak of instability emphasized in the first edition of GRS.

Downwards mobility of family income has increased during the 1970s and, according to some studies, during the 1980s as well; volatility between adjacent years has increased during the 1970s, stabilizing (or moving inconsistently) afterwards. Dispersion of income shocks between the early 70s and the early 2000s is pictured as gradually increasing, rising by 20 to 60 per cent over the period. In general, most of the literature only partially supports the risk shift hypothesis, anticipating most of the increase in economic instability from the 1990s back to the 1970s and early 1980s.4

These features of the datasets are impossible to review within the limits of this paper (see Gosselin and Zimmerman 2008 and Winship 2011 for an in-depth discussion). It will suffice to say that studies based on the CPS and the PSID are more likely to claim that economic instability has increased, with the CPS providing systematically higher levels for instability indicators and the PSID showing an increase in the dispersion of transitory earnings shocks across the last three decades. On the other hand, studies based on the SIPP typically find a flattening of earnings instability after the mid 80s; evidence on income instability is also heavily dependent on data treatment (see CBO 2007; Dahl et al 2010 and Orszag 2008 on the one hand and Hacker et al 2010 and Gosselin and Zimmerman 2008 on the other). Finally, Winship 2011 suggests that correctly addressing CPS and PSID data problems (and minimizing the impact of imputed income values) all datasets converge to the more cautious results of SIPP based studies.

The dilemma posed by trimming is a thorny one. On the one hand, inaccurately reported/recorded observations could distort the analysis introducing abnormal levels of variation in some years (which becomes even more distortive if moving averages are used to smooth the trends). On the other hand, since the income of self-employed workers and small entrepreneurs can often assume very low or even negative values in at least one year, many observations could be deleted, obscuring the economic realities of these occupational groups (nonetheless crucial to assess the aggregate level of economic instability).

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4) Last and most delicate is the issue of the modelling assumptions required to construct and interpret measures of economic instability. As shown by differences in the trends of earnings and income instability (especially when conditioning by socioeconomic group, as in Gosselin 2008) household income dynamics emerge as the result of various compositional effects: composition of families, of education, fertility, and employment patterns (especially for women), and of different income sources. Therefore, when decomposing permanent and transitory variance with Gottshalck and Moffitt’s 1994 model, two highly unrealistic assumptions are required: either every household member is equally affected by the transitory shock, or that income composition remains static.6

All this considered, the attempt to devise an objective measure of economic insecurity, useful as an empirical (and possibly uncontroversial) starting point for the study of the risk shift, has not produced the hoped results. Many uncertainties remain, concerning the true direction and extent of changes in economic instability, as well as their unfolding over time. More to the point, methodological and conceptual difficulties have arisen, casting doubts on the possibility to observe concepts of insecurity and risk without knowing more of the living conditions and the preferences of the observed individuals. When looking at changes in either the likelihood of an income drop and the dispersion of income shocks alike, the problem remains of how to avoid modelling assumptions that translate into unrealistic expectations of stability in presence of demographic or contextual changes (such as age-related income dynamics or the unfolding of the budget cycle).7 Another open question is how to discriminate between voluntary and involuntary variations and foreseen versus unforeseen shocks, in order to distinguish conscious planning and self-insurance attempts from actual economic misfortunes. Unfortunately, not even a common standard has emerged yet to make good of what the data provide, in order to map variation consistently across time and space.

1.2 Measuring risk privatization/income instability across countries

Obviously, comparative scholars are also interested in the chance to replicate instability measures outside the US. The availability of longitudinal data at least as good for the task as the

6 More in general, when looking at changes in either the likelihood of an income drop and the dispersion of income shocks alike, the problem remains of how to discard unrealistic expectations of stability in presence of demographic or contextual changes (such as age-related income dynamics or the unfolding of the budget cycle). This also bears the question of distinguishing voluntary and involuntary variations and foreseen versus unforeseen shocks, in order to distinguish conscious planning and self-insurance attempts from actual economic misfortunes. Moreover, students or retirees may well be excluded from the model, but the effects of the inflows on family income should not yet be disregarded, or some sudden income shifts will not be interpreted correctly.

7 Income usually increases with age (until retirement). Transitory shocks to earnings are better compared at similar points of the business cycle. When looking at family income, one would assume public transfers to step in during recessions. However, it is an untenable assumption that they can provide a full compensation for the economic downturn.
PSID is by far unmatched in other industrialized countries, so that scholars interested in the American case can integrate and cross-validate their figures and findings with multiple comparable datasets. This is rarely the case of other countries. Moreover, it implies that, in terms of finding an appropriate measure of insecurity/risk, US-centred literature is likely to remain the state of the art for at least the early future. So, what are the most accomplished measures developed to analyze income insecurity? How well can they travel to other countries? Here I will consider a measure for each type of indicator described above and assess which international datasets potentially offer material for approximating a replication (without claiming how well they would work in practice).

Looking at measures of **absolute downwards social mobility**, the most sophisticated measure in existence is the Economic Security Index (ESI) developed by Hacker and his co-authors (Hacker *et al* 2010). Turning to measures of **income dispersion of income within individuals**, Scott Winship (2009) has developed an innovative measure of “pivot volatility” in order to focus on income ‘churning’ while excluding volatility increases due to upward or downward trends in income levels. Finally, looking at changes in the **dispersion of transitory shocks**, it is useful to look more closely at some operational choices by Gottschalk and Moffitt (1994 and 2009).8

A) The **ESI** represents the **share of individuals** who experience a yearly drop of 25 per cent or more in their real ‘available household income’ (excluding new pensioners), while also lacking an ‘adequate safety net’ for income replacement in the short term. The new index translates into explicit modelling choices, some of the assumptions implicit in previous analyses.9 Data are collected from different dataset: primarily the SIPP, but also the Consumer Expenditure Survey (CEX, used

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8 The same authors have also devised a more developed method to disentangle the permanent and the transitory component, by looking at ‘autocovariance structures’ (the degree to which earnings are correlated across periods) and applying a so-called ‘dynamic error components model’ Adapting these models allows changes over time in components proportions, with the estimation of a fairly complex nonlinear model (Moffitt and Gottschalk, 1995; 2008). Complexity is not the only problem for such models, as they normally require a broad set of economic assumptions that (when data details and sample size allow their adoption) alter the nature of the measured object in fundamental ways.

