

Labour Strategies of Families: A Critical Assessment of an Appealing Concept

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Phyllis Moen and Elaine Wethington were absolutely right when they called family strategies “the intuitively appealing metaphor for family response to structural barriers”.¹ This appeal probably explains the avalanche of studies on the subject since the 1970s and especially since the 1980s. The last contribution, to my knowledge, is a collection of articles edited by Laurence Fontaine and Jürgen Schlumbohm in 2000.² I will not even try to outline the vast historiography. This paper focuses on another problem. It is an attempt to show that concepts built on appealing metaphors lose much of their appeal in empirical research for the simple reason that their application tends to be more complicated than expected. In the following pages an example of such an experience is presented. Within the virtual walls of the Dutch National Research Institute for Economic and Social History, the N.W. Posthumus Institute, we have been struggling with family strategies since 1994. Now that we are about to publish the third volume on the subject, it is time to evaluate what we have accomplished.

Let me start by stating the obvious: the issues studied and the methods used in scientific research closely follow the trends in society at large. It is hard to imagine a discussion on family strategies before the 1970s. Structural functionalism dominated the scholarly climate of the 1940s, 1950s, and 1960s to such a degree that there was hardly any room for agency. In those days, human behaviour was often simply considered to be the logical result of economic and social developments at the macro level. For historians, the influential *Annales* school set the tone by emphasizing structures and trends rather than events and individual actions. Basically, historical actors were reduced to puppets on a string.

All this has changed remarkably within two or three decades. Waves of individualism have flooded Western societies and, suddenly, scholars have shifted focus too. From a myopic preoccupation with structural

1. Phyllis Moen and Elaine Wethington, “The Concept of Family Adaptive Strategies,” *Annual Review of Sociology*, 18 (1992), pp. 233–251, 233.

2. Laurence Fontaine and Jürgen Schlumbohm (eds), *Household Strategies for Survival 1600–2000: Fission, Faction and Cooperation*, *International Review of Social History*, Supplement 8 (Cambridge, 2000).

determinants of human behaviour they have switched to the study of agency. There are two famous names attached to this development. Pierre Bourdieu's 1972 article on marriage strategies is often considered to be the starting point. Bourdieu himself, however, warned from the very beginning against too strong an emphasis on freedom of choice. Therefore, he introduced the concept of *habitus* to bridge the gap between structure and agency.³ Anthony Giddens is the other well-known representative of this process in sociology. He too introduced a new concept to study the interrelationship between structures and agency, and he called it *structuration*.⁴ The rational-choice approach goes one step further by reducing social developments to the choices, intentions, and acts of individuals. In the so-called *new home economics* the family is even considered to be an organizing and acting network of individuals.⁵ Logically, these developments in scholarly approach were the preconditions for the kind of research we reflect upon today. Once individuals were transformed from puppets into rational actors again, the stage for strategies was prepared.

As far as social history is concerned, we can put a date to this process – I would say approximately 1980. Louise Tilly's article on the individual lives and family strategies of the French proletariat was published in 1979.⁶ Tamara Hareven's study of 1982 on family time and industrial time was based on the family-strategy concept too.⁷ As far as I know, it took until 1985 before the first discussion on family strategies was organized at a social science history conference. In the same year, Ewa Morawska pointed to the possibility for historical actors to "play within the structures".⁸ This is the position most historians still agree upon nowadays: structural forces in economy and society are very much present in the everyday life of historical actors, but only as the stage on which these actors also have a choice from several options.

The research project I report on here started in 1994 with a volume called (in translation) *Living Together, Working Together? Five Essays on the History of Labor and the Family*.⁹ The essays are methodological and historiogra-

3. Pierre Bourdieu, "Les stratégies matrimoniales dans le système de reproduction", *Annales ESC*, 27 (1972), pp. 1105–1127.

