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Prediction, explanation and the epistemology of future studies

Paul Dragos Aligica *

Mercatus Center, George Mason University, Arlington and Hudson Institute, 5362 Emerson Way, Indianapolis, IN 46226, USA

Abstract

This article is a contribution to the development of the epistemological foundations of Futures Studies. The article starts by presenting the conventional "covering-law" model asserting the symmetry between prediction and explanation, a model that continues to undermine the authority of Futures Studies as a discipline despite the fact that Logical Positivism, the epistemological paradigm that inspired it, is no longer dominant. Then the article outlines the fatal weaknesses of that model showing how out of its criticism emerges the prospect of a coherent and robust epistemology of prediction. Two major points are made: First that predictive argumentation is not demonstrative but merely evidential. Therefore formal logic argumentative structures of the "covering law" type are inadequate in giving a complete and accurate account of predictive argumentation and practice. If the nature of predictive arguments is evidential then the epistemology of prediction should be based not on mere formal logic but on a larger theory of argumentation. Second, the criticism illuminates the complex problem of the types of knowledge and information used in predictive arguments to build up evidence. Explicit and formalized knowledge and statistical evidence are not enough for a successful predictive procedure. Background information and personal, local and tacit knowledge play a surprisingly major role in predictive arguments and procedures and that has very important epistemological consequences.

One of the most challenging difficulties Futures Studies had to face since its inception as a discipline has been the fact that in an era dominated by the legacy of Logical Positivism the Futures Studies project seemed epistemologically odd and not quite matching the rigid standards of scientific investigation imposed by the main-

* Tel.: +1-317-549-4157; fax: +1-317-545-9639.
E-mail address: paligica@yahoo.com (P.D. Aligica).

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stream Positivist cannon. In spite of its impressive advances in theory, methodology and applications, the shadow cast on it by the fact that it was epistemologically suspicious to the philosophic mainstream undermined a good deal of its credibility and authority as a discipline. Even in the wake of the retreat of Positivism as a dominant paradigm the situation in this respect remained frustratingly dysfunctional. Thus there is no surprise that many preeminent scholars in the field argued that an epistemology of Futures Studies was long overdue and that given the current intellectual circumstances, the effort of developing it came to represent one of the major priorities of the field at this point [1, 9,14,15, pp. xxiii, 191–238]. Futures Studies had to establish its epistemological credentials in a clear and robust way and thus to claim its clout and legitimacy undermined by Logical Positivism in front of the scholarly community.

Undoubtedly the main source of the damage done by Logical Positivism to the epistemological foundations of Futures Studies was neither the rigid methodology implied by it nor its ultra-empiricism but its widely accepted and influential theory of *explanation*. The crux of that theory is that explaining and predicting events are logically and methodologically identical. It is true that positivists were interested in developing a theory of *explanation* and not of *prediction* but due to the alleged logical symmetry between the two, a complete and analogous theory of prediction emerged in a natural way by implication from the theory of explanation. This model and the relationship between prediction and explanation implied by it have raised to dominance and become the backbone of epistemology and the theory of sciences for a couple of decades. The problem is that the account it has given to both explanation and prediction is incomplete and in many respects harmful to the explanatory and predictive practice. By tying the two too close together in a rigid conceptual framework it has arbitrarily constrained their domains and undermined the epistemological legitimacy of many of the methods, practices and approaches associated to them.

In the case of *explanation*, the model, while adequate for many important types of scientific explanations is not at all applicable to all scientific domains. It is definitely not a complete account of explanation and the consequences of the straightjacket it has imposed to scientific inquiry are appreciable. Imposing prediction as a fundamental concept and criteria for explanation the positivist epistemological model sets standards that many disciplines could never achieve by their very nature. As such they were arbitrary relegated outside the proper domain of science. The result was an unnecessary long and painful debate in all the disciplines affected by that demarcation criterion, a sterile debate that rages to this day in, for instance, political science or sociology.