9 The ‘available household income’ is before tax and tax credits and adjusted for family size, debt burden, and annualized retirement assets (if the family head is older than 59) and it is reduced by out-of-pocket medical expenditures, including insurance payments. An ‘adequate safety net’ is defined as sufficient liquid wealth to absorb the income loss over the time that a typical individual would need to recover completely from a comparable shock. Since the ESI is just an index of aggregate vulnerability (not an estimate of the probability of a ‘typical’ family to incur a loss) there is no need to control for demographic characteristics other than family size, such as permanent income level and exposure to specific risks. It is particularly desirable that households’ ability to self insure is finally taken into account, looking at medical insurance payments and liquid wealth deposits. Its improved grasp on the complexity of income volatility comes however at the cost of more complexity. Movements in the indicator can be produced by actual income drops, erosion of family wealth, or raising medical expenditures: if savings get eroded over time, the process can remain silent for a while, only to become visible when the buffer is exhausted. The fact that PSID waves, used to operationalize the safety net, are conducted every two years can contribute to further swings in the indicator.
to produce a reliable estimate of medical spending) and the PSID (used to assess the amount of wealth deemed sufficient for individuals to be self-insured).\textsuperscript{10} The 2011 report shows that the trend in the ESI is rising since the mid 80s, moving up and down with the business cycle but leaving individuals more and more insecure with the passing of time. This evidence partially supports Hacker’s claim of a risk shift taking place in the last decades.\textsuperscript{11}

B) Pivot volatility (Winship 2009) is a descriptive measure of income movements within a short-term window (within-person dispersion) for the ‘typical’ individual. It involves no modelling assumptions and focuses on the frequency and extent of income reversals and eschews changes due to continuous income increments or decrements. It is constructed by looking at individual dynamics within a nine-year window, focusing on ‘pivot years’ (defined as years in which the direction of change reverses). Absolute percentage changes are computed on each side of a pivot year and averaged to measure the absolute change around each pivot; then, these ‘pivot values’ are averaged together to obtain individual pivot volatilities along the window. Finally, individual volatilities are averaged to produce an aggregate indicator, interpretable as “the average across people of the average pre- and post-pivot percent change in earnings across possible pivot years” (Winship 2009:51).

C) Following Gottschalk and Moffitt’s methodology (2009) the analysis of shock dispersion with a variance decomposition model requires choosing the length of the window (of years) over which the variance components (permanent and transitory) are calculated. If the window is too short, transitory shocks may have failed to disappear, bearing on the estimate of the permanent component. If the window is too long, changes in the permanent component could sneak in and the number of independent data points in the trends risks being too low. In consideration of the trade-off, studies employing this method usually pick moving (that is, overlapping for all but one year) windows of five to nine years.

Which kind of dataset is needed to make use of these state of the art indicators? At the most basic level, it has to come with household data and offer substantial background information. In consideration of findings above, the availability of data for the 60s and 70s is crucial for a meaningful representation of income instability trends over time. A large sample size for the panel compo-

\textsuperscript{10} Observations are discounted when they own an amount of liquid wealth equal to the cumulative income loss experienced, within the PSID, by a typical individual of the same socio-demographic group as he recovers from the shock. Since this period lasts several years, the PSID is necessary to follow it in its entirety.

\textsuperscript{11} The ESI value, in percentages of the population, was 12.2 in 1985, 13.7 in 1992, and 17 in 2002, while expected to reach 20.4 in 2009. Although it must also be stressed that it dropped to figures between 12 and 13 per cent, with the only exception of the end of the Nineties, its minimal values show the same time trend than its peaks. In sum, recoveries did take place, but they came short of compensating previous increases in vulnerability.
nent, low levels of attrition, and the inclusion of consumption data are also desirable traits. Consistent data collection policies and, as far as surveys are concerned, oversampling rounds to keep cross-sectional representativeness are other defining characteristics of the ideal dataset. For large cross-country comparisons, the issue of similarity/comparability among datasets is also crucial. How do actual longitudinal data sources for the greatest world economies compare with this ideal standard? The question is particularly relevant now, as the US literature referenced above has highlighted the limits of even a ‘top tier’ dataset such as the PSID.

As explained by Jenkins (2007) in a recent review, available longitudinal datasets consist of administrative data collections (such as the Italian INPS or the US SSA), cohort surveys, and household surveys like the PSID. Only datasets of the first and third kind are the only ones that allow inferences on both individual and aggregate dynamics. Administrative datasets cover for long years high proportions of the population of interest with no attrition problem, providing high quality income data (at least for individuals in the formal economy). However, they often come with very little background variables and, in some cases, with no type of household information, confining their use to the study of earnings dynamics only. All of the opposite, household surveys usually cover a restricted sample, may have limited or discontinuous coverage over time (respectively the case of time-limited surveys and rotated panels), and adopt peculiar compromises in terms of representativeness and following rules (maybe to emphasize/keep in sight a population subgroup of interest). On the other hand, they usually provide plenty of household and background information, including in some cases information on consumption.

Comparative longitudinal datasets are a natural place to start looking for data. The only native cross-country datasets, all too recent for an analysis of long term trends, are the European Community Household Panel (ECHP, started in 1994 and discontinued since 2001), the more recent EU-SILC (a rotating panels dataset started in 2004) and the Survey of Health, Ageing, and Retirement in Europe (SHARE), a cohort based survey started in 2003 and focusing on the living conditions of the elderly. An authoritative source of data for this kind of analysis is the Cross-National Equivalent File (CNEF, see): a harmonized collection of existing panels now including the US PSID, the German SOEP, the British BHPS, the Canadian SLID, the Australian HILDA, the Swiss SHP, and more recently the Korean KLIP as well as the Russian RLMS-HSE. Another important feat of the CNEF is the presence of estimates for taxes and transfers, which allow a more detailed examination of the impact of public policies (data inconsistencies have been indicated in the CNEF version of the PSID, however).
A promising starting point, the CNEF rather reveals the limits of household surveys outside the US. Some concern the temporal dimension. Among the datasets above, excluding the PSID, only the pioneering SOEP was started before the 1990s (1984): the BHPS and the SLID started in 1991 and 1993, the RLMS-HSE and the KLIP in 1995 and 1998, whereas the HILDA and the SHP only begun in 2001 and 1999. This makes them unsuitable for the analysis of past trends and only the SOEP is able to produce indicators of pivot volatility or shock dispersion for the early 1990s. Furthermore, respondents of the SLID are rotated in such a way that the same individual is only covered for a maximum of 6 years.

Others issue concern the sample size. The SOEP is also the only dataset to come with a sizeable sample (about 12,500 households in the last wave) whereas the others score between 4,200 and 8,700 (the SLID namely scores more than 38,000, courtesy of its rotating sample design). Besides the surveys included in the CNEF two more should be added: the Italian Survey on Household Income and Wealth (SHIW), managed by Italy’s central bank, and the Swedish Household Market and Nonmarket Activities (HUS). Both present a potentially good time span (the SHIW runs since 1977, the HUS since 1984) but are ill suited for longitudinal studies because of the small size of their panel component and, in the Swedish case, also for the low frequency of its interviews.

