"Comportamento razionale ed equilibrio nei giochi e nelle situazioni sociali" Annotated extended abstract and comments

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Abstract

In this note I provide an extended abstract of my undergraduate thesis [2, 1987] and I relate it to the literature, in particular, to later work of mine and of other scholars. The thesis has a strong methodological flavor, but it also contains some analytical contributions to game theory, including (i) the first game-theoretic analysis of the *conjectural equilibrium* concept, which is strictly related to self-confirming equilibrium (Fudenberg and Levine, [26, 1993]), (ii) a discussion of a kind of *rationalizability in extensive form games* (developed independently of the earlier work of Pearce, [43, 1984]) which emphasizes conceptual problems related to counterfactuals, and (iii) an *ante litteram* discussion of the so called *"structural inconsistency"* of sequential equilibrium assessments (cf. Kreps and Ramey, [38, 1987]).

1 Introduction

On July 7, 1987 at Bocconi University I defended the thesis titled *"Comportamento razionale ed equilibrio nei giochi e nelle situazioni sociali"* (Rational Behavior and Equilibrium in Games and Social Situations) to obtain the five-year degree in "Discipline Economiche e Sociali" (Economic and Social Sciences). In this note, I provide an extended abstract of the thesis and offer some comments to relate it to later work of mine and of other authors.

The thesis is written in Italian as a treatise, according to the rules and customs prevailing at the time.[1] The Introduction (not numbered) is followed by two methodological chapters (chapter 1 and 2), two chapters with substantive contributions (chapters 3 and 4), and concluding remarks (chapter 5). Parts of the thesis appeared, with additions and modifications, in Battigalli [3, 1998a], [4, 1988b] and Battigalli & Guaitoli [14, 1988], [15, 1997].

It is worth noting in advance that, when I wrote the thesis, I was unaware of some important articles that would have greatly improved my understanding of strategic reasoning and would have allowed me to improve my work. In particular, I would have greatly benefitted from reading Aumann's [1, 1974] classic article on correlated equilibrium, and Bernheim [21, 1984] and Pearce [43, 1984] seminal papers on rationalizability. I describe below the contents of thesis using "the benefit of hindsight", i.e. taking advantage of a more precise language and understanding developed afterword, or contained in work I was not aware of in 1987. I also relate the contents of the thesis to later work of mine and of other authors.

2 Two methodological chapters

Chapters 1 and 2 of the thesis (which follow the Introduction) are methodological. My starting point is that, as argued by von Neumann & Morgenstern [47, 1944], game theory should provide the formal language of the social sciences, and of economics in particular. I go on to argue that game theory is necessary to go beyond the "parametric analysis" that explains behavior as a function of parameters, like prices, that each individual takes as given, and yet are the main variables that economists try to explain by means of compatibility conditions such as “demand=supply”. Such an approach prevents the analysis of genuine disequilibrium behavior, which instead can at least be *expressed* in the language of game theory. In particular, the parametric approach typical of models with Walrasian market clearing is inadequate to

[1] The thesis has been scanned and it is downloadable from my webpage (www.igier.unibocconi.it/battigalli). It is not my doctoral thesis, which was published (again, in Italian) six years later (see [6]). The 5-year degree in Economic and Social Sciences of the ’80s roughly corresponds to the sequence of 3-year Bachelor plus 2-year Master degree in Economics and Social Sciences currently available at Bocconi Univeristy. My thesis is therefore comparable to a master thesis, although the degree I have been awarded was simply called "Laurea" (Bachelor), not "Laurea Magistrale" (Master).

analyze disequilibrium behavior, at most it can describe sequences of temporary equilibria based on incorrect beliefs about future prices (a similar observation applies to some so-called "disequilibrium models", which are actually models of non-market-clearing equilibrium).

