# An extra section (#10) for "Belief-Dependent Motivations and Psychological Game Theory"

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**Abstract**

We discuss several methodological issues that didn't make it into the regular nine sections of our 2022 *Journal of Economic Literature* article (vol. 60(3)). These concern how to model economic settings as well as how to generate predictions.

## 10. Further methodological considerations

**Solution concepts** PGT-analysis involves two key steps: (i) modeling belief-dependent utility, and (ii) applying a solution concept. Step (i) is unique to p-games, step (ii) is relevant also for traditional game theory. Our main goal has been to emphasize what is unique to p-games, so we have focused mostly on step (i). However, since we feel strongly about step (ii), let us explain our view.

Economists frequently take for granted that ad hoc notions of equilibrium (whereby players are assumed to have correct beliefs) meaningfully describe strategic interaction. Often such assumptions are not well justified. In one-shot play settings, if players reason about each other's rationality and beliefs, inferences should concern steps of deletion of non-best-replies (possibly all the way to a form of rationalizability). If learning is allowed by looking at recurrent interaction, the appropriate solution is (some version of) self-confirming equilibrium, in which beliefs may be incorrect, although consistent with

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evidence. In neither case is the most commonly applied solution—sequential equilibrium—generally implied.[1] Only in rare cases is it justified to assume that a sequential equilibrium will be played, most notably when this solution concept yields the same prediction as rationalizability, or self-confirming equilibrium.[2]

Since a proper discussion would call for its own article, we have not gone there. Our approach has mainly been consistent with our favored view as we focused on steps of deletion of non-best-replies. But since previous scholarship (including ours) often referred to more traditional notions of equilibrium, we made a few related references when recalling such work.

We hope future work will take the appropriateness and relevance of solution concepts more seriously than has been done in the past.

**Transformations of the game form** A small literature studies transformations of the game form (with monetary payoffs)[3] that do not change the reduced normal form (see in particular Thompson 1952, Dalkey 1953, and Elmes & Reny 1994). Considering only those that preserve perfect recall, they are: interchanging essentially simultaneous moves (INT),[4] coalescing sequential moves (COA; inverse: sequential agent splitting), and addition of a superfluous move (ADD). Any solution concept that just requires knowledge of the normal form, such as Nash equilibrium, or iterated admissibility, is obviously invariant to such transformations (and their inverses). Yet, such invariance cannot be a dogma. Note that the most used solution concept, sequential equilibrium, is only invariant to INT. Versions of "extensive-form" rationalizability for games with sequential moves are invariant to INT and COA, but not ADD.[5] Our general view is that it is interesting to study the invariance properties of independently motivated solution concepts, without viewing violations of some form of invariance as a flaw. We have just one *caveat*: If simultaneity of moves is represented indirectly, by letting players move in an arbitrary sequence and defining information sets so that the choices of early movers are not observed, then invariance

[1] B&D extend Kreps & Wilson's (1982) classic notion of sequential equilibrium to p-games. See BC&D for relevant p-games definitions of all solution concepts mentioned above. See Battigalli, Corrao & Sanna (2020) and Jagau & Perea (2018) for epistemic foundations of (versions of) rationalizability.

[2] A special circumstance may apply in $G_{10}$ (see Section 4.1) given the interpretation that player 2 is player 1's "imagined" audience (as hinted at). If 1 is, so-to-say, "his own audience," we have a single active player, so "forming equilibrium expectations should be easy" (Dufwenberg & Dufwenberg, p. 262).

[3] The literature refers to "extensive-form games," which include a specification of players' payoffs at terminal nodes. We interpret such payoffs as monetary one, hence we have what we call "game forms".

[4] Note that the formalism initially adopted to describe games indirectly represented simultaneous moves in an arbitrary sequential order without flows of information to late movers.

[5] See the discussion and characterization by Battigalli, Leonetti & Maccheroni (2020).

w.r.t. INT is a must.[6]

How does this relate to the analysis of p-games? We have a two-tiered answer:

(i) In general p-games, players' utility depends on the *temporal* sequence of beliefs. This means that an accurate representation must explicitly account for time and distinguish between periods and stages within periods, as–say–in models of bargaining protocols. Transformations involving moves across periods should be expected to have an impact on behavior. For example, as shown by Gneezy & Imas (2014), angry agents "cool off" as time goes by and are therefore less willing to harm others (see the discussion of fast *vs.* slow play in the working paper version of Battigalli, Dufwenberg & Smith). Consider a game form with time in which player $i$ in period $t$ (when he may be frustrated) decides to stop ($S$) or continue ($C$). If $C$ is chosen, the play moves to period $t+1$ when $i$ can harm $j$ ($H$) or not ($N$). Since $i$ in period $t+1$ may have cooled off, a COA transformation making $i$ choose either $S$, or $C\&H$, or $C\&N$ in period $t$, makes it more likely that $i$ harms $j$. Thus, it is important to use rich and accurate representations of the rules of the game (see BC&D for a first step in this direction). The rules of games played in the field and in the lab, by necessity, imply that information accrues to both active and inactive players. Since information affects beliefs, we should formally represent the information of inactive players. Also, the rules of the game only specify information *flows*, not how much information is retained by players. To keep a sharp separation between game form and players' personal features, the former should specify information flows as, for example, in Myerson (1986), or in the literature on repeated games with imperfect monitoring (e.g., Mailath & Samuelson 2006). The use of traditional information partitions is acceptable under the presumption that players have perfect recall (and this is common knowledge).

(ii) Compared to traditional game theory, invariance with respect to a particular transformation depends also on the form of psychological utility functions, not only on the solution concept. For example, if utility depends only on the initial, or the terminal beliefs of others, sequential equilibrium is invariant w.r.t. INT (which, however, may not be applicable if simultaneity is represented directly as we do), whereas extensive-form rationalizability is invariant to both INT and COA. Yet, utility may depend on beliefs in ways that prevent any form of invariance.

**Private sensitivities and incomplete information** Let us expand on the topic of incomplete information. In preceding sections $\theta_i$ often denoted player $i$'s "sensitivity" with respect to some psychological concerns (e.g. guilt or reciprocity). It makes sense to assume that player $j$ is not informed of $\theta_i$, and has to form beliefs about it in order to predict $i$'s behavior. In this case the analysts must consider beliefs in p-games with

[6] See the characterization of INT-invariance by Bonanno (1992).

incomplete information. We already discussed this topic, in Section 4, where, however, we focused on image concerns, i.e., how $i$ might care about $j$'s beliefs about $\theta_i$. However, even absent image concerns, incomplete information is very natural and important. If $\theta_i$ measures $i$'s sensitivity to feel, e.g., guilt, reciprocity, or anger, then $j$'s beliefs about $\theta_i$ may matter not because $i$ cares about those beliefs but because they will impact $j$'s choices.[7]

Most applied work using PGT either assumes complete information, or (legitimately) ignores the issue by focusing on the shape of the best reply correspondences, without using solution concepts. The analysis of psychological games with incomplete information is developed in Attanasi, Battigalli & Manzoni (2016), Bjorndahl, Halpern & Pass (2020), and BC&D, which is more general and systematic. Incomplete information is the lack of common knowledge of some features of the game form (feasible sets, information structure, material payoff functions) or of players' preferences. As explained in BC&D, if we rely on solution concepts with an epistemic foundation (versions of rationalizability) or a learning foundation (versions of self-confirming equilibrium), incompleteness of information can be addressed directly by means of relatively straightforward extensions of such solution concepts.[8] The situation is different if the analyst maintains the traditional equilibrium assumption that players have correct conjectures about the decision rules associating co-players' private information with their behavior. In this case, following the seminal contribution by Harsanyi (1967-68), the traditional approach proceeds in two steps. It first posits an *implicit* representation of players' possible interactive beliefs about the unknown parameter vector $\theta$ by means of "types" encoding both private knowledge about $\theta$ and hierarchical exogenous beliefs; this yields a "Bayesian game". Then one proceeds to analyze the Bayesian perfect (or sequential) equilibria of such a game, that is, profiles of decision rules whereby every type of every player carries out a sequential best reply to the co-players' decision rules given the beliefs of this type. The key to connecting Harsanyi's approach to PGT is that, in a Bayesian equilibrium, each type of each player is associated to a hierarchy of (exogenous *and* endogenous) beliefs, which—in a p-game—enters the utility functions. This allows us to obtain all the known "traditional" equilibrium concepts (those that postulate correct beliefs, as in GP&S and B&D) as special cases of Bayesian perfect equilibrium. Furthermore, Harsanyi's approach gives an additional degree of flexibility seldom noticed by non-specialists: a Bayesian game may feature multiple types of the same player with the *same exogenous* hierarchy of beliefs, even when there is complete information! This allows for equilibria where types with the same exogenous belief hierarchy have different plans and different beliefs about co-players. Since observed

[7]For analyses of games where players are uncertain about each others' such sensitivities, see Attanasi, Battigalli *et al.* (2013, 2016, 2019) and Battigalli, Charness & Dufwenberg (2013, footnote 5) on guilt, Sohn & Wu (2019) on reciprocity, and Aina *et al.* on anger.