Administrative (tax and social security) datasets offer very good basis for the analysis of earnings dynamics. This is the case of France, Italy, the Nordic countries, and of the UK to a lesser extent: all these countries host administrative collections comparable to the SIPP for length (the first available observations date back to the early 1980s), representativeness, and high income data quality. The major hurdle on the way of employing them in household income analyses is their very narrow focus on the wage income of the employed population (tax data are generally more suitable than social security data in this respect) which makes difficult to infer any information on the family context and background. A very promising chance in the direction of more comprehensive databases is matching administrative datasets with one or more waves of household surveys: a common practice in the Nordic countries that is becoming more and more frequent in the US given the recent turn from the PSID to the SIPP as the dataset of choice.

The EU-SILC is a precious resource from this point of view, as far as its national datasets store (under strict confidentiality causes) the fiscal/social security codes of the respondents. This implies that the national statistical and administrative agencies that have access to the full data version can, if willing, match administrative data with one or more waves of their national SILC survey (such as the 2005 version, with unique questions on the family of origin of the respondents), recov-
ering at least time invariant or slowly variant variables for each respondent.\textsuperscript{12} In any case, this kind of matching is not free from errors if the original panel has collected little information useful to reconstruct each respondent’s family structure over time, or exempted from issues of cross-sectional representativeness when the starting date of the administrative dataset is more than ten years far from the date of the survey.

Keeping this data limitations in mind, one can conclude that the room for replicating the most effective indicators emerged in the US debate on economic instability/risk privatization is narrow, at least for scholars interested in measuring trends supposedly started in the Seventies. Even if it is likely that the structural transformations responsible for the increase in income instability have taken place a decade later than in the US in most Western countries, the analysis of trends starting at the end of the 1980s risks missing the most appropriate benchmark of the true extent of the changes occurred. In this sense, it is worth stressing that measures of instability levels at specific points in time are particularly sensitive to data collection and preparation choices, whereas, when comparing trends across countries, such country-specific idiosyncrasies would mostly cancel out.

If the recently expanding supply of high quality and fairly well harmonized panel datasets is encouraging, the best chance to retrieve the long time series needed to analyze dynamics of economic instability relies upon the use of administrative data and their careful matching with household surveys (such as with SIPP+SSA datasets in the US). This requires, first of all, national waves dating back enough in the past to retrieve background information for the 1970s and 1980s. Provided the secreted versions of the surveys contain serial identifiers unique to each respondent, this seems to be the case of Sweden and Germany (and possibly Italy). Even more importantly, this move requires the active interest of national administration and agencies in combining datasets by the use of sensitive information they alone can access. Unless these actors gain a stake in the production of better knowledge (and policy prescriptions) the only way forward is to refine the analysis of wage and earnings dynamics, which, however, entirely miss the insurance role of household pooling and familial redistribution.

To conclude, the search for a more appropriate and sensible measurement of levels and trends of income instability is certainly a very worthwhile and much needed task. Nonetheless, scholars have not yet been able to agree on a common indicator or standard to measure these phenomena. Moreover, the question whether income instability is a good proxy of socio-economic risk (not to say of a risk shift) is itself hotly debated and suggests more information and even more refined indi-

\textsuperscript{12} A dataset of this kind is the new Italian AD-SILC (see DdT and FGB 2012).
cators such as the ESI are needed to pin down some counterfactual implications (How and how much income instability translates into consumption patterns and household well-being? How well can families self-insure? At what level instability becomes undermining for individuals and society?). The data limitations reviewed in the last pages suggest that, at least in the foreseeable future, the comparative research is in no better position than US based case studies to address the problems that plague this research program. Much will depend on the availability of the national bureaucracies and the ingenuity of scholars working with matching techniques.

This conclusion, not to be interpreted pessimistically, is maybe sufficient to argue in favour of a qualitative reorientation of the risk privatization debate. For sure, however, it does not prove that such a turn is at all necessary. Indeed, some may argue, the risk privatization approach is already predominantly qualitative, as qualitative are its best contributions to the study of the welfare state (that is, the narratives provided by Hacker 2006 and Gosselin 2008). The only real improvement, the argument goes, truly is to pin the numbers down. For I see the point of such a criticism, in the next section I will move a second critique that comes closer to suggesting that such a reorientation is necessary. I will contend that the difficulties encountered by the quantitative research and the strong normative underpinning of Hacker’s historical analysis are both to ascribe to the limits of a notion of risk that equates it with volatility. The limits of this conceptualization have a bearing on both the measurement and the explanation of the risk shift, urging for a less abstract and more careful approach to real world policy changes in the empirical analysis.

2. ‘Measures’ of public responsibility: volatility or acceptability?

In this section I will substantiate the third and fourth claim advanced in the introduction and indicate the cause of the methodological difficulties discussed above in some conceptual aspects of the risk privatization debate. On the one hand, I suggest that indicators of aggregate ‘insecurity’ can measure a risk shift from the public to the private sphere only under the assumption that secular increases of insecurity can be fully offset by public means. On the other, indicators of ‘risk privatization’ as those described in the last section do not play the same political role than expenditure levels. As far as risk shifting reforms do not address directly the issue of distributing volatility among social actors, an exclusive focus on volatility as an outcome risks limiting our understanding of how the politics of the risk unfolds.

2.1 How well does volatility measure social risk?

To restate Hacker’s starting point, given that the US dynamic economy and lightweight welfare state could have adapted to new social risks while expanding coverage for more traditional
ones, the fact that incomes have become more risky in the last decades is directly traceable to the policy changes occurred in the realm of social insurance. Here is the sense of the risk shift. Therefore, measuring increases in income volatility roughly boils down to quantifying the translation of risk from public to private responsibility. Such a line of argument bears first of all the following question: assuming volatility could be measured with proper data and without error, how well would it capture changes in the exposure to old and new social risks?

The issue of the (degree of) overlap between volatility and risk is ground for some of the most fundamental critiques moved to Hacker within the risk privatization debate. Perhaps unexpectedly, one of the harshest criticisms has come from Peter Gosselin in one of the methodological notes of *Highwire*, one of the books more closely affiliated to GRS. In distinguishing his own approach from Hacker’s, Gosselin (2008: 325-26) says:

“In interpreting income volatility primarily as a measure, I differ from others, such as Yale political scientist Jacob S. Hacker, who have written on this subject and who appear to treat it primarily as a previously unnoticed danger in its own right.”

Less surprising is a resonating judgement by Scott Winship, one of the starkest challengers of the risk shift thesis and of its quantitative methodology (2011:19-20):

“Model-based measures of "transitory variance", on the other hand, are not only dependent on specific model specifications, they measure a quantity that is a statistical construct and not observed or necessarily experienced by actual households from year to year. The key weakness of all these measures is that they cannot distinguish between anticipated or voluntary instability on the one hand and unanticipated or involuntary instability on the other. Measures of transitory variance can only claim to do so under strong model assumptions about "permanent" income and effects of economic shocks experienced by people. This issue remains a topic for future research.”