Since this is a methodologically important point, let me be more explicit and detailed, and let me take the liberty of quoting a part of the introduction of my lecture notes on game theory [13, 2010], which is indeed based on my thesis:

> *(E)very "complete" formal model of an economic (or social) situation must be a game;* economic theory has analyzed perfect competition by taking shortcuts that have been very fruitful, but must be seen as such, just shortcuts. (...)
>
> If we subscribe to methodological individualism, as mainstream economists claim to do, every social or economic observable phenomena we are interested in analyzing should be reduced to the actions of the individuals who form the social or economic system. For example, if we want to study prices and allocations, then we should specify which actions the individuals in the system can choose and how prices and allocations depend on such actions: if p is the vector of prices, y is the allocation and $a = (a_i)_{i \in N}$ is the profile of actions, one for each agent, then we should specify relations $p = f(a)$ and $y = g(a)$. This is done in all models of auctions. For example, in a sealed-bid, first-price, single-object auction, $a = (a_1, ..., a_n)$ is the profile (...) of bids by the n bidders for the object on sale, $f(a_1, ..., a_n) = \max\{a_1, ..., a_n\}$ (the object is sold for a price equal to the highest bid) and $g(a)$ is such that the object is allocated to the highest bidder,[2] who has to pay his bid. To be more general, we have to allow the variables of interest to depend also on some exogenous shocks x, as in the functional forms $p = f(a, x)$, $y = g(a, x)$. Furthermore, we should account for dynamics when choices and shocks take place over time, as in $y^t = g^t(a^1, x^1, ..., a^t, x^t)$. Of course, all the constraints on agents choices (such as those determined by technology) should also be explicitly specified. Finally, if we are to explain choices according to some rationality criterion, we should include in the model the

[2] Ties can be broken at random.

preferences of each individual i over the possible outcomes. This is what I call a "complete model" of the interactive situation.[3] I call variables, such as y, that depend on actions (and exogenous shocks) "endogenous". (Actions themselves are "endogenous" in a trivial sense.) The rationale for this terminology is that we try to analyze/explain actions and variables that depend on actions.

At this point you may think that this is just a trite repetition of some abstract methodology of economic modelling. Well, think twice! The most standard concept in the economist's toolkit, Walrasian equilibrium, is not based on a complete model and is able to explain prices and allocations only by taking a two-steps shortcut: (1) The modeler "pretends" that prices are observed and taken as parametrically given by *all* agents (including all firms) before they act, hence before they can affect such prices; this is a kind of logical short-circuit, but it allows to determine demand and supply functions $D(p)$, $S(p)$. Next, (2) market-clearing conditions $D(p) = S(p)$ determine equilibrium prices. Well, this can only be seen as a (clever) reduced-form approach. Absent an explicit model of price formation (such as an auction model), the theorist postulates that somehow the choices-prices-choices feedback process has reached a rest point and he describes this point as a market-clearing equilibrium. In many applications of economic theory to the study of competitive markets, this is a very reasonable and useful shortcut, but it remains just a shortcut, forced by the lack of what I call a complete model of the interactive situation.

So, what do we get when instead we do have a complete model? As I am going to show (...), we get what game theorists call a "game". This is why game theory should be a basic tool in economic modelling, even if one wants to analyze perfectly competitive situations.

Chapter 2 goes on to provide a taxonomy of "parametric", "strategic"

[3] The model is still in some sense incomplete: I have not even addressed the issue of what the individuals know about the situation and about each other. But the elements sketched in the main text are sufficient for the discussion. Let me stress that what I call here "complete model" does not include the modeler's hypotheses on how the agents choose, which are key to provide explanations or predictions of economic/social phenomena.

and hybrid models, with an eye to general equilibrium and so called "disequilibrium" theory (more precisely, non-market-clearing equilibrium theory). Part of this material has been published (in Italian) in [3, 1988].

3 Chapter 3: conjectural equilibrium

Genuine disequilibrium behavior can be meaningfully expressed only within game-theoretic models, but I find existing solution concepts, such as Nash equilibrium, unsatisfactory, as they do not seem to capture the notion of stability with respect to learning proposed, for example, by Frank Hahn [29, 1973] and foreshadowed by Hayek [33, 1937],[4] nor do they represent the result of introspective strategic reasoning.

In Chapter 3, I propose an extensive-form game-theoretic notion of "conjectural equilibrium" inspired by Hahn, who writes that

> "an economy is in equilibrium when it generates messages which do not cause agents to change the theories which they hold or the policies which they pursue." (See [29, 1973, p 38].)

