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How Far Reaches the "Middle Range" of a Theory? A Reply to the Comments

(doi: 10.2383/32059)

Sociologica (ISSN 1971-8853)

Fascicolo 1, gennaio-aprile 2010

Ente di afferenza:

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How Far Reaches the “Middle Range” of a Theory? A Reply to the Comments

by Hartmut Esser

doi: 10.2383/32059

The function of theories is to specify the “generating mechanisms,” which explain a subject one is interested in. Explaining mechanisms necessarily have to refer to one or more “laws,” which allow for the causal connection of certain (initial) conditions with the effects to be explained. Such mechanisms and laws must reach beyond the case at hand and should be as general as possible. They *always* represent more than a mere generalization of empirical regularities and an explanation *always* involves more than the (detailed) description of single cases, (historical) courses, or the covariation between events. Against this background, do Classical Assimilation Theory (CAT), the Theory of Segmented Assimilation (TSA), or the New Assimilation Theory (NAT) then really constitute “theories”? The CAT has generalized trends of the U.S.-American immigration history and claims this generalization to make up a (“universal”) law. The TSA has extended the possible structural outcomes of integration processes and explains them by varying conditions and processes. Finally, the NAT points to the fact that assimilation also occurs under the changing conditions of the “new” immigration. One can find without doubt many indications of causal relations and generating mechanisms in each of the three approaches. The CAT with its cycle models, for example, refers to the emergence of ethnic segmentations as a consequence of closures within the receiving country due to an increasing competition between immigrants and natives. The TSA develops the hypothesis that contact with host society’s deviant subcultures will hinder the social advancement of immigrant children if families and networks fail to meet this risk. And the NAT refers to the

lack of competition on the labor market as a condition for the social advancement of following generations ("non-zero-sum-mobility"). While none of these proposals is implausible, the specification of the respective comprehensive causal relation making the special hypotheses understandable is mostly missing.

Here, as in many other cases, the connection seems to be trivial and the appropriate (general) law unnecessary. However, this is (mostly) by no means the case. In the first instance, the matter appears to be indeed quite simple: Social distances restrict opportunities for efforts towards integration *in general*, lacking contact to certain contexts results in the exclusion of options *in general*, and already occupied positions limit the chances for mobility *in general*. The underlying logic is extremely simple and reliable: If opportunities for actions are *objectively* restricted, actors will have *no* "choice," and a theory of action is apparently unnecessary. This is why the explanation on the basis of limited opportunities works so well – and this paired with extremely low levels of theoretical complexity and further empirical preconditions. Therefore, not only a theory of the integration of immigrants should start from this broadest framework for the explanation of social phenomena – and the examples described above demonstrate that this indeed often makes up the explanations' implicit core. The high parsimony is the great advantage of Peter M. Blau's approach, which forms the essence of the model of intergenerational integration: Group sizes as *structural* restrictions determine objectively and firmly what is actually *possible* in terms of intergroup relations. And if nothing further happens, this will control the social processes in such a way that one does not have to pay special attention to any further details.

However, things are, of course, (mostly) not that simple: Actors do always, in principle, have certain options within the respective objective limits, and for explaining their "choice" one now needs a general law and, hence, a theory of action. A quite simple variant of such a theory is, of course, the expected-utility-theory. According to this theory, the product of *objective* success expectations and *objective* gains determines the actual single act (and Blau's opportunity theory would represent a simplifying special case of it). But again things are often more complicated and more difficult: Actors by no means always follow the objective guidelines, but rather consider their *subjective* expectations and evaluations or, in Max Weber's words, their "subjective meaning." The action theoretical response is the *subjective*-expected-utility-theory, which explicitly refers to *subjective* expectations and gains. And this is not yet the end of the story: The subjective conceptions are products of an interpretative "construction" and "definition" of the situation in terms of which cultural and normative demands as well as the embeddedness in social contexts and interactions play a major role. In the meantime, there are also useful theoretical models available

on this point. As one can well imagine, these models are yet much more complex (see below).