4. A. Giddens, *Central Problems in Social Theory* (London, 1979).

5. N. Folbre, *The Economics of the Family* (Cheltenham, 1996).

6. Louise A. Tilly, "Individual Lives and Family Strategies in the French Proletariat", *Journal of Family History*, 4 (1979), pp. 137–152.

7. Tamara K. Hareven, *Family Time and Industrial Time: The Relationship Between the Family and Work in a New England Industrial Community* (Cambridge, 1982).

8. E. Morawska, *For Bread with Butter: The Life-Worlds of East-Europeans in Johnstown, Pennsylvania, 1890–1940* (Cambridge [etc.], 1985).

9. Michiel Baud and Theo Engelen (eds), *Samen wonen, samen werken? Vijf essays over de geschiedenis van arbeid en gezin* (Hilversum, 1994).

phical, but in the concluding chapter the use of family strategies in empirical studies is warmly applauded. Notice, however, that the question mark in the title testifies to our hesitation. This did not prevent us from working hard on a set of empirical studies. In 1997 this collection of papers was published in *The History of the Family*.¹⁰ Our pretensions were modest. The articles do not answer all the questions, it said in the Introduction, “they just contribute to the body of knowledge that in the end will result in a convincing theory on family strategies”. Again, however, you will find a question mark in the title: “Structure or Strategy?”.

Following this project, four collaborators from the N.W. Posthumus Institute aimed at a real test of the concept in a set of case studies in the Netherlands and Belgium.¹¹ To be more precise, the focus of their work is on strategies of families related to labour and income. What they are looking for, basically, is whether or not families and individual family members act as if they are maximizing their “joint utility”. The first step is to detect relationships between the family cycle, allocation of labour, income, and migration. Next, the authors ask whether this relationship can be interpreted as the result of a family strategy. The case studies all use quantitative databases, mostly at the micro level, and they all try to answer questions at three different levels: (1) how did families adjust to their changing social and economic *context*?; (2) how did families regulate the *internal* allocation of labour? And (3) what role did *individuals* play within these family strategies? The actual studies cover several subpopulations within the Low Countries. Ad Knotter focuses on marginal dockworkers in the Amsterdam harbour in the first decades of the twentieth century; Richard Paping’s research uses data on families in the Groningen clay district between 1830 and 1920; Jan Kok’s contribution to the volume deals with the families of actors born in the province of Utrecht between 1812 and 1912; and, finally, Eric Vanhaute uses information on the economic and demographic characteristics of all households in two Belgian villages for the years 1846 and 1910.

In my view, the results of these studies, to be published shortly also in English, are undoubtedly impressive. Our knowledge of the populations, cities, villages, and regions included has increased markedly and, certainly, our insight into the late nineteenth- and early twentieth-century behaviour of the Dutch and Belgians has improved. However, that is not the issue in this paper. Here, the more interesting question is: do we know more about family strategies after these studies have been completed?

Let me first quote the authors themselves. Do they think that the

10. M. Baud and Theo Engelen, “Introduction: Structure or Strategy? Essays on Family, Demography, and Labor from the Dutch N.W. Posthumus Institute”, *History of the Family*, 2 (1997), pp. 347–354.

11. Jan Kok *et al.*, *Levensloop en levenslot. Arbeidsstrategieën van gezinnen in de negentiende en twintigste eeuw* (Groningen [etc.], 1999).

concept was helpful? After studying the Groningen area, Richard Paping complains that data problems prevented him from using the concept properly. He was only able to analyse the actual behaviour, not the deliberations and motives behind it. The great advantage of thinking in terms of strategies, though, he adds, is that the researcher is forced to look for the individual motives of families. Jan Kok has reached the conclusion that “the theory on family strategies appears to be a fruitful approach to see many aspects of migration and leaving home in a new context”. This sounds very careful indeed. He also points out the many requirements the data has to meet in order to reach really convincing conclusions. In the end, this author warns, we have to be constantly aware of the fact that the strategies we are looking for are nothing more than hypothetical constructions. It is interesting to notice that in Ad Knotter’s conclusion the expression *family strategy* is not even mentioned once. The closest he gets is when he points at the joint utility function of the new home economics. Eric Vanhaute is realistic in assessing the possibilities and problems of the concept. We shall never be able really to reconstruct the actual decisions made in families of the past and by historical actors, he argues, but we do reach interesting conclusions when we look at collective processes caused by the interaction between structural changes, on the one hand, and family choices concerning labour, on the other.