I argue that the "messages generated by the economy" cannot be anything but pieces of information induced by the (some exogenous shock and) the actions of the agents that populate the economy, which in turn are dictated by their strategies ("policies"). Therefore a "complete model" with the ensuing game theoretic analysis is required to provide a rigorous definition of this equilibrium concept.[5] Informally, a **conjectural equilibrium** (CE) is a situation where players best respond to their conjectures, and these conjectures are confirmed by the evidence obtained by each player as a result of the strategies played. In order to define CE it is necessary to specify what players can learn ex post, once the game is over, this is the **terminal infor-**

[4]In *Economics and knowledge* Hayek writes: "We may therefore very well have a position of equilibrium only because some people have no chance of learning about facts which, if they knew them, would induce them to alter their plans. Or, in other words, it is only relative to the knowledge which a person is bound to acquire in the course of the attempt to carry out his original plan that an equilibrium is likely to be reached."

[5]On the other hand, Hahn's [30, 1977] and [31, 1988] articles are not framed in the language of game theory. This prevents Hahn from providing a faithful formalization of the informal notion of equilibrium he put forward in [29, 1973].

mation structure.[6] When each player can observe *ex post* the whole path of play, there is **perfect terminal information**.[7] This is just an interesting possibility, *not an axiom* of my work. *In a CE, for each player* j*, the subjective probability distribution over* j*'s terminal information sets induced by* j*'s strategy and conjecture is the same as the objective distribution induced by (the probabilities of chance moves and) all players' strategies.* This is very explicitly and quite carefully motivated as a necessary condition to have a steady state when a typical game is played recurrently, although no formal analysis of learning dynamics is offered, as in [29, 1973, p 38].[8]

I assume that players have at least some *minimal knowledge of the game*: each player knows the game tree (or arborescence), his own information partition, and his own payoff function. But I allow for lack of common knowledge of the rules of the game (including the probabilities of states of nature, the order of moves and opponents' information partitions), and of opponents' payoff functions/preferences.[9] Technically, I model **conjectures** as subjective assignments of probabilities to initial nodes (chosen by nature) and of transition probabilities to other players' arcs. Knowledge of the opponents' actions labeling and information structure is assumed (in Chapter 4) to imply that conjectures are given by behavioral strategy profiles of the opponents, as the probabilities of opponents' actions have to be measurable with respect to their information structure. I will come back to this point. To sum up, I use the term "conjecture" to signify a kind of *reduced-form subjective model* of how nature and opponents play. Each conjecture of a player induces a map from his set of strategies to the set of probability measures over terminal nodes. Conjectures are assumed to be derived from "deeper" subjective

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[6] In the thesis, I derive such structure from the elements of the extensive form game (information structure and payoff functions), assuming that players have perfect recall and observe their realized payoff, e.g., because it is money, or consumption. But I allow for dummy information sets where players are inactive. Thus, my approach is equivalent to assuming that the terminal information structure is a primitive element of the model, but payoffs have to be measurable with respect to terminal information. In my later work on this topic, I dropped such measurability assumption, which may be inappropriate in some applications. Obserbability of own payoffs plays an important role in [19, 2011].

[7] This is called **perfect feedback** in [19, 2011].

[8] On learning in games see, for example, the early survey by Battigalli *et al* [18, 1992] (not surprisingly focused on the conjectural equilibrium concept), the book by Fudenberg and Levine [28, 1998], and Nachbar's survey [41, 2008].

[9] In my later work I assume full knowledge of the extensive form and I model incomplete information in a more familiar way.

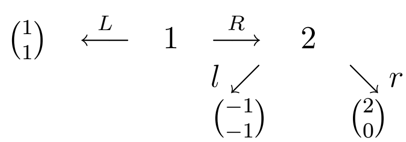
theories about nature and opponents' behavior. Such derivation is sketched in Chapter 4.