But the impact of the model on *prediction* was even worse. The spread of the belief in the identity of predictive and explanatory scientific procedures undermined at a fundamental level the efforts to reflect on the nature and potentialities of predictive procedures different from those used for explanation. The legacy of this state of affairs continues to be felt very strongly in Futures Studies. Nevertheless it is interesting to stress that doesn’t happen due to the embrace of the positivist model by the discipline. Familiar with the complexities of future oriented thinking, Futures scholars never took the model seriously. But outside the sphere of its own theorists and practitioners, the Futures Studies field has been still perceived through the epistemological lenses shaped by the positivist model. The truth is that the legitimacy and status of Futures Studies rest with the position the field manages to validate for itself in the mainstream epistemological and scientific methodology forum. And the reality is that the epistemological asymmetry between explanation and prediction has not been adequately recognized and considered outside the field in epistemology or social theory, and that the Futures Studies scholars haven't made and drawn that distinction convincingly enough.

The discussion of the specific methodology of prediction—a theme that with very few exceptions has been neglected by the philosophers of science themselves—failed to enter the mainstream epistemological and philosophy of knowledge debates. And the crucial obstacle to that development continues to be the myth reigning in mainstream social sciences that explanation and prediction are or should be symmetrical processes. It is interesting to note that disentangling the models of predictions from those of explanation, and making the case for a solid epistemological argument remains today a priority for the futures research community as it was 30 years ago. In a path-breaking article written in 1964 Hellmer and Rescher wrote: "As long as one believes that explanation and prediction are strict methodological counterparts, it is reasonable to press further with solely the explanatory problems of a discipline, in the expectation that only the tools thus forged will then be usable for predictive purposes. But once this belief is rejected, the problem of a specifically predictive method arises, and it becomes pertinent to investigate the possibilities of predictive procedures autonomous of those used for explanation" [5, p. 32].

During the last decades Futures Studies made important progress in theory, methodology and applications. But it is still to make a convincing case to gain epistemological legitimacy outside its own field. The task is clear: translating into the mainstream's epistemological terms the insights gained by the discipline and placing them within the ongoing debates in philosophy of science and theory of knowledge. That effort and the epistemological battle for the future and status of the field are even more urgent today when the place of logical positivism is filled by a number of scattered approaches that may lead to a broader and more realistic view of explanation but that continue to neglect the issue of prediction. Thus in spite of the change of the climate of philosophical opinion, the prediction issue is in danger of remaining strongly tied in its entanglement with explanation, and to unwittingly carry on the legacy of the positivist model.

Therefore it is even more important today to disentangle the theory of prediction from the theory of explanation and thus to contribute to the elaboration of a strong case for an autonomous and specific epistemology for Futures Studies. This paper is a contribution to this effort of carving a firm epistemological ground for Futures Studies. As such it continues by presenting the classical model of the symmetry between prediction and explanation and then outlines its fatal weaknesses showing how out of its criticism emerges the possibility of a coherent, robust, original and very interesting epistemology of prediction. All these are done being aware of the fact that the epistemology of Futures Studies could not be reduced to a mere extension of a theory of prediction and that themes such as conditionals, counterfactuals and scenario-related analytic narratives that carry on their own epistemological load are as important as prediction is. However given he external perception of Futures Studies, a perception that is defined and shaped by the notion of prediction, the issue of prediction should be addressed with priority.

1. The "covering law", nomologico-deductive model of explanation and prediction

The classical statement of the thesis of the logical symmetry of scientific explanation and prediction was made by Hempel [7,8]. The basic point is that explaining and predicting events are logically and methodologically analogous processes. As in most positivist discussions of the issue, the focus of his argument was however on explanation. To elucidate what is happening in explanations, the (apparently) more basic concept of prediction is introduced as a means to specify and operationalize the definition of explanation: "An explanation (...) is not complete unless it might as well have functioned as a prediction; if the final event can be derived from the initial conditions and universal hypotheses stated in the explanation, then it might as well have been predicted, before it actually happened, on the basis of a knowledge of the initial conditions and general laws" [7, p. 38].

Nevertheless due to the alleged logical symmetry between the two (who were considered two faces of the same process) a complete and analogous theory of prediction emerges in a natural way by implication. The key notion is that the scientific explanation is "nomologico-deductive"—that is, it deduces the event's or phenomenon's realization from general laws and the specific conditions in place. The explanatory or predictive argument for a particular event, share the same inferential pattern:

(C) Information about the antecedent conditions obtaining prior to P's occurrence;
(L) General Causal laws;
(P) Claim of P's realization.