These Dutch authors are not the only ones struggling with family strategies. From the very beginning, scholars were not only aware of the possibilities of the concept, but also of its ambiguities.¹² From 1994 on, we have collected within our research programme a long list of questions to be answered and problems to be solved. They can be divided into two clusters. The first deals with issues concerning the application of the concept. The second cluster is more fundamental and questions the very core of family strategies.

If one accepts family strategies as a viable theoretical assumption, there are a few potential pitfalls when using it in empirical research. First, the use of the strategy concept carries the danger of exaggerating the *freedom of human actions*. Once puppets are replaced by individuals playing within the structures, we still have to be aware of the strength of contextual

12. Among many others: G. Crow, “The Use of the Concept of ‘Strategy’ in Recent Sociological Literature”, *Sociology*, 23 (1989), pp. 1–24; R. Edwards and J. Ribbens, “Meandering Around ‘Strategy’: A Research Note on Strategic Discourse in the Lives of Women”, *Sociology*, 25 (1990), pp. 477–489; Leslie P. Moch *et al.*, “Family Strategy: A Dialogue”, *Historical Methods*, 20 (1987), pp. 113–125; Phyllis Moen and Elaine Wethington, “The Concept of Family Adaptive Strategies”, *Annual Review of Sociology*, 18 (1992), pp. 233–251; D.H.J. Morgan, “Strategies and Sociologists: A Comment on Crow”, *Sociology*, 23 (1989), pp. 25–29; M. Shaw, “Strategy and Social Process: Military Context and Sociological Analysis”, *Sociology* 24, (1990), pp. 465–473.

variables. No actor, be it historical or contemporary, can make completely autonomous decisions.

The second problem observed is the *preoccupation with the economic aspects of strategies*. Sociologists have pointed out the cultural motives behind the behaviour of the actors involved.¹³ There is a role for historians here. Tamara Hareven even mentioned that the most important contribution of historical research has been to emphasize that strategic choices were guided by cultural factors too.¹⁴

The next major methodological problem deals with the time perspective. Strategies may aim at long-range goals. In that case they involve investments in education and the professional career of parents and children, the decision to migrate, or to save money. The results can be measured by looking at intra- or intergenerational social mobility. On the other hand, strategies may be rather straightforward coping mechanisms in case of sudden disasters. Obviously, both strategies may overlap or even counteract each other. How do we disentangle the results from behaviour directed at long-term and those of short-term goals?

In the second cluster of problems related to family strategies the concept itself is seriously challenged. First of all, one has to deal with the expression "strategy". By implication it suggests that historical actors were *consciously* choosing from a set of options and, thus, that they were guided by *rationality*. Obviously, this is not always the case. Much of what human beings do is the result of customs, tradition, and unconscious motives rather than of rational calculations. When trying to detect strategies behind observable behaviour, we have to be aware of this problem. This is especially pressing for historians. Most often, they cannot interview the populations they are studying. As a consequence, they have to deduce the underlying motives from the behaviour these motives resulted in. Interpretations then become very complicated, because, as already mentioned, historical actors may have been acting unconsciously.¹⁵

We have the example of the so-called western European marriage pattern, as described by John Hajnal. The characteristics of this pattern are well-known: late age at marriage and a high level of celibates. To be sure, this marriage restriction may have indeed been the rational choice of the

13. R. Friedland and A.F. Robertson (eds), *Beyond the Marketplace: Rethinking Economy and Society*, (New York, 1990), pp. 23–24; Crow, "The Use of the Concept of 'Strategy'", pp. 14–15; Morgan, "Strategies and Sociologists", pp. 27–28.