After the main definition of conjectural equilibrium, I analyze some examples. In the simultaneous move game depicted below (Fig. 2, p. 111), if player 1 (player 2) cannot distinguish ex post (L, l) from (L, r) ((L, r) from (R, r)), the non-Nash strategy profile (L, r) can be supported as a conjectural equilibrium: each player chooses the "safe" action believing that the other one would give a lower expected payoff, they are both wrong but choosing the "safe" action prevents them from finding it out.

1\2	l	r
L	2,2	2,1
R	0,0	3,1

	terminal information
1	$\{(L,l),(L,r)\},\{(R,l)\},\{(R,r)\}$
2	$\{(L,l)\},\{(R,l)\},\{(L,r),(R,r)\}$

Next I discuss at length the so called "Threat Game" or "Entry Game" depicted below (Fig. 4, p. 114), under the assumption of perfect terminal information. Here CE and Nash equilibria differ, but CE and Nash paths coincide. This is an instance of a quite general result presented in Chapter 4.



An interesting feature of the CE and imperfect Nash path (L) is that it is induced by the non-Nash, CE strategy profile (L, r) which is in turn supported by (a correct conjecture of player 2) and the un-falsified conjecture of player 1 that player 2 would "fight" (l) with sufficiently high probability. On the other hand, the pure Nash equilibrium inducing (L) is (L, l), where 2 is using a clearly irrational (conditionally dominated) strategy (indeed, "l if R" is conditionally strictly dominated by "r if R". Of course, this CE makes much more sense if player 1 does not know the payoff function of player 2. Since the CE concept does not rest on the assumption of complete information, this is not conceptually problematic. The point of this example is that imperfect Nash equilibrium paths can obtain as rest points of learning processes, and are best interpreted as conjectural equilibria where players plan to choose undominated actions also at information sets off the CE path, but may have incorrect (yet confirmed) conjectures. Indeed, it can be shown that every CE where conjectures are consistent with the true extensive form is equivalent to a possibly different CE in which every player plans to use conditionally undominated strategies.[10]

The chapter goes on discussing some deficiencies of the standard strategic-form representation and proposes a different representation given by a "semi-normal form" augmented by a strategic-form *ex post* information structure. "Semi-normal" means that the states of Nature are not integrated out. Essentially, it is as if Nature were a player with a constant payoff function.[11] The ex post information structure specifies for each player j and each strategy s_j of j a partition of the co-players' strategy profiles (Nature is included among the co-players). Note, this partition of co-players' strategies, in general, depends on s_j.[12]

4 Chapter 4: Equilibrium, knowledge, and strategic thinking

In *Chapter 4*, I sketch an analysis of how players' knowledge affects their conjectures, and I try to capture the consequences of strategic reasoning under common knowledge of the game, or complete information.

When the extensive form (but not necessarily the opponents' payoff functions) is known by each player, CE is a special case of what Fudenberg & Kamada [25, 2011] call **partition-confirmed equilibrium**. The latter is more general because I rule out correlated beliefs, which are instead allowed in [25, 2011] an issue I discuss below.[13] If, furthermore, there is perfect terminal information, a special case of my CE is obtained: it is called **self-confirming equilibrium with independent unitary beliefs** by Fudenberg & Levine in "Self-Confirming Equilibrium" [26, 1993a].

The comparison between this early work of mine and the work of Fudenberg and Levine deserves careful discussion. **Self-confirming equilibrium**

[10] See my note [12, 1999]. For similar statements see, for example, [25, 2011].

[11] Interestingly, I use the "semi-normal" in my recent work on rationalizability in incomplete information games. See Battigalli *et al.* [20, 2011].

[12] This strategic-form representation is essentially equivalent to the strategic form used by Esponda [24, 2011] to provide a non-epistemic characterization of rationalizable conjectural equilibrium. Of course, Esponda provides a fully fledged epistemic analysis of this concept. See below.

[13] Also the definition of conjectural equilibrium in Battigalli *et al.* [18, 1992] and Battigalli [12, 1999] allows for correlated beliefs.