One can immediately see: The matter becomes much more complicated, much less “objectively” comprehensible, and one would actually have to consider in detail each single case and each single sequence of the connection between objective situation, subjective interpretation, definition of the situation, and the respective single act (and its structural outcomes). But that is still not enough: The various processes mostly refer to whole chains of path dependencies and “situational logics,” which eventually account for the connection between a certain structural condition and a structural outcome. For example: Follow-up immigrations give rise to competitions, which trigger closures and by that provoke ethnic conflicts (CAT). An averting host society entails the displacement of immigrants in inner city quarters and their contact to “wrong” segments of the society. This process cannot be absorbed by families who lack ethnic social capital and finally results in “downward assimilation” (TSA). As compared to the first generation, the second generation has structurally much better chances for interethnic contacts due to their earlier and longer stay in the host country. Such interethnic contacts facilitate acculturation to the educational system as a prerequisite for social advancement (NAT and CAT). Yet, this does not apply to all cases. It rather presupposes the presence of special conditions, which are mostly assumed to be present in the background and are not further specified. For example: The absorption of following generations in the host society soon dissolves ethnic communities again; it may well be possible that the values held by families and immigrant networks do not protect against the impact of deviant milieus; the second generation has no access to interethnic relations due to, for instance, external closures or membership in closed ethnic networks. In addition, the various processes and conditions may mutually influence each other, and depending on the respective constellation other courses and structural outcomes may well be possible. This impression gets even stronger in view of the fact that everything changes continuously and that completely new historical situations arise, which apparently cannot be grasped by the available models.

The mentioned theories (CAT, TSA, and NAT) have not put up with it, each in its own respective way. They all have tried to overcome the mere description of cases and courses and to get closer to a theoretical systematization. However, a problem soon arose: *Each* of the three theories had its exceptions and each could present empirical evidence in support of its hypotheses, at least under certain historical and social conditions, which mostly only became discernible at a later time. This also holds true for the CAT. Unsurprisingly, there were and still are the well-known controversies on the range and validity of the single theories. The model of intergenerational

integration was the attempt to reconstruct the various theories of "middle range" by means of a comprehensive logic in such a way that one can finally identify in which case each can claim its validity. The problem becomes directly obvious: If one *cannot* consider every single theoretical possibility, every single case, every possible condition, and every variant of a situational logic, one will *have to* accept a simplifying generalization. The only question is then: How far can such a generalization go? And what can be done to meet the – inevitable – objection: "But everything is actually different! And clearly much more complicated!"? This objection makes up the core of the three comments, even though at least two of the contributions explicitly acknowledge the value and advantages of the proposed model: The model was far too abstract and one had still to do the chief part of the work; the action theoretical core was unnecessarily narrow; the assumptions and references made by the model, in particular those in terms of the group size effects, were unfounded; and the whole apparatus was useless, because the central integration processes followed a different logic than the one proposed by the model with its focus on group sizes, opportunities, and resources.

Richard Alba, in particular, believes that despite all advantages the price for the model's *level of abstraction* was too high: the far too heroic simplifications common to all "simple economic models," like the actors' "perfect information" or the perhaps far too simple dichotomy in terms of the differentiation of the options into receiving-country- and ethnic-context-alternatives [Alba 2010]. The model, thus, hardly allows for considering the many further heterogeneities, which the other theories address in their details and which are relevant to the integration process, like those in terms of the immigrant families' socio-economic position or their living environment [Alba 2010]. Similarly, Michael Eve thinks that the immigrants' options were clearly more heterogeneous, that the references for integration varied much more than the model assumes, and that the generations' circumstances differed from the proposed ones [Eve 2010]. Debora Mantovani and Giuseppe Sciortino point out that the status quo option did not automatically refer to the ethnic context and that not all ethnic resources would represent "specific" capital [Mantovani and Sciortino 2010]. In particular, so again Alba, the model failed to account for the influences of the wider social environment and historical development, but rather focuses on the modeling of endogenous processes in the context of immigration and integration [Alba 2010]. The model had, thus, "something of an ahistorical and context-free character" and without considering these specific external influences one could not understand the processes of, for instance, the diminishing social distances in the course of the economic developments after World War II that made the non-zero-sum-mobility possible [Alba 2010]. The main problem, however, was the translation of the basic func-