14. Tamara K. Hareven, "A Complex Relationship: Family Strategies and the Processes of Economic and Social Change", in Friedland and Robertson, *Beyond the Marketplace*, pp. 215–244, especially pp. 219–220; see also: Tilly, "Individual Lives and Family Strategies", pp. 137–138.

15. Hareven, "A Complex Relationship", p. 218; E.A. Wrigley, "Fertility Strategy for the Individual and the Group", in Charles Tilly (ed.), *Historical Studies of Changing Fertility* (Princeton, NJ, 1978), pp. 135–154, especially p. 148.

actors involved after considering the availability of niches. If this was the case, we can indeed call it strategic behaviour. If, on the other hand, this marriage pattern is rather a mechanism at the level of society to keep population within limits, we will not find individual strategies. Mostly, such society-level rationality was translated into cultural norms. In this case, then, historical actors only lived by the norms and values concerning the right moment or the proper age to marry, probably unaware of the macrorationality behind these norms.¹⁶

Inferring motives from resulting behaviour poses yet another set of problems: what if the motives were geared at reaching goals that are difficult to measure, like more freedom or challenges? Also, what if the actor did not have the right information on either his situation or his possibilities? And how do we deal with behaviour that was not intended at all, but was the result of changing circumstances or failure to reach what one really intended to reach?

When families are not only treated as the context in which individuals make their decisions but as the actual decision-makers themselves, the conceptual and methodological problems are even bigger. Crow asked bluntly whether “collectivities such as households and families can be treated as social actors”.¹⁷ And from a feminist point of view there obviously is a sceptical reaction to treating households as a harmonious unit.¹⁸ Dealing with family strategies, therefore, also implies looking into the power relations *within* families. What positions did women have? Did strategies reckon with the consequences for the children involved? For this very reason family economists replaced the *joint utility function* by so-called *bargained family decisions*. In this way they created room for the conflicting interests of family members. One has to find a balance in which the interests of both the individuals and the family are taken care of. Obviously, this only adds to our interpretational problems when studying family strategies. In other words: whose strategies are we talking about when we deal with family strategies?

Lastly, the danger of circular argumentation is never far away when we use the family-strategy concept. Most historical studies do an excellent job in gathering data on family behaviour. Next, these data are processed in cross-tabulations, graphs and multivariate analyses in order to distinguish subgroups with behaviour that deviates from the average, including the covariates of this behaviour. So far, this is nothing more than straightforward

16. Theo Engelen and Arthur P. Wolf, “Introduction”, in Theo Engelen, François Hendrickx and Arthur P. Wolf, *Marriage and the Family in Eurasia: Perspectives on the Hajnal Hypothesis* (Stanford, CA, forthcoming).

17. Crow, “The Use of the Concept of ‘Strategy’”, p. 6–7.

18. N. Folbre, “Family Strategy, Feminist Strategy”, *Historical Methods*, 20 (1987), pp. 115–118; L. Morris, *The Workings of the Household: A US–UK Comparison* (Cambridge, 1990), pp. 17–21.

historical research. Only when one wants to link these behaviour patterns to plausible strategies of the actors involved, there is a methodologically risky step involved. Basically, we deduce strategies from behaviour and afterwards use it to explain this behaviour.¹⁹

The methodological problems listed here are not new. They have often been discussed already in theoretical studies on the concept of family strategy. The best test, however, comes from down-to-earth empirical research. Therefore, I have selected a few examples from the four case studies in the Netherlands to illustrate the ambiguities one encounters when adopting the strategy concept. In order to show that other researchers face the same problems, I have added two other examples from recent empirical studies using the family-strategy concept.