(SCE) was independently proposed by these authors with essentially the same conceptual motivations as CE, and it has been given a rigorous learning foundation within a scenario where a game is played recurrently in a society between agents drawn at random from large populations (see Fudenberg & Levine [27, 1993b], see also Fudenberg & Kreps [1995, 1995]). In my thesis, I am vague and informal about the learning scenario, and I do not have this particular one in mind. There are two main differences between my CE and SCE as defined in [26, 1993b]: (i) on the one hand, SCE assumes perfect terminal information, (ii) on the other hand, SCE (reasonably) allows for correlated beliefs and allows different pure strategies in the support of a mixed equilibrium strategy to be justified by different confirmed beliefs. Indeed, an equilibrium mixed strategy is interpreted as a stable statistical distribution of pure strategies in a population of agents playing in the same role (e.g., the first mover of the game), different agents may play different strategies, observe different distributions of outcomes and hence have different stable beliefs justifying those strategies. My notion of CE is instead more appropriate to model the stable outcome of repeated interaction between the same individuals who maximize myopically their subjectively expected payoff without taking into account the impact on the future choices of their co-players.[14] Thus, in a CE each mixed strategy (hence all the pure strategies in its support) has to be justified by a single confirmed belief. This is the **unitary beliefs** condition in the terminology of Fudenberg and Levine. Since expected-utility maximizing players have no positive incentives to mix, I might just as well have assumed that players use pure strategies. Also, in my 1987 notion of CE, I assume that players (if they know the opponents' information structure) have beliefs/conjectures represented by opponents' behavioral strategy profiles, which – by Kuhn's theorem – is equivalent to assuming that conjectures are product measures over pure strategy profiles. I did not have a good justification for this.[15] To sum up, CE and SCE are

[14] In this respect, my notion of conjectural equilibrium is perhaps closer to Kalai and Lehrer's *subjective equilibrium* of a repeated game (possibly with incomplete information and imperfect monitoring), with the difference that I assume players to be extremely impatient whereas they allow for any discount factor. See [34, 1993a], [35, 1993b] and [**?**, 1995].

[15] Actually, I had understood that knowing that co-players are choosing their strategies independently does not exclude subjective correlation, an issue I discuss in the thesis. But somehow I was not yet prepared to accept the consequences of this. Unfortunately, I did not know Aumann's [**?**, 1974] article on corrleated equilibrium.

The basic notion of CE that I now present to my students has players using pure generalizations of the Nash equilibrium concept motivated by the idea of modeling steady states of learning dynamics when a typical game is played recurrently. Fudenberg & Levine's [26, 1993b] SCE is not more general than my CE because it assumes perfect terminal information, nor it is less general, because it allows for non-unitary, correlated beliefs. SCE with unitary, independent beliefs is a special case of the CE concept of my thesis. *Assuming* that players (know the extensive form and) have *perfect terminal information, in two-person games, pure-strategy CE and pure-strategy SCE coincide.*

One can show by example that CE and SCE are strict generalizations of the Nash equilibrium concept. Indeed the two-person game with imperfect terminal information shown above is one such example. An early working paper version of Fudenberg & Kreps TK provides a three-person example with perfect terminal information. Both examples feature pure strategy equilibria. It turns out that violating either the two-person or the perfect terminal information assumptions is necessary to obtain CEs that are not observationally equivalent to Nash equilibria. Indeed, I prove a first elementary equivalence result:

In two-person games (with perfect recall and knowledge of the extensive form)*, each CE induces a mixed Nash equilibrium path* (Teorema 4.4, p. 173).

I show by example, in a discussion of the "Entry Game", that the mixed Nash equilibrium inducing a given CE path may have to be imperfect (non subgame perfect, non sequential). This provides an interpretation for imperfect Nash equilibrium in such games: it may obtain as the stable outcome of learning, given that players do not necessarily know their opponents' payoff functions, or do not necessarily believe that the opponents are (sequentially) rational. This equivalence result has been independently discovered and substantially generalized by Fudenberg & Levine [26, 1993a]: in n-player games with observable deviators (which encompass all two-person games with perfect recall), every SCE with unitary independent beliefs induces a mixed Nash equilibrium path.[16]

As I said above, some CEs do not satisfy obvious properties of credibility and strategic sophistication following from introspective analysis of the game

strategies to best respond to confirmed correlated beliefs/conjectures. I then move on to the more general case of strategy distributions supported by non-unitary correlated beliefs.