Thus an explanation of an event is a deductive argument whose premises include general laws. Hempel called that a "covering-law" or "nomologico-deductive" argument. An event is said to be explained if an argument of this sort whose conclusion is a statement describing that event or phenomenon is constructed. An example from astronomy illustrates Hempel's approach [3, pp. 349–50]. The phenomenon P to be explained is the planet Mars' retrograde motion in a specific day. To explain P is to show how it follows from the antecedent conditions obtained prior to P's occurrence via General Causal laws. More precisely, in this case the antecedent conditions are (C1), Mars' mean distance from the Sun and from the Earth; (C2), Mars' mean period of revolution; (C3), Mars' mean angular velocity; (C4), Mars' position on that day. The relevant general laws are the laws of celestial mechanics: Kepler's three laws (L1); Galileo's two laws (L2); and the laws of Huygens and Newton (L3). Thus P is explained by showing how it follows from (C1, C2, C3, C4) via (L1, L2, L3, L4). In a similar way P could have been predicted before that specific day simply from the knowledge (C1, C2, C3, C4) by extrapolation via (L1, L2, L3, L4) [3, p.349]. In other words, in the Hempel interpretation explaining P is nothing more than predicting P after P has occurred while predicting P is just explaining it before it has occurred. The justification of a prediction of P is symmetrical with the explanation of P. The ultimate test of an explanation of a phenomenon is to be able to, simply by imagining a temporal shift, to predict P by the same explanatory techniques [3, p. 349; 13, pp. 165–68].

The implication of that is that the sole difference between prediction and explanation is mere temporal. To distinguish between a prediction and an explanation is to determine whether the argument precedes or succeeds the target event or phenomenon. If the argument precedes P, then we have a prediction; if it succeeds P, we have an explanation. Other than that the argumentation and the logical structure is identical. Explaining P is simply "predicting" P after it has already happened, and predicting P is simply "explaining" P before it has already happened. Explanation and prediction are symmetrical processes [13, p. 165].

As it has already been mentioned, this model had an important influence in epistemology in the next couple of decades. But as the account it has given to both explanation and prediction has been incomplete, its influence on the explanatory and predictive practice was largely negative. By tying the two too closely together, and by grounding them in a restrictive logical model it arbitrarily constrained their domains and undermined the epistemological legitimacy of many of their methods and approaches while generating endless sterile debates and controversies. But that is only one aspect of the problem. The most basic problem of the Hempel model is that it is simply not true to the facts.

2. The looseness of fit between predictive performance and the truth of explanatory theories

One of the most powerful and basic challenges to the explanation–prediction symmetry thesis expounded by Hempel was the existence of explanations that lack "predictive power". Examples abound: explanations of the occurrence of geological phenomena like earthquakes and evolutionary explanations of the emergence of a new biological species or the explanations of radical processes of social change, conflict and cooperation, are among the most obvious. It is impossible, in most cases, to predict these phenomena with any significant reliability; but still they could be explained in a meaningful way. Only by semantic abuse these explanations could be considered "pseudo-explanations" [10, p. 365]. The fact is that the ideal situation which the Hempel model described had little backing in history. In reality that situation was the exception in the history of scientific inquiry and the epistemological condition evoked by the geological, evolutionary or historical examples was the norm.

A closer look at the history of scientific inquiry reveals that in this respect there are two traditions or approaches in history of science: one is putting the emphasis on explanation; the other on prediction. Rarely the two have converged. Toulmin [15], Rescher [3], Hanson [3] all tried at one point or another to break the spell of the myth of the symmetry between explanation and prediction by simply over-viewing the history of science and especially of astronomy and showing that those histories present very few examples of disciplines or cases wherein this equilibrium between explanation and prediction has actually been achieved.

Paradigmatic for all these authors were the cases of Ionian and Babylonian approaches to astronomy. Both Ionians and in Babylonians started to develop by the same time specific ways of thinking about the heavens. Nevertheless their approaches were quite different. By the time of the Greek Golden Age (Fifth century B.C.) Babylonians had developed arithmetical technique for forecasting the moon cycles and lunar eclipses that were unmatched by the Greeks. However, that achievement had no serious explanatory or even a persuasive speculative basis. Their approach was purely arithmetical. No conceptual system was behind their outstanding predictive success. They simply compiled and analyzed a data basis containing each of the celestial motions as a set of independent variables, each changing in a regular, predictable manner. Thus they could calculate the variables separately, and re-combine them so as to predict various celestial configurations or events. As much as it is known they had no theory to explain their predictive successes or failures.