tions' courses into the empirical circumstances – and not precisely the question about the nature of the basic mechanisms. There was (thus) a clearly too great distance between the model's abstract parameters and the empirical processes [Alba 2010]. Mantovani and Sciortino regret that the *action theoretical basis* of the model was limited to the “rationality” of an investment behavior. The model would not lose any of its analytical potentials if it included “a thick notion of *reason* instead of its emphasis on rationality” [Mantovani and Sciortino 2010, emphasis by H.E.]. Only then one could take into account that situations cannot be grasped “objectively,” but are rather only interpreted and defined through their cultural and normative embeddedness: the context of the migration decision, for instance, within the scope of a “culture of emigration”, the group size, which cannot be reduced to mere numbers and statistical distributions, the “construction” of ethnic boundary making, and the whole process of “replenishment” being at the heart of the model of intergenerational integration [Mantovani and Sciortino 2010; similarly Eve 2010]. Above all in Eve's comment one can find objections against the *basic logic* of the model as a whole. The fundamental mistakes were, accordingly, the orientation along ethnic groups as mere aggregates (that is along statistically produced numerical data on individual actors who share the same national background and who have no relation with each other), the enumeration of these aggregates in the abstract and context-free category of group size, and the assumption that this was a relevant condition for the integration process and for the equipment with resources. Not the varying numbers of individualized “co-nationals” were important, but rather “kin or friends or members of the same social circle.” [Eve 2010]. And “the only numbers which might be relevant would be those in kinship and friendship networks” [Eve 2010]. All processes specified in the model are “understandable much more simply just by reflecting on the *normal* mechanisms underlying the formation of social networks (whether of members of a minority or a majority).” [Eve 2010, 5; emphasis by H.E.]. Yet, this was “a logic and a set of choices which is quite different from those imagined in Esser's model.” [Eve 2010, 3]. In short: One not only does not need the model, it is also badly misleading.

A response to these objections is impossible without pointing to some basic methodological principles, which particularly have something to do with the model's (too high?) level of abstraction. By this I refer to a general methodological problem in social science theory construction, which has determined the debates from the very beginning up to today: Are “general” theories and explanations actually possible (and meaningful) in social sciences, or would one have to restrict oneself to historically and socially specific fields and to cautious generalizations of “middle range,” if one wanted to address an issue properly – if not only theoretically “open” and “dense” descriptions of single cases were allowed and meaningful anyway? This dilemma in

terms of the payoff between abstraction and parsimony on the one hand side and proximity to reality and complexity on the other hand side was deliberately developed in more detail and step by step above. And the problem is obvious: No general and applicable rule exists, which could determine in advance when a generalization is still justifiable and when it goes too far, because *any* generalization involves exceptions and assumptions that are not fully met. This, by the way, also applies to the ever so "dense" description of an ever so special single case, as already the notions and categorizations used contain certain generalizations. The problem is further intensified by a closely related dilemma: The more parsimonious and the more general a model is, the higher is its explanatory power and (yet) also the higher is its risk of becoming falsified. For this reason, one should always prefer more general and more parsimonious models to theories of a lower range – provided that they solve the respective explanation problem and that their further implications do not turn out to be empirically wrong. As long as a – ever so abstract – model solves the explanation problem in question, however, there was *no* reason to change it. While one could always question the one or the other *assumption*, this would be – to tell the least – unnecessary in this case and, moreover, not advisable as long as there was no alternative model with an equal explanatory power available (for the time being). Things will change, if the model's *implications* prove to be wrong: Then there must be a mistake in at least one part of the model and one would have to correct it.

None of the contributions claims, however, that the problem at issue – the explanation of the three theories' conditional validity (CAT, TSA, and NAT) – has not been solved. This also holds true for Eve's otherwise and throughout extremely critical contribution. The criticism rather refers to certain *assumptions* in the construction of the model, like the explanation of ethnic boundary making or the conditions that various generations might actually face. There is (for the time being) virtually no founded reason to change something in the construction of the model itself. Nevertheless, one cannot (or should not) ignore the objections made, be it only to be prepared in case that the model finally fails to prove its reliability. In addition, if one is not only interested in "functioning" formal constructs but in the empirical processes, which the model formally reconstructs, one is well-advised to refrain from the instrumentalism of the "as-if"-modeling that is (still) common among, for instance, economists or (some) rational-choice-sociologists above all. Against this background I wish to address three related questions raised in the comments in more detail. They refer to the bridging from the model's abstract constructs to the empirical processes, the extension of the action theoretical basis by introducing the "interpretative" dimension, and the question about the significance of group sizes for (intergenerational) integration.