Richard Paping states that the strategies of agricultural labourers in the province of Groningen changed markedly in approximately 1900. One of the measures he uses is the occupational choice by children. In the second half of the nineteenth century, he argues, children were sent into service as early in life as possible. In the short run, this implied an improvement of the family's financial position by both raising the income and cutting the costs. Another table, however, shows that in the long run the prospects for children who stayed at home were significantly better than for their counterparts working as servants. Thus, in this case, the short-term strategy blocked a long-term strategy. Paping considers this to be a *choice* out of sheer necessity. In his view, this situation changed by 1900. The income of labourers by that time had reached a level where they could keep their children at home and thus could aim at long-term goals.

To be sure, there is nothing wrong with Paping's statistical analysis of the data at his disposal. The interpretation in terms of strategic actions, however, involves choices that are open to debate. First of all, in this example it is taken for granted that the decision on whether or not children were sent out as servants was a rational and conscious choice. That need not have been the case. In many instances, people simply act according to the example set by their peer group. It is not hard to imagine that the poorer segment of the inhabitants of Groningen formed such a sub-population. In such a case, sending children into service is a tradition rather than a strategy. Also, can we accept that the Groningen labourers in those days had the same information at their disposal as the researcher has nowadays? Paping makes a cross-tabulation which shows that having been a servant diminishes the chances of upward social mobility. Only if this was known to the actors there and then, can we speak of a strategy.

Finally, Paping's account suggests that all decisions made were fully directed at improving the situation for the family as a whole. There seems

19. J. Goudsblom, "Rationele en andere keuzes. Kanttekeningen bij het rationele-keuzemodel", *Amsterdams Sociologisch Tijdschrift*, 22 (1996), pp. 620–630, especially p. 622.

to be no room for individual strategies, for instance by the children involved. After 1900, a growing proportion of these children stayed at home. Again, this decision is presented as the result of a grand family master plan. The implicit obedience of the children involved is remarkable, the more so while contemporary reports complain about the very lack of obedience. Even the conclusion that children who went into service had a smaller chance to escape the status of a labourer is debatable. Could it not have been the case that families with a higher income were able to educate their children better? Children who went into service, in this view, did not lose prospects by becoming servants but simply because their parents were poor. Anyway, what room was there for actors to think of strategies in a situation where they probably were simply the victims of structural poverty? And indeed, by 1900 this period of severe poverty had ended. As an alternative to Paping's conclusion, one could speculate that the strategies – if they existed at all – did not change about 1900, but rather the possibilities to realize these strategies. This alternative explanation would imply that after 1900 Groningen labourers were playing the same game, but within changed constraints.

Most of the remarks made on Richard Paping's contribution apply to the other studies too. Jan Kok, for instance, concludes that migration was not a strategy aimed at upward social mobility. His reasoning is straightforward: at the age of fifty those who had migrated were not better off than their sedentary colleagues. Again, the researcher has data at his disposal that most of the people he studies did not know. Kok may very well be right when he draws the conclusion that the subjects of his enquiry only migrated to get out of a miserable situation. Still, they may also have had the distinct impression that migration would improve their position. The fact that it did not work out that way does not make it less a strategic intention.

In Ad Knotter's account of the Amsterdam dockworkers we find another example of what often is called "an economy of makeshifts". The income of the dockworkers came from earnings on one hand and a set of social security funds on the other. Married women, we learn, only worked when there were no alternatives at all, mainly at the beginning of the marriage. As soon as the children were able to earn an income, they replaced their mothers on the labour market. The question to answer here is: do we need the concept of strategy to explain why those family members who had the highest income possibilities actually worked? Or why married women only worked when all other possibilities failed? On the other hand there is one strategy missing. Given the fact that all these families constantly faced the danger of primary poverty, why did they not really maximize their income by having both women and children work?