On the other hand, the astronomy of the early Ionians consisted almost completely of a mixture of speculation and theory. Prediction calculus was totally marginal if of any interest at all. Instead analogies, metaphors and bold speculations gave substance to their approach. As such they offered no basis for predictive power. Nevertheless the circular tubes full of fire, the cosmology of love/attraction and hate/repulsion as basic forces of the universe, the wholes in the sky having behind eternal fires etc. offered challenging and vivid explanations of the celestial and cosmic phenomena. The Ionians strived for explanations and understanding. The Babylonians had great forecasting power but they clearly lacked understanding. That was demonstrated by their futile endeavors to apply arithmetical analysis to other phenomena such as earthquakes, plagues, and political calamities. Their inability to articulate conceptual differences between these different types of phenomena revealed a lack of explanatory imagination. The lesson of the Greek and Ionian cases is that to discover what kind of events may be predictable and to develop acceptable theories about them is different from developing effective techniques for their forecast.

That crucial dichotomy was transmitted in astronomy to Aristotle and Ptolemy. On the one hand was Aristotle, whose world-views and explanations offered a certain degree of intelligibility while they lacked almost any predictive power. On the other hand, the great Greek astronomers continuing the Babylonian tradition—Eudoxus, Apollonius, Hipparchus, and indeed Ptolemy—could predict the motions of planets and stars but neglected or rejected the possibility of ever explaining the principles behind the motions of the cosmos. Further in time, Keplers' arithmetic approach on the one hand, and Newton's system of integrated explanation, on the other hand, continued the tradition. But due to Newton, something new happened: a synthesis of the two traditions that was fully functional on both dimensions, explanation and prediction. This synthesis has since been crucial for the way the issue of explanation and predictions has been seen. Due to the Newtonian synthesis, nowadays we expect a scientific inquiry to combine merits of both kinds. Thus today our view of scientific investigation owes a great deal both to the Babylonians and to the Ionians. One tradition developed the forecasting techniques, the other developed the use of the speculative and theoretical imagination. As authors like Toulmin and Rescher noted "only if you are committed beforehand to the view that science has a single, all-embracing purpose are you obliged to praise the one and decry the other" [13, pp. 165–69; 15, pp. 18–30].

Indeed, the history of scientific theory could be written as the uneasily coexistence and continuous struggle between these two opposed forces, the urge to explain and the urge to predict. Only rarely an optimal situation of equilibrium was achieved. But once it happened in the Newtonian system its impact on the way people understood afterwards science, explanation and predicting was overwhelming. That is the context of the Hempel's thesis should be seen. For Hempel, explaining P was simply "predicting" P after it had already happened. The question is: is this a realistic account of the scientific practice and the history of scientific inquiries? What are the examples illustrating Hempel's thesis? As Hanson put it, the answer is always the same: in Newtonian mechanics [3, pp. 351–55].

Hanson's influential criticism of the symmetry between explanation and prediction pivots exactly on this issue. In a milestone article published in 1962 Hanson noted that the history of science presents "very few examples of disciplines wherein this optimum state of affairs has actually been achieved" [3, p. 350]. He attacked the symmetry thesis by simply showing that the Hempel model is in fact an unwarranted generalization: that (a) the sole key example used is Newtonian mechanics and other possible cases were not used for generalization purposes and that (b) even in physics which was the original model, the things were not as straightforward as the Hempel thesis implied. As quantum theory was fundamentally nondeterministic, noted Hanson, it would be interesting to speculate on what Hempel's analysis of explanation and prediction might have been like had he used as an example and inspiration quantum theory instead of classical mechanics: "There may therefore be rather more yet to be said about the logic of explanation and prediction, as these activities actually obtain in fields other than that one in which Hempel's analysis appears to be sound. Because philosophers might still wish to know what is the logical structure of the concepts of explanation and prediction as they are used in living, growing sciences, and not only as they were employed for a brief period in a discipline which is by now little more than a computing device for rocket and missile engineers" [3, p. 358].