There is no doubt that filling the abstract constructs of the model with empirical content is a basic condition for any theory construction and also that it is exactly here, where the chief part of the work emerges and where the devil is in the details as Richard Alba correctly states. Yet, this problem occurs in *any* theoretical modeling, and it is also crucial here to keep the respective necessary bridging as simple as possible. This presupposes, at first, the specification of what has to be actually bridged and empirically specified. Starting from the assumption that the heart of the integration process refers to the situation-oriented acts of actors (and their structural outcomes),¹ one initially needs to select a theory of action that can actually be related systematically to certain structural characteristics of situations. Blau's opportunity theory is the simplest variant of such a theory, but, as was already mentioned, it only considers (structural) restrictions. Thus, for explaining action within restrictions, a theory of "choice" becomes necessary. The first theory that seems to be appropriate here is the expected-utility-theory, because it is the simplest one (as regards content) and because it considers also opportunities and preferences in addition to restrictions. Only in case that it proves not to work, one would have to take a further extension into account. This similarly applies to the differentiations addressed in all contributions, like those regarding the various options for action. Here, one can start from the simplest case as well: dichotomous alternatives referring in the first instance to completely undifferentiated spheres, like "the" host society or "the" ethnic community. But again the rule is: Only in case that the model fails to work (or if one examines other conditions and/or new explananda), a (systematic) reason for a further differentiation will arise: additional options (not only investment in rc-capital, but also utilization of ethnic capital or the attempt to reevaluate ethnic capital through "political action," for example), different segments (of the host society and/or ethnic group), or other types of actors who may be involved in the process (like, in addition, elites, further types of

¹ The model of intergenerational integration explains certain structural outcomes as (mostly: unintended) effects of *individual* actions of actors who orient themselves along socially *structured* situations. It, hence, connects the macro-level of social structures with the micro-level of individual actions and back again with the macro-level, possibly in longer chains of whole sequences [see explicitly on this point Esser 2010, 14f. and Figures 2a, 2b, and 4 there]. In this respect, the model by no means places "migrant's attitudes and strategies" into the foreground as, for example, Eve believes [Eve 2010]. And there is also no "tension", because it started from the individual level but then referred above all to "group outcomes" as Alba objects [Alba 2010]: The *collective* "group outcomes" are explained as (unintended) effects of a structured action of *individual* actors. The "mixture" of individual assimilation and collective segmentation, which Alba mentions, represents a simple special case of the model: There exists a specific equilibrium of new immigrations and absorption, which accounts for the fact that ethnic communities continue to appear as being ethnically segmented due to the replenishment by new immigrants who are (still) not assimilated. Simultaneously, there are high rates of absorption at the individual level indicating the high number of immigrants who leave the ethnic community. The contribution also refers to this point [Esser 2010].

potential immigrants or generations). The problem already described above arises at all events: It is not quite difficult to extend the actors' options, to further differentiate the options' references, or to increase the number of the types of actors involved in the process.² The difficulty – and the “art” – lies rather in the *reduction* of a model's complexity: Just in view of the ever increasing effort connected with the modeling, one should strive for assumptions that are as simple as possible, while also being able to cope with a maximum of differentiations in the model as special (initial) conditions and making correct predictions. The rule could be: As simple as possible, as differentiated as necessary! This by no means implies that a detailed empirical work was unnecessary. Quite on the contrary: The empirical work makes up the basis for all abstracting simplifications – and an essential fund of knowledge in case that one has to redesign the model in a more differentiated way or change it completely after all.

This all applies to Debora Mantovani's and Giuseppe Sciortino's suggestion to consider “a thick notion of reason” instead of using the expected-utility-theory in the model, too. Yet, already using the *subjective*-expected-utility-theory instead would have made the model clearly more complicated. Moreover, as the model has proved, at least in the first instance, to be overall suited for integrating CAT, TSA, and NAT into one framework, there was no real (methodological) “reason” for this extension. Mantovani and Sciortino do not specify which alternative, analytically precise “interpretative” theory of action they have in mind. This is not really surprising: Despite the meanwhile many attempts and suggestions, even in the field of (experimental) game theory [see, for example, Bicchieri 2006, 55-99], there exists hardly any sufficiently precise formulation of the mechanisms governing the “definition” of a situation and explaining a specific act by the “interpretation” of symbols, culturally determined mental models, and normative conceptions, also in interaction with (rational) expectations and evaluations [for an example of such a more recent, yet also inappropriate, attempt see Gross 2009]. One of the few analytically sufficiently precise theories of action that include cultural symbols, categorizations, and the “definition of the situation” would be at best the model of frame selection [Esser 2001; see Kroneberg, Yaish, and Stocké 2010 for a recent version and application of the model]. Yet: The model would have become again much more complicated, and as it obviously manages to solve the problem at hand without this extension, one should stick to the still relatively simple expected-utility-theory – be Mantovani's and Sciortino's warnings