However, what is probably the clearest withdrawal from any essentialist interpretation income volatility as ‘risk’ has been put forward by Gottschalk and Moffitt (2009:2-3) the scholars who pioneered the research on income instability:

“Higher income instability is often described as representing an increase in risk that decreases individual or household welfare, as noted in popular accounts (Hacker and Jacobs 2008; Gosselin 2008). However, this conclusion must be approached cautiously. Some types of instability are the result of voluntary decisions by workers and families […] fluctuations in bonuses among highly paid analysts in the financial sector, do not seem like a source of individual risk that should be of much social concern. However, greater instability of earnings and income among low-wage and unskilled workers, where liquidity constraints are almost surely important, may well
represent an increase in household risk that is troubling. [...] Finally, we note that instability of family income involves additional considerations beyond instability of individual earnings [...] while instability of earnings primarily reflects changes in labor market factors, family income instability reflects a host of additional considerations.”

In sum, even authors belonging to different sides of the risk privatization debate seem to agree that volatility and risk cannot be equated without knowing more about the decisions and the characteristics of the risk-takers. What can be done to improve the fit between the concept of risk and measures of volatility? Most economists suggest looking at the volatility of consumption patterns in order to control for households’ abilities to self-insure by smoothing income inter-temporally: a solution that would greatly increase the data availability problems described above. A complementary strategy, in the light of the quotation from Gottschalk and Moffitt, is to look at volatility trends across subgroups of the population, focusing on peculiarities that may facilitate interpreting trends. Typically this is done by looking at different demographic characteristics or life events. The rationale here is not so much to reach a more accomplished overarching indicator, but to look empirically at the relation between income volatility and a set of predefined risks.

For instance, Gosselin 2008 and Gosselin and Zimmerman 2008 decompose income volatility in the US by family-earners groups, finding that two-earner families are able to diversify away some of the volatility experienced by the breadwinners. They also show figures across income level and generations, indicating that volatility increased much greatly for the working poor and less than average for families with a middle-aged head. Decomposing it for income type (wage, transfers, etc…), they show that among the main drivers of the increase in income volatility between the 1980s and the 1990s stand the greater income instability of public transfers. Finally, following Burkhauser and Duncan (1989) the authors control for downward mobility conditioning for seven life events interpretable as ‘risks’: separation/divorce; death of spouse; birth of a child; reduction of income as a consequence of retirement or disability; unemployment or illness of the family head; fall in work hours of the second earner. In so doing, they manage to show a secular increase in the association of each of these events with a 50% income drop (even if risks themselves occur less frequently than before).

None of these results is particularly surprising: the bottom line is that the most vulnerable social groups are very likely to suffer dire financial consequences when they experience unfavourable life events. At a closer look, however, this kind of analysis answers quite well to the problems highlighted in the three quotations. First of all, it does not treat volatility as a risk in itself, but as the manifestation of a (socially stratified) condition of social vulnerability. By separating risk and in-
come volatility, the approach sidesteps the problem of looking at consumption data to assess households’ ability to self-insure (in order to focus on misfortunes that outmatch their efforts). Second, by looking at both income volatility and socio-demographic characteristics, this kind of analysis offers the policy makers a wider choice of alternative social goals, other than reducing income instability as such, on universalistic premises. On the downside, however, it clearly (and less innovatively) focuses on a set of risks that are determined a priori, stepping back from Hacker’s more ambitious attempt to reach beyond a backward-looking and formalistic conceptualization of risk.

To restate, Gosselin’s answer to the challenge of operationalizing risk with volatility is to emphasize their distinction, interpreting the latter as an indicator of vulnerability to risk, rather than of risk as such. The analytical implication of this reasoning is that the increase in social vulnerability has not a single cause but it interacts unfavourably with increases in inequality. By conflating volatility and risk, Hacker comes instead to argue that the “great risk shift” is the product of the agency of conservative policy entrepreneurs and of the “personal responsibility crusade” they have waged in the last decades. Without conservatives moving US politics ‘off center’ (Hacker and Pierson 2006), he argues, the American welfare state would have remained faithful to its old ambitions and answered the incoming structural challenges. Apparently, the whole idea of a risk “shift” (as opposed to a risk “increase” or a “vulnerability increase”) rests upon this counterfactual. It is tempting to conclude that, although less innovative, Gosselin’s approach finds at least a remarkable strength in being independent from similar bold pretensions.

Moreover, Hacker’s assumption appears largely untenable as one turns to the comparative context. First of all, it could be argued that reforms Hacker himself describes as distinctively risk privatizing (such as the introduction of defined contribution pension schemes or the marketization of healthcare) are strongly correlated with the incumbency of Third-way centre-left governments and have been justified as attempts to modernize the welfare state (see Levy 1999; Liester 2004).

A way to reconcile the idea of a conservative backlash with risk shifting reforms enacted by the left is to emphasize the role of neoliberal economic ideas (Hall 1993) in marketing conservatives principles to the moderately progressives. However, turning for instance to the hotly debated issue of retirement, serious economic research has never neglected as such the insurance function of traditional pension systems based on intergenerational transfers and predefined benefit levels (see for instance Beetsma and Bovenberg 2009, Gottardi Kubler 2011). Its main policy prescription was to find a more flexible balance with a greater ability to diversify systemic risks (such as ageing, unemployment, or economic stagnation). This suggests that, even if neoliberalism has certainly a role in the diffusion of risk privatizing reforms, there is no way to say that recent economic thinking is
so unequivocally close to the conservative agenda that it could rewrite from afar the policy programs of the left.

Second, it could be argued that, alongside quantitative retrenchment of existing programmes, risk shifting reforms have been introduced in response to fiscal crises or unfavourable economic or demographic dynamics such as de-industrialization or population ageing (Hemerijck 2006). How to take into account the role of politics under this alternative scenario? Are there alternatives to the risk shift or, at least, alternative venues to it? Questions of this sort lead us to the fourth claim advanced in this paper: namely that the exclusive focus on volatility may mislead the interpretation of the politics of risk privatization.

2.2 Volatility and the politics of social risks

All in all, the impression is strong that figures such as those presented by Gosselin (and the conceptualization of risk they imply) are more in tune than Hacker’s with conventional policy debates on the scope of public responsibility and its role in exerting a ‘welfare effort’. Even if providing a more fragmented kind of evidence than aggregate measures of income instability, this alternative research design provides not only more interpretable results, but also a clearer indication of when (and perhaps whether) an increase in volatility is likely to represent a situation of social concern.