To sum up: the Hempel model is a generalization based on only one or a very limited number of examples of scientific, theoretic and predictive activities. A broader sample of such activities would have revealed a more complex relationship between prediction and explanation. Ignoring that, the model imposed an ideal unattainable in many domains of scientific inquiry. Both explanation and predictive practice were affected by this unwarranted generalization. Entire fields coming under its powerful spell felt compelled to look for laws, regularities that did not exist or if they did exist they did not have the predictive power (and sometimes not even the explanatory power) of Newtonian mechanics. Hence a relentless source of frustration.

But if explanation was put on a procrustean bed, the problem of prediction was even worse. As it has already been mentioned, the end result was to simply import to predictive issues the epistemology centered on explanation and the entire limiting conceptual apparatus associated with it.

But there is even more in this overview of what the history of science tells us about the alleged symmetry between explanation and prediction than the fact that it was historically inaccurate and as such its influence was unconstructive if not harmful. Besides revealing the unwarranted generalization laying at the foundation of the Hempel thesis it also reveals the actual and real looseness of fit between predictive performance and the truth of theories; what Rescher calls “predictive slack” [13, p. 169]. Rescher notes the important aspect that even the most theoretically and empirically based predictions made using scientific methods and theories are not immune to failure. There is no assurance of success. In all predictive applications of science, good theories sometimes yield poor predictions. On the other hand, a successful prediction is not a confirmation of the scientific nature or correctness of the theory that inspired it. A theory is not necessarily true when it is “confirmed” through the success of its predictions or applications. There is no certainty that the success is due to the theory as opposed to an otherwise unspecifiable aspect of the overall situation. The sole warranted conclusion is that there was a fit between theory and the phenomena. But what is the epistemological nature of the fit is not apparent. Successful predictions and applications are not a guarantee of the truth or correctness of the explanation or theory behind it [13, p. 169–70].

The myth propagated by Hempel and his epistemological followers that success at prediction and application is a feature of scientific explanatory success is contradicted by countless examples of erroneous theories leading to successful predictions. Thus prediction-guiding theories cannot automatically be blamed for the predictive failure and prediction-guiding theories cannot automatically be credited with the predictive successes. Ptolemaic astronomy and Galenic medicine underwrote many successful applications. But that does not establish their claims to truth. Newtonian gravitation itself had impressive credentials, until it came to be replaced by Einsteinean relativity and if history of science teaches something it is that too many theories and explanations claiming predictive success were later abandoned as faulty [3, pp. 352–58; 13, p. 171; 15, pp. 18–30].

In a similar way, the predictive and applicative success “does not show the actual truth of our theoretical picture of nature but only its local empirical indistinguishability from the truth (whatever that may turn out to be) at the present level of observational sophistication” [13, p. 171]. A successful prediction shows at it’s best that “the inadequacies of our theories—whatever they may ultimately prove to be—are not manifesting themselves within the (inevitably limited) range of phenomena that currently lie within our observational horizons” [13, p. 171]. Thus it is legitimate to conclude with Rescher that the thesis that the truth of a scientific theory is the best explanation of its success in matters of prediction is simply wrong. The relationship between the correctness of an explanation and the success of a prediction is not logical, organic or necessary. To describe, analyze and set their standards under a common logical model is misleading. The mismatch between theoretical truth and predictive success lays bare the fact that the domains of explanation and prediction although related are different.

Once the separation of the explanation and prediction domains is established there is an important additional lesson to be drawn. If predictive success has any scientific bearing that is not of a demonstrative nature but merely of an evidential nature. Predictions are always evidential and not demonstrative. In the prediction's domain even the best confirmed theories are no more than reasonable and provisional estimates of the truth. In that domain the logical and formal structure of the arguments is of an evidential nature. The arguments aim not at demonstration but at marshaling evidence in favor of a predictive thesis. That leads to a very powerful conclusion: If the nature of predictive arguments is evidential then the epistemology or the foundational theory of prediction should reflect that evidential aspect too and as such should be based not on mere formal logic but on a larger theory of argumentation.