² One could model such differentiations quite easily by introducing additional functions that run parallel to the basic functions. Examples may be the higher chances for integration due to higher education or an open immigration policy. This would also apply to historically specific or single events, like a change in immigration policy, the trend on labor markets, or September 11, 2001 with all its consequences. The basic relations would in any case remain unchanged.

(for example with regard to the explanation of the “definition” and “construction” of ethnic boundary makings at which the model of intergenerational integration only hints without any further action theoretical specification) ever so plausible and important.

Michael Eve’s objections go a big step further. They not only refer anymore to possible extensions and differentiations, but aim at the heart of the model: Integration had nothing to do with group size as a mere statistical summation of isolated individuals with a “particular nationality”. Rather, social relations alone and “normal” mechanisms of network formation were important. Indeed, one cannot deny that social relations and networks are significant for the integration process. It is also true that numerical group relations have less to do with immigrants’ motives and opportunities than conditions within the near environments. Yet, it is again (only) the problem of a possibly too high abstraction. At first, I shall point out again that the model by no means only assumes undifferentiated effects of group size. The relation between group size and the parameters for integration options rather represents a *variable* in the model and the text says respectively: “To be sure, structural effects of group size do neither necessarily occur nor have they to be particularly strong. This is the case, for example, when members of a larger group are distributed over spacious regions or when ethnic groups are otherwise ‘individualized.’ Functions 1a and 2a, therefore, describe those (extreme) cases, in which the options’ EU-weights are largely *independent* of group size.” [Esser 2010]. Only functions 1b and 2b describe stronger effects of group size, including a “cumulative” reinforcement of the ethnic-context-option at a certain threshold value and in dependence on certain special conditions, like the emergence of systems of ethnic relations. One can easily imagine empirical differentiations of the initial assumption of an equal distribution in terms of group size as a structural restriction: spatial segregations or institutional focal points, for example. And again it is a question of the model’s reliability for the explanation problem at hand, whether one has to make the model respectively more complex – what would be without doubt possible (via introducing further functions between functions 1a and 1b and 2a and 2b; see already footnote 2). Spatial segregations and focal points, yet, only represent *structural* parameters of the social environment, and Eve stresses that not aggregates made up the actually relevant environments, but personal relations and networks. One can readily agree to this point and one could include it in the model as further differentiations (just as in the case of segregations and focal points). The question then arises whether it would have been better to focus on the level of network relations instead of group size from the very beginning. Eve seems to presume that immigrants’ social relations and networks had *nothing* to do with group sizes (or segregations and focal points). This is yet quite astonishing. Net-

works represent systems of (bilateral) social relations like, for example, friendships. Nearly all general explanations of friendships, however, start from the assumption that any possible mating presupposes the *chance* for a *meeting*. The social structuring of these chances (by group sizes of ethnic membership, but also of gender, education, or religion) then determines the pattern of *possible* friendship relations. In terms of the subsequently possible mating process additional other aspects, like congruent values, dissonances with the wider environment, or already communication opportunities due to a shared language become relevant [see already the early contribution by Lazarsfeld and Merton 1954]. In short: Personal relations and networks do *not* make up fix entities that are independent of the wider social environment and structural restrictions, including group sizes. The critical point in Peter Blau's theory and which Eve rightly points to is his assumption that *solely* structural chances were important – and not actors' preferences and their subjective ideas as well. Yet, it is precisely this objection which one *cannot* make against the model of intergenerational integration, because it also includes possible "choices" within structural restrictions. It needs to be added here that the significance of group size goes far beyond the one for the formation of bilateral friendships and rather narrow personal networks: Ethnic economies, ethnic enclaves, ethnic organizations, and ethnic communities *can* hardly emerge when group sizes are small, and they can hardly be structurally developed or even institutionally completed. In addition, it is certainly important for the emergence, stabilization, change, or dissolution of personal relations and ethnic networks whether they are surrounded by other inner-ethnic relations or not, or whether "weak ties," bridge relations und "structural holes" exist, which are not independent of how many actors of a certain category are actually present.