There is no doubt that, all over the advanced political economies, the debate on welfare reforms is at least as interested in qualitative aspects (modernising and strengthening the distributive principles embodied by the welfare state) as it is in purely quantitative ones (the generosity and fiscal impact of welfare provisions). Therefore, it makes perfect sense to distinguish, on the one hand, quantitative interventions with little effect on the basic goals and principles of existing welfare institutions from, on the other, qualitative changes with potentially little fiscal impact. There is, however, a subtle difference in the role played by quantitative indicators of social welfare in each case. When the debate focuses on quantitative aspects, measures of welfare effort such as coverage and expenditure levels are a powerful lens to interpret the policy debate and advance hypotheses on the preferences of actors such as interest groups, the political left and right, as well as unions and employers. As institutionalist studies of welfare expansion have shown, however, when more qualitative issues are at stake, such as entitlement principles (citizenship, career record, or need) or funding strategies (payroll taxes or the general revenues), the lines of conflict and coalition building get blurred and become difficult to identify ex ante (see Baldwin 1990 and Ferrera 1993).
In this respect, the measures of income volatility and mobility reviewed in the previous pages, when intended as rough indicators of a reduced scope of public responsibility for social welfare, can be said to occupy a middle ground. On the one hand, the whole idea of a risk shift points at an entirely redistributive question: risk has been moved from the state and the employer to the family. One could then easily imagine a conflict line as clear as Hacker describes, with the political left and its usual allies trying to keep risk on the shoulders of ‘strong’ economic actors, and the economic right striving for the opposite, with centrist parties sticking to their time-honoured preference for a ‘mixed’ welfare state. On the other hand, however, the risk shift is by definition a ‘subterranean process’ where figures of volatility, difficult in themselves to conceptualize and measure, are hardly as fungible for political mobilization as the size of expenditures and benefits. Aggregate spending indicators are suitable for crude international comparisons, whereas measures of welfare generosity, when made public, give an intuitive measure of the adequacy of social programmes vis-à-vis their stated goals. Moreover, the perception that family income has become more volatile is generally not complemented by the impression that public budgets or firm profits (save perhaps some multinational quasi-monopolists) have become correspondingly less so.

On the contrary, reactions in the political debate are arguably not different from those I quoted from the academic publications. The ambition to stabilize household incomes regardless of their level enters a number of normative trade-offs, most of which are nothing new but have grown more compelling in the face of mounting budget pressures. Such trade-offs include first and foremost the level of taxation and public debt or the labour costs necessary to support extensive systems of social protection. In addition, they concern the level of social mobility as well as the scope of economic opportunity and meritocracy that is compatible with a society where incomes (including earnings from self-employment and household firms) are statutorily kept from changing. Then comes the issue of configuring social shock absorbers that do not interfere with the mobility of the workforce across firms and sectors (and nations, for instance within the EU) and can work equally well for the stable careers typical of manufacturing and the more flexible work patterns of the service economy.

Finally, recent studies in the political economy of welfare (Cusack and Iversen 2005, Häusermann 2010) as well as in the evolution of European party systems (Kriesi et al. 2008) suggest that, in societies that are growing more and more unequal over time, different social groups (identified by their age, occupation, or region of residence) may well be available to accept different levels of volatility in order to reach their desired level of absolute income. From this point of view, the question of the nature and limits of public responsibility cannot plausibly be reduced to a dis-
tributive conflict on the amount of volatility borne by different societal actors. More to the point, it is highly unlikely that, in such a multi-dimensional policy domain, the only apparent line of conflict may be that between the supporters of welfare expansion and the conservative wagers of a ‘personal responsibility crusade’.

What can then be done, at the methodological and conceptual level, to lay the foundation of a more sensible understanding of these thorny policy dilemmas? On the basis of what argued before, it is possible to suggest that the problem could be lessened by reducing the emphasis on volatility, for instance by adopting, like Gosselin, a more traditional focus on a predefined set of social risks. This could involve a more empirical and less normative understanding of the explicit goals of existing programs, while looking at very specific (rather than highly abstract) outcomes (including volatility) over the appropriate time horizon (very short for, say, unemployment benefits but very long for, say, pensions). These issues will be discussed more in depth in the third part of the paper.

In concluding this section, instead, attention must be paid to the crucial challenge posed by the choice of defining a priori the set of risks and goals against which to measure ‘volatility’. The problem is precisely which risks and goals should be eventually considered, among the many that is possible to imagine. On the one hand, the question is made methodologically problematic by its apparent normative implications on the one side, and, on the opposite side, by the pending risk of misunderstanding the ultimate goals of social programs that exist in specific historical contexts (a danger which is, anyway, implicitly present in Hacker’s counterfactual reasoning as well). On the other hand, this is the framing of the problem that possibly comes closer to understanding policy debates concerning welfare reforms, allowing for the analytical framework that best contextualizes and fits the policy problems and solutions under analysis.

A possible way out of the conundrum is to complement the analysis of risk based on the notion of volatility with one focusing on the sociological question of how social risks and insurance goals are selected. This task can be fulfilled by reconsidering the relationship between risk and volatility in the light of the so-called ‘sociology of risk’ (see for instance Beck 1992; Douglas and Wildawsky 1982; Giddens 1990; Luhmann 1993; see also Taylor-Gooby and Zinn 2006). It is already worth stressing that the point here is not to suddenly shift the frame of reference to a ‘constructivist’ understanding of risk, thus removing the problem of quantification from the picture. On the contrary, the point is to get right both ‘the numbers’ and the set of ‘risks’ or ‘life events’ used to put volatility in context, perhaps decompose it, and (possibly with better and more comparable indicators) relate it back to the social and political choices that have produced them.
Adding to the picture the sociological perspective, allows contextualizing the centrality of volatility in a ‘rationalist’ tradition in the study of risk: a tradition that draws on the work of the economist Frank Knight (1921) and is by far the dominant one in the hard sciences and in economics. The ‘rationalist’ approach is based on the distinction between uncertainty, experienced at the individual level, and risk, intended as a calculable statistical construct that follows the rules of the calculus of probability. Within this camp, risk is usually defined as “expected harm”, thus formally as the damage times the probability of the negative event (Royal Society 1992). The traditional definition has, however, recently been challenged inside the very rationalist field. The search for a more neutral conceptualization, faithful to the ambivalent nature of risk, has led the International Organization for Standardization (ISO) to define it as “the effect of uncertainty over goals” (ISO 2009). Yet, this seemingly intuitive notion is possibly incomplete, at least for the political scientist, as far as it does not raise any question about those ‘goals’. Confronting the goals on the basis of which risk itself is defined is precisely the main concern of the sociological approach. It is also the reason why I believe it can fruitfully contribute to the risk privatization debate.

The label of ‘sociological approach’ to risk commonly refers to three distinctive schools of thought, all interested in the historical evolution of the concept of risk, that developed almost independently across the 1980s and the 1990s, in the wake of a widespread loss of public confidence in the authority of experts and in the effectiveness of public risk management. The first school, the socio-cultural approach, was established in the US by the work of Mary Douglas and Aaron Wildawsky (1982). The second strand covers the macro-theories of modernization and individualization advanced by sociologists such as Ulrich Beck (1992), Zygmund Bauman (2000), and Anthony Giddens (1990, 1998), as well as by the philosopher Niklas Luhmann (1993). Finally, the third consists of the ‘governmentality’ approach that has developed drawing on the original work by Michel Foucault (1991). Each of these perspectives offers a peculiar but comprehensive framework to study the interplay between risk, politics, and society over at least the last century of its evolution. Here, rather than attempting an insufficient overview of their specificities, I will focus on their commonalities and on the intuitions useful to refresh the risk privatization debate.