3. Predictions and "background information"

The issue of the evidential nature of predictive argumentation is crucial as it opens up the way to the complex problem of the types of knowledge and information used to build up evidence by this kind of argument. In this respect non-formal and tacit knowledge plays a surprisingly major part and that has very serious epistemological consequences. As formal logic argumentative structures are not enough to give a comprehensive and fair account of predictive argumentation, so statistical evidence and explicit and formalized knowledge are not enough for a predictive exercise. The next section of the article will return to the issue of the argumentative structure of prediction. This section will focus on the type of information or knowledge those argumentative structures are supposed to marshal.

One of the most effective ways of illustrating the non-formal dimension of predictions is to focus on the probabilistic dimension intrinsic to them [5, pp. 30–35]. Due to the fact that prediction deals with uncertainty, a factor that is intrinsic to future, probability is called to play a pivotal role in the operation of predictive arguments. The rational assignment of probability values is vital in the predictive practice. Although the common wisdom is that the assignment of probabilities in a rational way is a function of statistical evidence, the truth is that in many cases predictions do not function that way.

Let's take an example of statistical and probabilistic evidence that occurs in direct reference to prior instances. Assuming an event x: the morning commuting train to downtown coming in time. Its record of successful coming in time is around 50 percent. Will event x take place this morning? A rational person will assign it a probability number close to 1/2. In reaching that conclusion the person will use as a basis for probability assignments the record of past instances. That way of determining the probability of future events is a regular, and by and large justified, inductive procedure.

Nonetheless, there are many circumstances in which this kind of evidence is misleading. Consider the case of a person who has been riding that train to work for a full calendar year. He has thus a large sample of rides at a constant fare of 1$. But one morning he is required to pay 2$. In his sample of rides he has now a 1/365 chances to have 2$ ride next time. Will he wonder if his return fare that evening will be 1$, will he be assigning probabilities to getting a 1$ fare next day and so on? Despite the preponderance for the next coming year of 1$ rides in his sample, that is rather unlikely. The reason is that he knows that fares may change and he knows that once a new fare structure is in place, it will take some time until it will be changed again. Also he might know something about the municipal budget or the state of the finances and the plans of the transportation company etc. Given that background information, it is clear why he will not base his personal probability directly on the cumulative record of past instances.

These examples illustrate the need for using background knowledge as indirect evidence as a means to bolster, manage and reconfirm direct instance confirmation. This need is not accidental or marginal. Instead it is constantly present in the use of evidence. The example above clearly illustrates the point that “knowledge about past instances or about statistical samples is not the sole and sometimes not even the main form of evidence in support of rational assignments of probability values” [5, pp. 30–40]. Indeed past cases or the statistical synthesis of those cases may provide relevant and pertinent knowledge. However the evidential use of such information must always be tempered by reference to background information. The statistics are important and sometimes necessary but most of the time not sufficient. On the other hand, for a successful predictive effort the background knowledge is almost in most cases necessary and in the majority of cases sufficient.

Thus past experience, even if synthesized in large N statistics is always only a prima facie evidential element. Its predictive usage is always structured, filtered and interpreted in the light of background information, information that needs not and generally is not of any theoretical or statistical nature. Even more, the background information is frequently intuitive in character and has the form of a vague recognition of underlying regularities. In terms of substance, local knowledge [11,12] or tacit knowledge [4] becomes decisive for successful predictions. In terms of structuring principles, analogies, metaphors, similes, images and correlations may have a pivotal role in this case. It is important to stress that in this respect there is no difference in principle between social, managerial and physical sciences. The use of background information, to temper the application of statistical information, is as important in engineering as it is in public affairs. This is even truer in administrative sciences and management. Successful predictions in those domains are less dependent on statistical information than on knowledge of behavioral regularities, of the intentions and preferences of the relevant people, of the institutional arrangements, group aspirations, traditions, customs, practices, fashions, of national attitudes and climates of opinion, organizational rules and regulations, and so on.

For instance in predicting of a Social Democratic victory in Germany in 2002 or 2006, a knowledge of the statistical record of past elections is of almost no relevance in comparison to a solid insight into existing trends and tendencies. Similarly, in predicting the success of the European Union constitutional negotiations the relevant statistical evidence is of no import, yet there exists a substantial, unstructured and in many cases unstructurable but highly relevant background information. In other words, there is always the structured, explicit prima facie information. But in itself it is not enough. The question is how relevant it is for the predictive case in question. Thus the background information comes to play a decisive role. Background information is the ultimate criterion in giving evidential weight to various pieces of prima facie information in conditions of uncertainty. Consequently the quality of the predictions of different people faced with the same historical or statistical information will depend primary on their background information.