[16] Their proof of this result is incorrect because they derive it as a corollary of an incorrect claim (see Kamada [37, 2010]). But one can give an independent proof.

under complete information (common knowledge of the game). This makes sense because CE is a meaningful concept even if complete information is not assumed. Yet, it is interesting to analyze the behavioral implications of such introspective reasoning. In the second part of Chapter 4 of the thesis (section 4.5, pp 177-229)[17] I develop an analysis of what conjectures are consistent with *1,2,...k*, steps of a kind of introspective reasoning that, in dynamic games, features a **forward induction** principle: even if a player observes unexpected moves by the opponent he interpret such move as part of a strategically rational plan of action, which restricts possible beliefs about the opponent's future moves or his private information (see Def. 4.20, p. 205).[18] The result for static games (where forward induction can have no bite) is a *re-discovery of the* (uncorrelated) *k-rationalizable beliefs*.[19] I call a (behavioral) strategy σ_j "**implementable**" (in Italian, "strategia effettiva") if it is a best response to some belief system[20] at every information $h_j \in H_j$ *allowed by* σ_j, I call σ_j "**super-implementable**" ("super-effettiva") if it is a best response to some belief system at *every* information set of j. Since every effective strategy is realization-equivalent to some super-effective strategy, I focus on simple *"implementability" as a basic notion of dynamic rationality.* With this, I can start a recursive definition of "k-reasonableness" of conjectures. A a **conjecture** is "**1-reasonable**" if it is realization-equivalent to a convex combination of mixed representations of "implementable" behavioral strategies of the opponent (or profiles of such convex combinations of mixed representations in n-person games). The reason for taking convex combinations of implementable (i.e. "1-rationalizable") strategies is explained in the example of Fig. 15bis, p. 203: a convex combination of best responses may fail to be a best response, but it represents a reasonable belief.[21] A (behav-

[17]Much of the content of this part of Chapter 4 appeared in [4, 1988]. However, in this article I focus on forward-induction refinements of Nash equilibrium, not of conjectural equilibrium. I motivate this also with the outcome-equivalence result for two-person games cited above.

[18]For a formal, epistemic analysis of forward inductin thinking see [16, 2002] TK.

[19]As I said, when writing my undergraduate thesis I did not know the *Econometrica* 1984 seminal papers by Bernheim [21, 1984] and Pearce [43, 1984].

[20]A belief system assigns a conjecture to every information set so that conjectures are not changed unless they are falsified. In the thesis I call such a belief system "regola d'apprendimento" ("learning rule") because it determines how a player's beliefs change in response to information. In can be shown that a belief system in this sense is equivalent to a conditional probability system. See [7, 1994].

[21]Interestingly, my good instinct allowed me to avoid a rather common conceptual mis-

ioral) **strategy** σ_j is **2-implementable** if it justified at each $h_j \in H_j$ allowed by σ_j by a belief system that assigns 1-reasonable information conjectures to information sets in H_j *whenever possible.* This is how I capture basic forward induction reasoning, i.e. a belief in opponents' rationality whenever possible. The inductive step of the recursive definition is similar: at step $k+1$ a player is assumed to believe in the k-implementability whenever possible. No further forward-induction condition is imposed. For extensive form games, I obtain an algorithm that may monotonically converge to a solution set or not. Non-convergence reflects "paradoxes" arising from counterfactual reasoning in games: fewer information sets are consistent with k-implementability than with $(k-1)$-implementability, hence my forward-induction restrictions may be non-monotonic in the number of steps (see example in Fig. 16, p. 206). When convergence occurs, the result is akin to Pearce's [43, 1984] extensive-form rationalizability (equivalent, if there are no chance moves).[22]