Although in different ways, all of the three approaches emerged to shed new light on the controversies between scientific knowledge and public (laymen) concerns, taking step from a refutation of the dichotomy between ‘objective risks’ and ‘subjective risk perceptions’. On the basis of this common interest, the three perspectives have formulated alternative definitions of risk that recognize it as a peculiar type of ‘social construction’: one which happens to be shaped and structured by ‘objective’ social realities (such as technological revolutions, power dynamics, and changes in so-
cial organization and in the productive system). Providing in insight that could be usefully applied to the measurement and interpretation of income volatility, sociological theories interpret the rationalist approach’s epistemological limits and lack of political appeal in the light of this ‘dual reality’ of risk. Moving from this distinctive understanding, the sociological camp looks at the connection between the empirical phenomena and the normative implications that concern the issue of risk and public responsibility. The three sociological schools confront this task by recasting the question of why experts and laymen have different perceptions of risk into the question of why exposure to different risks happens to be more or less ‘acceptable’ in different historical contexts. Of course, they provide very different answers to the question and interpret differently the transformations and the actors behind changes in the social significance of risk. Such differences, however, are not the point of my contention: as effectively summarized by Taylor-Gooby and Zinn (2006), there is one broad lesson to be learnt from the sociological perspective, and it is that:

“Risk is not just an objective entity but also a specific way of understanding society and placing a value on particular approaches to opportunities and dangers.”

Common within the sociological approach is also associating the issue of acceptability with that of choice, and therefore responsibility. Douglas and Wildawsky (1982) and their followers link acceptability and choice through what they call the ‘forensic functions of risk’: mechanisms that allow societies to cope with uncertainty through the attribution of individual accountability and culpability and the identification of somebody responsible for the harm. Governmentality scholars relate the individualization of risk to the success of the neo-liberal agenda and of the specific forms of social control it purports.

Finally, risk ‘society theorists’ such as Beck and Giddens (Beck et al. 1994) suggest that secular processes of modernization and individualization have led to the emergence of a new distinctive kind of risks, characterized as catastrophic, systemic, and increasingly recognized as self-inflicted side-effects of other human endeavours (that is, choices). In simple terms, this awareness is seen as producing a loss of confidence in the collective management of risk and a more self-critical and proactive stake in the issue (‘reflexivity’), which translates in a more individualistic approach to matters of risk and responsibility (that is, choice).13 Luhmann even suggests that (manmade) risk is only distinguished from (natural) danger when harm can, rightly or wrongly, be traced back to a single critical decision: by virtue of the persuasiveness of this (pseudo)causality, he argues, risk has

13 Beck and Giddens differ, however, in their explanation of ‘reflexivity’ as a consequence of macro-institutional changes (Beck) as opposed to socio-cultural ones (Giddens).
become the only category under which modern societies are able to think of their future and face the challenges ahead.

In all cases (and reaching well beyond the limited field of socioeconomic risk) the notion of acceptability points at the existence of a politics of risk that does not revolve solely around distributive implications, but also around identities and norms. It suggests that public responsibility for risks that are commonly associated with marginal or culpable minorities may become less acceptable within a given society, even if they touch on everyone to some extent. Or, by contrast, it points out that members of the middle and even of the working class might fear pervasive risks like over-taxation and public debt, which bring about certain losses out of their control, more than individual life events they feel they can ‘do something about’. In other words, within societies riddled by both ‘individual uncertainties’ and ‘systemic catastrophes’ pre-existing social fissures and identities, widespread losses of trust in institutions or politics, and the evolution of cultural norms may create unexpected reserves of political capital for a renegotiation of public responsibility. This has implications not only for how risk privatization is explained but also for how it is measured and interpreted: ‘volatility’ is an important indicator, but it only reveals its significance through the lenses of ‘acceptability’.

It is easy to see that, from this common ground, sociological theories of risk offer many tools to expand and put in context Hacker’s thesis of a privatization of risk, pulling it out of the specificity of the US case and even out of its own assumptions. While governmentality theorists would probably draw closest to Hacker’s focus on the agency of conservative policy entrepreneurs, proponents of the socio-cultural school could shed light on the most complex interaction between distributive and value-related issues influencing the politics of risk and public responsibility. On its turn, the risk society framework may give a more plausible interpretation of how post-industrialism is reconfiguring social mobilization and political competition for equality and security, creating rival claims within the left and giving new political appeal for the welfare residualism of the economic right.

More to the point of the present paper, sociological theories of risk also give suggestions of conceptual and methodological nature. First of all, they give definitions of risk that complete the rationalist understanding of risk as an objective statistical construct. Their bottom line is: risk is dual, partly an objective reality and partly a social construction. Second, they argue for a more sophisticated qualitative approach not because indicators are imperfect, but because they remain contentious (and basically not interpretable) absent an analytical framework aware of this ‘dual nature’. Finally, they leave to the political scientist the task to avoid exercises of abstract measurement,
while defining with greater precision which risks are identified and politicized, as well as how they map onto the most salient cleavages in each national social fabric. My impression is that a qualitative approach modelled on these suggestions can win back to the comparative study of risk privatization a number of opportunities that GRS and its quantitative follow-ups have ignored.

3. ‘Tweaking’ the qualitative framework: the case of Bismarckian pension policies

In the previous sections I argued that the expansion of quantitative studies in the research on the privatization of risk, while addressing a crucial and worthwhile empirical issue, shows at best a mixed record. While state-of-the-art indicators of income instability in the US debate already reach beyond what non American datasets would allow for other countries, the lack of a transparent measurement and of a shared methodological standard weakens the evidence supporting the empirical puzzle at the basis of the ‘risk shift’ thesis. In the meantime, this same evidence fits the risk privatization story only under strong (or, at least, case-specific) assumptions and becomes tricky to interpret otherwise, not lastly because indicators of income instability, unlike measures of ‘welfare effort’, play little role in the political debate. I tried to trace back these problems to a conceptual fallacy: the reliance on a definition (or, better, a ‘notion’) of risk that is commonly used in the hard sciences and in economics, but which has often proved itself ill-equipped for the study of risk in the political and social sciences. Thus, I indicated in the sociological theories of risk and in their notion of ‘acceptability’ the insights on whose basis risk should be reconceptualised, in order to complement a rigorous analysis of volatility with an improved qualitative framework, more suitable for cross-national comparisons.