Hence the great importance which must be attached to personal knowledge. The human repositories of background information become crucial. “They have a large store of (mostly unarticulated) background knowledge and a refined sensitivity to its relevance, through the intuitive application of which they are often able to produce trustworthy personal probabilities regarding hypotheses in their area of expertness” [5, p. 45; 6]. The goal is to channel, draw and articulate the relevant knowledge from these personal repositories. Be they certified experts or simple social actors that happen to be in social contexts or configurations that makes out of them depositary of relevant background knowledge for specific predictive issue, the key problem of predictive effort is how to capture their “expertness”, their knowledge. Thus the epistemological problem of prediction becomes more complex than the standard approach implies when it describes it as a mere problem of model building and assembling statistics. The problem of using background knowledge as indirect evidence as a means to bolster, manage and reconfirm explicit and formal hypotheses and extrapolations is revealed. But even more than that, prediction requires managing knowledge in a social process and as a social process. The existence of this crucial social dimension requires a specific epistemology dealing not only with logical structures but also to the ways background knowledge is captured and marshaled in evidentiary arguments: a “social epistemology” dealing not only with databases, statistics and models but also with the social context and social processes of knowledge creation and analysis specific to a future oriented inquiry [2].

4. Beyond the “covering-law” model

The previous section of the article proved that statistical evidence and explicit and formalized knowledge are not sufficient for a successful predictive operation. The type of information or knowledge the predictive argumentative structures are supposed to marshal is heterogeneous and surprisingly complex. That has not only very serious epistemological consequences but also serious methodological implications. The significance and complexity of those epistemological consequences and methodological implications is amplified by the already mentioned fact that the formal logic argumentative structures are not adequate to give a comprehensive and fair account of predictive argumentation. This section of the article returns to the issue of the argumentative structure of prediction and explores it in depth, making the case against the relevance of the covering law types of models and for the development of an autonomous epistemology of prediction.

Consider the following covering-law argument:

(L) The response of social actors to continued relative deprivation is social revolt.
(C) Relative deprivation prevailed by the end of the 1980s in Eastern Europe.
(P) Eastern Europeans revolted by the end of the 1980s in Eastern Europe.

This argument could equally be an explanation, a prediction or a retrodiction. It could be an explanation of the fact that Eastern Europeans revolted by the end of the 1980s. But it could equally be construed as a retrodiction establishing this fact from the previously known general law (L) and a previously established fact concerning the condition of social actors in Eastern Europe in the 1980s (C). Moreover, if expressed in a tense-neutral language, or if the past tense in (P) is replaced by future, the covering law argument could also be construed as a prediction of the Eastern Europeans revolts by the end of the 1980s in Eastern Europe on the basis of (L) and (C). The implication of this exercise is that in order to discuss predictions or explanations it is necessary to go beyond the mere logical structure of the arguments. What is called an explanatory, a predictive, or a retrodictive argument "is not an argument considered in an abstract way, but rather a concrete argument that is given at a certain time, in a specific context, and for a specific purpose" [10, p. 360].

Thus to tell between explanatory, predictive, and retrodictive arguments, it is necessary to explore those dimensions of an argument that are not formal or logical. The rhetorical context and the non-formal relationships between the premises and the conclusion become crucial in understanding the nature of explanatory, predictive, and retrodictive arguments. Given the fact that a covering-law argument structure can be used equally well for an explanation, a prediction or a retrodiction, any theory of explanation and/or prediction should build less on the common logical covering-law element and more on the specific differences given by the specific non-formal dimensions. The implication is that the Hempel approach, limited as it is to the common covering law element, cannot differentiate between these different explanatory, predictive or retrodictive procedures and their consequences at the applied level.

This crucial feature of predictive arguments has momentous epistemological implications. The function of an explanation is to connect information in a coherent, meaningful and presumably true way. To explain is not merely to give additional bits of information. The new information is important but it is secondary in the sense that in itself it is not the goal of an explanation. In order to be relevant for that explanation, that information must contribute to the comprehension of what it is to be explained. Having all the relevant information is a necessary but not sufficient condition for an explanation. Only when this information is connected in a coherent, meaningful and truthful way may we speak of an explanation.