Restricting attention to two-person static games,[23] the conjectural equilibria with k-rationalizable beliefs defined in the thesis are similar in spirit, but not equivalent to the k-rationalizable conjectural equilibrium concept (see Rubinstein & Wolinsky [45, 1994], Battigalli [12, 1999], Esponda [24, 2011]). Here is the difference (explained with the benefit of hindsight): conjectural equilibria in k-rationalizable beliefs (of static games) capture the implications of assuming that players are rational, their conjectures are confirmed, and there is level-k mutual belief of rationality (common belief as $k \to \infty$); on the other hand, k-rationalizable conjectural equilibrium relies on the assumptions of rationality, confirmed conjectures and level-k mutual belief of rationality *and* confirmation of conjectures. In other words, my refined CE concept does not capture mutual (or common) belief in the confirmation of conjectures. Arguably, sophisticated players should not expect

take, i.e. to represent "reasonable beliefs" in extensive-form games by taking convex combinations of "reasonable" behavioral strategies, instead of combinations of their mixed representations, or – equivalently – taking Selten's [46, 1975] "behavioral strategy mixtures".

[22] On extensive-form rationalizability see [10, 1997] and [16, 2002]. Indeed my algorithm has also the same flaws of the one proposed by Pearce. I discuss and "fix" these flaws in [**?**, 1996]. This is related with how Pearce and I tried to model independence using a kind of structural consistency property of belief systems (see below). The correlated version of Pearce's rationalizability is conceptually correct, as shown by my epistemic analysis in [16, 2002].

[23] I focus on two-person game to avoid, in this particular comment, the correlated-*vs*-independent beliefs issue.

their co-players to keep playing the same strategies if they think that the co-players' conjectures may have been disconfirmed (of if they think that someone else may think this, etc.); hence, my refinement is somewhat naive. In the more general case of extensive-form games, conjectural equilibrium in k-rationalizable beliefs is a forward-induction refinement of CE somewhat similar to Reny's [44, 1992] "explicable equilibrium".[24]

The fourth chapter also considers the relationship between my version of k-rationalizable CE (equilibrium in k-reasonable conjectures) and other solution concepts. I point out a (back then) disturbing fact: sequential-equilibrium assessments need not be 1-rationalizable (1-implementable). This has nothing to do with sequential equilibrium failing forward induction, because – as explained above – my version of k-rationalizability incorporates forward induction only from step $k = 2$ on. The reason is what came to be known as the possible "structural inconsistency" of sequential equilibrium assessments in Kreps and Ramey [38, 1987], which appeared shortly afterward. According to **structural consistency** the conditional beliefs of a player at any information set h should be derived from a *product measure* on the set of strategy profiles S_{-i} of the opponents, by conditioning on the set $S_{-i}(h)$ of such profiles that allow h. I was shocked to find out that sequential equilibrium assessments may fail structural consistency (see example in Fig. 17, p. 215 and the similar example in [4, 1988], [38, 1987] and [42, 1994] Fig. 229.1). Then I pointed out that Selten's [46, 1975] perfect equilibria are 1-implementable (Teorema 4.6, p. 220).[25] *I now think that such structural consistency is just an ill-conceived property and therefore the lack of structural consistency should not bother us at all.* The fact that I and other theorists found structural inconsistency disturbing depended on a misleading formal language and an incorrect way to way to think about updating and stochastic independence in dynamic games.[26]

[24]In [14, 1988] (published only 9 yers later as [15, 1997]) Danilo Guaitoli and I apply k-rationalizable CE to the analysis of a macroeconomic dynamic game with incomplete information.

[25]Actually, since the claim refers to extensive-form (or agent-normal-form) perfect equilibria, the claim is only true generically with respect to payoffs on terminal nodes. The reason is that ties may induce "coordination failures" between different agents of the same player. The correct version of the claim appears in my [4, 1988] article and refers to normal-form perfect equilibria. See Reny [44, 1992] for a similar result.

[26]Interestingly, also Pearce's definition of extensive-form rationalizability take structural consistency for granted. In my later work I clearly explain the flaws of structural consistency and of the theoretical concepts based on it, and I show that Kreps & Wilson's

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consistency (despite claims to the contrary by Kreps and Ramey [38, 1987]) is a strong stochastic independence property (see [6, 1993], [7, 1994], [8, 1996], [9, 1996] and [17, 1996]).

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