The same line of argument can be followed in assessing Erik Vanhaute's main conclusion. After comparing the censuses of 1850 and 1910, he finds

that the younger generation, growing up when industrialization started, married younger and had larger households. Again, I find it difficult to accept strategic motives behind the changes in behaviour. Basically, there was no longer enough land to go round. The alternative was found in factory or proto-industrial labour. At the same time, the old norms and values concerning the proper age at marriage faded because they were not grounded in the new economic reality. My objection to implying a strategy behind these changes is based on the idea that a strategy by definition implies a choice from several options. When there are no alternative options except the actual behaviour we find, it is hard to call this a strategy.

As already mentioned, other researchers run into the same problems when trying to apply the promising concept in actual empirical research. In the volume edited by Laurence Fontaine and Jürgen Schlumbohm we find an admirable and thorough contribution by Dennis Frey on survival strategies of German artisans in the mid-eighteenth and early nineteenth centuries.²⁰ The author uses probate inventories to investigate the strategies used by poor artisans in the city of Göppingen. From their net worth, he concludes that in the period studied the wealth of the middle class, and especially of the upper stratum, grew significantly, whereas the possessions of the lower classes stagnated. Still, when studying the inventories of the families included, Frey found the poorer households to have had fashionable items among their possessions. From this empirical finding he derives strategic actions among the smaller artisans. In his view, they lost the economic competition with their wealthier colleagues, but compensated for this by acquiring clothing, jewellery, and furnishings according to the latest fashion, even if this meant augmenting their debt burden. Frey considers this to be an investment in social and cultural capital and therefore as a valid strategic goal of the artisans. To be sure, the author himself already points to the fact that “precise motivations and reasoning are not self-evident in the inventories”. Again, the researcher uses the outcome of the behaviour of historical actors to assign them conscious strategies without presenting direct evidence. Is it not possible that these artisans just desperately tried to hold on to their traditional lifestyle as long as possible?

Rebecca Jean Emigh set out to find strategies among fifteenth-century Tuscany households using the ideas on the topic by Chayanov, Bourdieu, and Weber.²¹ This author too provides us with an excellent overview of the theories of the three famous scholars and presents a profound empirical study of two rural Tuscan communities. Here, we shall only use the parts

20. Dennis A. Frey, “Industrious Households: Survival Strategies of Artisans in a Southwest German Town during the Eighteenth and Early Nineteenth Centuries”, in Fontaine and Schlumbohm, *Household Strategies for Survival*, pp. 115–136.

21. Rebecca Jean Emigh, “Theorizing Strategies. Households and Markets in 15th-Century Tuscany”, *History of the Family*, 6 (2001), pp. 495–517.

dedicated to Chayanov. From his theory she uses the finding that the output per worker must be higher when the dependency ratio is high and vice versa, simply because the cost of labour is never imputed. The Tuscan sources provide data to link output per worker and the composition of the household. As it is, the correlation coefficient for 100 cases between output and dependency ratio is 0.458. This shows that indeed many households were following Chayanovian household strategies. On the other hand, this conclusion leaves us with at least two questions. The first question has to do with alternatives to the strategy chosen. If members of large households had to work harder in order to feed also the dependent family members, can we call this a strategy or is it simply fate? Secondly, one also has to deal with the households not conforming to the theory used here, that is, households with either a high dependency ratio and a low output, or a low dependency ratio and a high output. This Emigh explains by pointing out the opportunities for profit and the accumulation of property and surplus on local markets. In this way a Weberian strategy is introduced. The households adopting Chayanovian strategies are simple budgetary units, whereas the other families use a Weberian profit orientation. For me, the puzzling conclusion appears to be that every household was following a strategy, or, in other words, that there is a strategy for every possible empirical finding.