On the other hand, predictions and retrodictions "are projections from the known data to the unknown"; they are endeavors to gain or increase the knowledge of particular events, phenomena and states. The function of the predictive or retrodictive argument in these circumstances is to substantiate, or give support to, such a projection [10, pp. 362–64]. The argument of the covering-law type is one of the many types of arguments that might be employed. When it is used to predict/retrodict, the premises (i.e., the laws and statements of initial conditions) have the role to justify the claims regarding future or past events and states. It functions as evidence for the conclusion that expresses a prediction or retrodiction. But when an explanation is employing a covering-law argument, the relation between the premises and the conclusion (i.e., the explanandum) is not an evidential relation. When a covering-law argument is used as an explanation, “the conclusion of the argument is evidentially or epistemically independent of the premises, whereas in a predictive or retrodictive argument of the covering-law variety, the conclusion is necessarily dependent on the premises for evidential support” [10, p. 364].

Explanations try to reveal connections between events, phenomena and states and if possible to reveal the fact that they are part of larger patterns, regularities and laws. The primary function of predictions and retrodictions, is to acquire and offer more knowledge of specific, concrete events and occurrences. The idea is to export from premises the necessary epistemic weight needed to gain credibility. The primary function of such arguments is simply to establish or prove the conclusion. Consequently in a prediction or retrodiction argument, the application of general laws is not essential. An argument that makes appeal to general laws is always welcome but still it is as good as any other argument; and thus in the last instance it is inessential. Using the covering-law model to make a prediction or retrodiction is sufficient, but not necessary. Statements of restricted regularities, quasi-laws, statistical laws, the so-called common sense generalizations or accidental generalizations can viably be employed in projective arguments.

As Kim put it, stating in precise terms a set of formal criteria for explanation and prediction may not help us much in understanding the nature and functions of explanation and prediction. The really significant thing is “to gain an understanding of these scientific activities and procedures rather than merely to be able to state such criteria” [10, p. 361]. Moreover Kim goes even further by stressing that a clear grasp of the formal criteria “constitutes neither a necessary nor a sufficient condition for an adequate understanding of the concepts under consideration. Given such criteria, we would still want to know why they are required at all, what their point is, to begin with. In order to know this, we have to know and understand the point of giving explanations and making predictions” [10, p. 361]. Understanding the practices, procedures and arguments in their social context and understanding the goals and objectives of predictive arguments while connecting the procedures to their social circumstances is thus an indispensable condition for any theory of prediction. Again the issue of “social epistemology” emerges as vital in articulating the epistemological foundations of prediction.

5. Conclusions

The investigation of the relationship between prediction and explanation and its epistemological implications revealed that the extensions of these two concepts only partially overlap and that the shared area is considerably larger than the region defined by the covering law structure. Probabilistic laws, quasi-laws and geneticcausal models of explanation are located in the same overlapping area without partaking the formal features and the specific mix of predictive and explanatory power of covering laws. The domain outside the overlapping area is also vast. On the explanation side there are explanatory structures with no or limited predictive power (such as the historical or evolutionary explanations) and on the predictive side there are correlational, time series, analogical and other predictive structures with limited or no explanatory power. Some predictions could be structured on the covering law or explanatory models. Most could not. A proper theory and epistemology of prediction should deal with the entire range of predictive approaches and models. To try to force all the others in one mold is unrealistic and unfeasible.

Despite the structural diversity and cognitive heterogeneity of predictions there are nevertheless some common and unifying epistemological themes emerging. Two of them are of crucial importance. The fist is that any attempt to articulate an epistemology of prediction should be based at a formal level on a theory of evidentiary argumentation or even better on a general theory of argumentation that incorporates both formal and non-formal argumentative models. The second is that the epistemology of prediction should explicitly deal with the intrinsic social nature of knowledge and knowledge production. This aspect is especially critical in this specific area of future-oriented intelligence where background knowledge with its informal, tacit, personal and social dimensions is so important. Both themes make the anchoring of the theory and epistemology of prediction (and for that matter of the Futures Studies in general) into the emerging field of Social Epistemology not an appealing alternative but a vital necessity [2].

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