In this section I will briefly present the research design I adopted in my dissertation, where I set up a qualitative framework in order to compare risk shifting pension reforms in Italy, Germany, and the US. The overview will describe my puzzle, research strategy and casing, while focusing on the way I assess and, where possible, quantify retirement ‘risk shifts’. It will address neither my hypotheses of explanation, the way I test them in the thesis, nor my country narratives and causal argument, which are not directly relevant to the topic of this paper.

The puzzle. To start with the basics, my research takes step from a question that is equivalent to Hacker’s in the first pages of GRS: why, along the last decades of economic change and increasing uncertainty, democratic political economies have been enacting welfare reforms that have shifted on individuals and families risks and responsibilities that were previously socialized and dealt with collectively? To substantiate this generic question I provide, for most of the economically advanced countries, several shards of evidence pointing in the direction of (1) an increase in eco-
nomic vulnerability of broad sectors of the population (2) a decrease in the share of public to total social expenditure and (3) a list of reforms aimed at introducing markets or market processes in the provision of welfare benefits. I then position myself within the research agenda on the privatization of risk and declare my interest in qualitative changes in the distributional principles embodied by existing welfare states, rather than in expenditure cuts and retrenchment. Nonetheless, I make explicit my dissatisfaction with the quantitative turn in the North American debate and point at what I see as a blank spot in the literature. Namely, I point at a comparative historical analysis (Mahoney and Rueschemeyer 2003) of risk shifting reforms able to connect Hacker’s research on the changing social distribution of risks (which I find convincing policywise, but weak on matters of politics) and European analyses of welfare ‘recalibration’ under the shadow of economic crisis (which I find questionable policywise, but intriguingly sophisticated when dealing with politics).

**Research design.** From a broad methodological point of view, my thesis is not particularly innovative and follows the traditional research design of American historical institutionalists such as Skocpol, Immergut, or Steinmo (see Steinmo 2007 for a review). Minoritarian in the methodological camp, this approach privileges the historical understanding of the specificities of each case study over more variable-oriented comparative perspectives. Therefore it deviates from the most popular ‘Mill’s methods approach’ promoted by scholars such as Lijphart (1971) in two fundamental respects: (1) cases are selected along the distribution of the “dependent variable” as (weaker or stronger) instances of the same phenomenon; (2) no explanatory variable is isolated with a most-similar or most-different criterion in order to result as “the effect, or the cause, or an indispensable part of the cause, of the phenomenon” (at least, not by virtue of the comparison alone). It is the task of the historical analysis to connect the dots and weight the hypotheses with the available evidence as well as counterfactual reasoning. ⁴ In order to improve my casing, I also rely, on a ‘hard case logic’ (Gerrings 2008), insofar I selected cases where (1) reforms of any kind are made “most difficult” by veto-heavy law-making processes; (2) subtractive welfare reforms are made “most difficult” by the institutional legacies in place; (3) risk privatization is made “most difficult” by the endemic presence of an institutionalized compromise between risk pooling and social stratification. ⁵

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⁴ In sum, this methodology turns to the heuristic power of process tracing and historical analysis (which can indeed be understood as a most-similar comparison over time) to discriminate between a number of equally plausible hypotheses. To achieve this goal, the cases must capture a great amount of variance both in the phenomenon to be explained and in the potential explanatory factors. For this reason, this strategy is often labelled of ‘diverse cases’ (Gerrings 2008).

⁵ By the third point I mean to say that I excluded cases where strongly universalistic policies could suffer a pressure to converge to international averages (e.g. healthcare in Sweden), as well as cases where the progressive residualization or privatization of social welfare could be easily interpreted as a cumulative policy feedback (e.g. food stamps in the US).
Casing. On the one hand, the hard case logic suggests the study of pensions (allegedly the hardest social policy to reform), in particular of Bismarckian\(^\text{16}\) pension systems (where risk pooling and social stratification are bounded in an occupationalist compromise, see Schludi 2005), in veto-heavy economically advanced democracies (where legislative blockages, dynamics of political competition, and adaptive expectations over the role of the welfare state make subtractive reforms politically painful). On the other hand, the kind of historical comparison described above urges to maximize the variation among the cases, both in terms of risk privatization (proxied \textit{ex ante} by the number of subtractive pension reforms undertaken) and in terms of the political and economic environment more in general. Ideally, this further requirement would have been fulfilled by the choice of four cases: Italy, Germany, Japan, and the US. Japan, however, got soon dropped for practical concerns. With regard to the three cases left, I focus on the period 1978-2007, looking at policy changes in entire pension system, in both publicly and privately sponsored schemes.

Risk privatization as the \textit{explanandum}. Taking advantage of my case selection strategy, focused on policies rather than national welfare systems (not to say welfare regimes), I approach risk privatization by taking a closer look at: (1) the specific functioning of each pension policy; (2) the underlying goals of retirement policy in each country; (3) their variation over time.\(^\text{17}\) My perspective is obviously informed by the reflections offered in the previous sections, even if the discussion of quantitative techniques is less extensive than here. I conceptualize risk privatization as a sequence of policy changes or a ‘policy trajectory’: a matter of changes over time, rather than levels. Unsurprisingly, I also characterize each of the policy changes that bring the trajectory about as a

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\(^{16}\) Bismarckian here refers to a particular type of pension program pioneered under Bismarck’s chancellorship. Bismarckian programs are financed by dedicated payroll taxes, typically follow occupational demarcations, and pay benefits proportional to wages. They are defined in opposition to Beveridgean ones, which rely on tax financing and focus on poverty protection. It is worth stressing that they are not intended as the pension policies of Esping-Andersen’s \textit{conservative/corporatist welfare states} (1990), nor they refer in any way to Bismarck’s own ideas or goals.

\(^{17}\) While points (1) and (2) may resemble Peter Hall (1993)’s concept of policy paradigm (equating risk privatization with a ‘paradigmatic change’), it is worth stressing that: (1) I am not assuming any hierarchy or strong correlation between policy goals and policy means/settings, nor that the former are more change resilient than the latter; (2) I am not looking at economic ideas (namely at the shift from the Keynesian to the neoliberal orthodoxy) as the sole or the main driver of changes in the policy goals. On the contrary, I consider the problem of risk acceptability in the light of the \textit{risk society} approach above, arguing that changes in social organization and the increasing relevance of systemic risks have been creating political room for a more decentralized management of risk.