It is time to make a few concluding remarks. After all is said and done, my original hesitation concerning the family-strategies concept has not disappeared. On the contrary, both methodological reasoning and empirical tests have strengthened my doubts about the applicability of the concept. I noticed, for example, that Laurence Fontaine and Jürgen Schlumbohm in their recent volume did not use a rigid definition of the concept for the authors. Rather they “welcomed individual contributors exploring a variety of approaches and aspects”,²² and presented a list of methodological problems. That is exactly what we did ourselves in the 1997 volume and what so many others have done before us. The line of argument always seems to be that it is very difficult to operationalize “family strategies”, but that it will be done anyway. Still, if we lack a clear and convincing definition, and if there are so many methodological problems with the concept, why do we still use it?

The answer to that question is probably to be found in the history of the concept itself. Through authors like Bourdieu, Giddens, Tilly, Hareven, and many others, social historians once again have discovered the importance of individual historical actors and how they tried to find their way within often very pressing structural constraints. With the benefit of hindsight it is hard to believe that only thirty years ago this was a

22. Fontaine and Schlumbohm, *Household Strategies for Survival*, p. 10.

revolutionary development. Now that agency regained its rightful position when looking at the past, we may wonder whether we still need family strategies as a tool. In the end, its contribution to our way of working has been that we take into account again that historical actors themselves may have influenced their lives. After reaching that point, however, the concept creates more problems than it solves.

Does this imply, then, that I negate the opportunity for historical actors to act according to individual or group strategies? Absolutely not. The problems start when historians try to (re)construct these strategic intentions from the results they generated. These results may be very misleading. As already mentioned, the actors may have had inadequate knowledge of all possibilities open to them, their subconscious motives may have influenced their decisions, their strategies may have failed etc. All these biases from the side of the historical “sender” provide reason enough to be very careful indeed when trying to describe (family) strategies. The contemporary “recipient”, however, adds another important reason. There is no way to retrieve and disentangle the complicated jumble of motivations and deliberations, the more so because we have to deduce them indirectly from the resulting behaviour.

Some colleagues might reply that the “projection of motives” is the inevitable fate of historians. They have always attributed motives to the actors they studied. Indeed they have, but their pretensions were more modest. Anyway, this might be exactly the reason why the scientific character of history is subject to endless debates and why the Dutch historian Pieter Geyl has referred to it as a discussion without end. The subjectivity of his interpretation is the historian’s “original sin” of which he is well aware. For that reason he carefully infers possible motives from past human behaviour, in most cases also pointing out the shortcomings of his interpretation and identifying alternatives. The problem I have with the concept of “strategies” is that it claims to be able really to reconstruct the personal deliberations of historical actors or families. In my view, this is impossible and, therefore, we should avoid the suggestion that it is.

What is the alternative? From historical data we can extract statistical and qualitative evidence of human behaviour in the past. We know that this behaviour is based on several components: structural constraints, individual or group strategies, and, finally, simple coincidence. The nature of the strategies, however, is beyond our grasp. We do not know whether it was a conscious or an unconscious strategy; we do not know whether it was a collective or a bargained strategy; we do not even know whether the actual results we find were the results aimed at. They may simply be miscalculations of the actors involved, or the result of several conflicting individual strategies ending in results that not one of the participants actually aimed at. Still, this leaves us with very informative statistical trends indicating how in certain historical periods certain social groups in

certain circumstances tended to act. That is very interesting historical knowledge, even if we do not know exactly what the purpose of the individual actors was. By adding many individual decisions into one average behaviour including its co-variates we might even venture ideas about the reasons behind these trends, the more so if we use statistical methods to measure the impact of certain well-described independent variables on the subject we are trying to understand. Again, all this advances our knowledge of the past without overcharging our scholarly possibilities.

My conclusion, therefore, is straightforward: family strategy has been a useful concept in the sense that it directed our attention to human agency again, but now that it has accomplished that I see no function for it any more. By focusing on strategies as an official concept we *may* exaggerate the agency of historical actors, but we *certainly* exaggerate the possibility of our reconstructing those strategies from the resulting behaviour.