The three comments reflect quite nicely the methodological positions and debates within social sciences in general. In addition to the many substantial details, they address particularly the usefulness of abstracting, comprehensive, and deductively operating models in view of the exceptions, particularities, and historical singularities in terms of social processes as well as the question of how one can systematically consider the interpretative-constructive dimension of human action and of social processes within the framework of an explanatory approach. The problem has become obvious: Abstract and systematizing models often deviate much too quickly from "real" conditions, and the mere collection and generalizations of limited correlations made from case to case often remain quite disordered and disconnected. Reactions with regard to the model of intergenerational integration differ very much: Debora Mantovani and Giuseppe Sciortino recognize the model's virtues and largely follow its logic. Yet, with their emphasis on the consideration of the interpretative dimension of action they point to a without doubt important detail, which they be-

lieve would further increase the model's potential. Their point of view is encouraging, but quite uncommon in the field of the sociology of migration, which usually keeps clear of the analytical perspective. While Michael Eve at least recognizes the model as an "interesting attempt" [Eve 2010], he leaves no doubt about the fact that not only central substantial assumptions were wrong, but rather that the whole attempt of a comprehensive modeling was unnecessary, possibly basically inappropriate, if not even ideologically biased. This has been the well-established common sense within greater parts of the sociology of migration for quite some time now, against which the model has not least and partly explicitly addressed.

Richard Alba suggests as a third possible way the distribution of the respective explananda to different theories of "middle range" and the solution of the specific explanatory problems in a way based on the division of labor between the various approaches [Alba 2010]. This seems intelligible as there is enough (empirical) work on details to be done and as without this work all abstract theoretical models would be quite up in the air. Yet, is this advice really a good one [see already the remarks on that point in Esser 2010]? Without doubt, it would be a bad one if it came to a continuation of the present segmentation of the various theories, over and above legitimated by the objection that only theories of *middle-range* were anyway possible and reasonable. What would this mean then? Should one continue just as hitherto to work on and with theories (of middle-range) without any orientation along a comprehensive framework? That is hardly imaginable: The important point here is not primarily the existence or survival of particular theories, their peaceful co-existence, and the undisturbed work within a certain familiar area, but the adequate explanation of the respective question and the comprehension of the correlations between the various mechanisms. And if different societal outcomes of intergenerational integration emerge and if the different processes, which result in the one or the other outcome, are interrelated and mutually depend on each other, one will actually have no other choice than trying to develop an integrative theory. To be sure, not everybody needs to attempt this. Yet, somebody should work on it as well, perhaps also using another and better model.

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How Far Reaches the “Middle Range” of a Theory? A Reply to the Comments

Abstract: In response to several special characteristics of the so-called “new immigration” and to the well-known weaknesses of classical assimilation theory, several theoretical suggestions have recently been made and discussed, including, in particular, the “Theory of Segmented Assimilation” and the “New Assimilation Theory.” In addition to the (classical) structural outcome of assimilation, these theories assume two other possible outcomes: ethnic stratification as the enduring social descent of following generations and selective acculturation as the social advancement by using and retaining ethnic resources and identities. This contribution reconstructs these theoretical developments and the presumed structural outcomes as special cases of a comprehensive model, i.e., the model of intergenerational integration, and systematizes sub-processes and single mechanisms outlined by the various theories. Another important result is the identification of conditions and background processes that do not necessarily occur empirically, but that underlie the different theories and structural outcomes as well as the proposed model of intergenerational integration.

Keywords: assimilation, ethnic stratification, selective acculturation, “new immigration”, intergenerational integration.

Hartmut Esser ist professor (em.) for Sociology and Philosophy of Science at the University of Mannheim. His main fields of interest are general methodology of the (explanative) social Sciences, the theory of action, migrations and integration, social inequality, and the sociology of family. Among his publications: *Sociology. General Foundations* and six volumes on *Sociology. Special Foundations*.