To add further details on my project, it is my contention in the thesis that such a political room is first opened by the appearance of systemic crises such as stagflation, public debt, or mass unemployment, and then reinforced by the loss of trust in public management and politics that arises in the face of prolonged policy failures (neoliberalism being here as much as an effect than a cause). However, the way political opportunities for decentralizing the management of risk (and by opportunities I mean room for innovation rather than openings for a conservative agenda) translate into actual policy change rests on country specific factors. In my work I emphasize the role of political competition and the quality of industrial relations.
‘(downwards) risk shift’. A risk shift is defined as a reduction of one or more of the conditional financial guarantees offered by the policy at time t-1. In other words, a risk shift is defined in functional terms, as the weakening of an internal ‘shock absorber’ targeted to a specific event (a shift from wage to price indexation being a typical example). To sharpen their connotation, ‘risk shifts’ are also defined in opposition to quantitative ‘retrenchment’, identified (in an admittedly ideal-typical fashion) with across-the-board expenditure cuts (good examples are generalized cuts in the revaluation of pension contributions, as well as increases in payroll taxes with no benefit increment).18

**Assessing the risk shift.** The main implication drawn in the last section of the paper is that it is futile to produce measurements of the ‘risk shift’ without making explicit which risks are shifted, thus which risks experience a reduction in the financial guarantee associated with them. Financial and economic studies have come up with very long and detailed lists of ‘risks’ associated with the functioning of pension programmes. These catalogues are ingenious in suggesting many things that could ‘go wrong with pensions’, but of limited use for understanding the relation between risk and social goals. In order to conflate them, I started from the alleged social goal embodied in Bismarckian pension systems: namely, status maintenance in the phase of quiescence for all those who qualify as ‘workers’. This general ambition has been realized in highly country-specific ways, which matter for both the generosity and the guarantees of these pension systems. The ability of a pension system to maintain status is frequently measured by its replacement rate.19 However, changes in the replacement rate are inadequate indicators of a risk shift as I define it. On the one hand, they may result from both across-the-board cuts and the reduction of certain financial guarantees; on the other, they ignore that the likelihood of achieving a full career may have dramatically changed over time (see Hinrichs and Jessoula 2012; on Italy see Berton et al. 2009).

So I pose a further qualification and consider three different dimensions of financial guarantee: accumulation, annuitization, and dynamization. With ‘accumulation’ I consider whether the program offers financial guarantees against the vicissitudes experienced on the labour market (such as contributory credits for high education, childbearing, unemployment…). With ‘annuitization’ I consider whether the program offers financial guarantees against events occurring at retirement (this is the case when retirees pay a premium to convert their contributions in a monthly pension benefit,  

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18 Although drawing a line between the two is sometimes tricky (especially when horizontal cuts are targeted to specific occupations) the distinction helped me to map more precisely the policy menu of contemporary pension reforms.

19 Replacement rates it offers, after a full career, at the moment of retirement (the ratio between pension income and the last wage paid to the new retiree) and then in various periods after retirement (the ratio between the current pension income of the same retiree and the last wages paid to new retirees in the same occupation).
or when pension income is conditioned to current economic and demographic scenarios). Finally, under the tag ‘dynamization’ I look at the financial guarantees offered against events occurring after retirement, such as indexation to inflation or wage growth (or, conversely, at the introduction of negative indexations to economic or demographic trends). A (downwards) risk shift occurs when, by the revision of a program or its substitution with another one, one or more of these financial guarantees are weakened.\textsuperscript{20}

**Empirics.** The empirics needed for assessing the extent of the risk shifts come from a close examination of pension laws and reform bills, and of their effects on the functioning, regulation, and relative importance of public and private pension schemes. For instance, even if it is often the case in practice, I make no a priori assumption that public schemes offer more risk pooling than private or occupational ones or that defined contribution programs are necessarily more risk privatizing than defined benefits one: I just look at how they really work and what they actually promise. Although my assessment is mainly qualitative, it does not mean that the effects of the various ‘risk shifts’, once identified and distinguished from across-the-board cuts, should not be quantified. This can be done very well by looking at the expected savings of the pension sponsors, or by the effects on the replacement rates \textit{ceteris paribus}, as economists and political economists would do for retrenchment as well.

**Quantification.** In the light of my critical assessment of the quantitative debate, it is nonetheless clear that a measure of how recent reforms have made replacement rates sensitive to individual fortunes and to economic or demographic developments would be a preferable indicator of the extent of the risk shift. Particularly telling is to examine how career profiles previously entitled to the same replacement rate become stratified on the basis of vicissitudes undergone individually or at cohort level. The main issue with this quantification strategy is that, since pension reforms produce their effects only far away into the future, the necessary data do not exist yet. Luckily, the problem is not without solution. First of all, even if to a very small extent, this exercise can be done with the estimates provided in official publications. Second, micro-simulations appear as a powerful ally for pension scholars. In fact, a number of recent contributions have shown the potential of using them to assess individual level outcomes of current policies in the remote future (see for instance Meyer and Riedmüller 2007). Compared to the analysis of current income dynamics, pension income micro-simulations resemble the analysis of earnings and employment patterns. The method also allows researchers to control for different assumptions and explicitly model counterfactuals. The biggest

\textsuperscript{20} As long as the concept of financial guarantee has been clarified, I will not enter here in the technical details of how specific policy settings reallocate responsibilities between pension beneficiaries and pension sponsors.
challenge for this approach is to develop reliable estimates of employment transitions and unemployment spells that can be used to model future lifetime earnings. Great steps forward in this direction are currently being moved courtesy of innovative datasets and economic modelling tools such as the Italian T-DYMM (DdT and FGB 2012). By the time I was designing my thesis, however, I opted to leave this methodology for future research, so I do not expect to include any micro-simulation based indicator in the current form of my work.

**Concluding remarks**

In this paper I have argued that the research program on the privatization of risk, after the publication of *GRS*, has experienced a quantitative turn, trying to reassess and improve Hacker’s contentious finding that the instability family income had experienced a marked secular increase and a huge peak in the early 1990s. I advanced four claims to comment on the conceptual and methodological implications of this new stream of literature, arguing that not only a common quantitative standard is still missing, but also that state of the art indicators for the US case are difficult to replicate in other contexts and that their interpretation rest on arbitrary assumptions and may mislead the understanding of the politics of the risk shift. In support of my critical appreciation, I offered an overview of the US scholarly debate, of the datasets available for the comparative analysis, and of the main criticisms moved against Hacker’s original analysis. In trying to interpret these difficulties I argued that the qualitative differences between US datasets and those available for other advanced countries are a problem with only limited solution.

Focusing instead on more interpretative and analytical problems, I also contended that the issues above might be the result of a shortcoming in the (implicit) definition of risk upheld in this debate. I thus moved to analyzing the alternative conceptualization common to the sociological theories of risk, arguing that it may provide a firmer starting point for a revised qualitative framework of this research program, more suitable for the comparative analysis and less centred on North American specificities. Finally, I presented the analytical framework of my dissertation, which deals with the privatization of retirement-related risks in three countries: Italy, Germany, and the US.

In conclusion, I believe that researchers interested in the comparative analysis of risk privatization should reconsider and redesign the tools offered by the existing literature. The quantitative approach is very sophisticated and boasts contributions from various social sciences, but it has probably grown to the limits of the notion of risk it has adopted. It seems logical now to turn back to the qualitative analysis to overcome these limitations and find new ways to combine rigorous and standardized indicators with an in-depth understanding of each national trajectory.
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