[8]See Sections 7.1-2 of BC&D and the references therein.

actions signal types, in equilibrium players may update their higher-order beliefs about the beliefs (and plans) of co-players, hence their intentions, which is instead prevented by the equilibrium concepts of GP&S and B&D.[9]

**Identifiability and revealed preference** PGT is criticized by some theorists who point out that key psychological parameters cannot be identified by means of the standard revealed-preference approach. To see the point, let us maintain the assumption of monetary consequences. The standard utility of terminal nodes $u_i : Z \to \mathbb{R}$ of each player $i$ should be derived from a utility of monetary consequences $v_i : [\underline{m}, \bar{m}]^I \to \mathbb{R}$ as follows

$$u_i(z) = v_i\left((\pi_j(z))_{j \in I}\right), \tag{1}$$

where $v_i$ represents $i$'s preferences over lotteries of payoff vectors via expected utility calculations (note that we allow for other-regarding preferences). Utility function $v_i$ can be approximately identified[10] by observing many choices between lotteries, under the assumption that such choices are consistent with a preference relation satisfying the axioms of expected utility theory. Now consider a utility function that captures guilt aversion, such as

$$v_i(m_i, m_j, \mu_j) = f_i\left(m_i, \left[\mathbb{E}\left[\widetilde{m}_j; \mu_j\right] - m_j\right]^+\right), \tag{2}$$

where $\mathbb{E}\left[\widetilde{m}_j; \mu_j\right]$ is how much $j$ initially expects to get given his belief $\mu_j$ about his monetary payoff, and $f_i$ is increasing (resp., decreasing) in its first (resp., second) argument. This is a kind of state-dependent utility function, where the state is the belief of someone else. The standard theory of subjective expected utility (SEU) with state-dependence cannot separate utility from beliefs, thus it does not allow identification. To what extent is this a problem?

First note that the identifications allowed by the standard theory always come with the aid of farfetched auxiliary assumptions. We think that the same can be done also with some forms of belief-dependent utilities. We gave examples of belief-dependent motivations, such as guilt aversion and image concerns, where psychological utility is a function of the material consequences (monetary payoffs) and an unknown state, the beliefs of others about material consequences and/or parameters. In a series of papers, Edi Karni and coauthors (see, e.g., Karni & Schmeidler 2016 and the references therein) give axiomatic foundations of state-dependent SEU that allow for separation between beliefs about the state and the utility of outcome-state pairs. Under such axioms and ancillary auxiliary assumptions of the same kind as above, the utility type can in principle be identified. A polar case

[9] See Section 7.3 of BC&D.

[10] Up to positive affine transformations.

also amenable to axiomatization and in-principle identifiability is given by how Caplin & Leahy model anticipatory feelings, drawing on Kreps & Porteus' work on preferences over temporal lotteries.

Yet, we do not claim that all interesting forms of belief-dependent utility give rise to in-principle identifiability. Is this a serious problem? Well, it would be nice to have such identifiability to be able to bring models to data in more ways than we currently do. *No more than that.*

From the perspective of the philosophy of science, we would have a "serious problem" only if we had to accept the *hard form of the revealed-preference approach* advocated by some decision theorists, who reject the use in economics of theoretical terms that cannot be derived from objects identifiable starting from supposedly observable preferences. We agree with the scholars who classify such approach as a special form of operationalism. Our attitude toward operationalism is the same as that expressed by Suppe (1977) in his long introduction to the collective volume on *The Structure on Scientific Theories* (essentially a monograph on the philosophy of science to which scholars in this field still refer for a textbook-like treatise): this doctrine is mentioned just in a footnote. That is, like all the philosophers of science we know, *we reject operationalism.* We think that those who invoke forms of operationalism (usually, not even showing that they are aware of such a link) to ban works on psychology and economics as "non-economics" are deeply wrong and exert a negative externality on the profession. In sum, no, we do not consider such a lack of identifiability a serious problem. It is possible to derive predictions about observables (behavioral predictions) from our PGT-based models in carefully designed experiments. Indeed, we do it, and sometimes the predictions are inconsistent with data, thus inducing a search for better models with new predictions, as should be the case in good science.

Many of the points we now discuss in Section **??** ("Experiments") concern special techniques that experimentalists have developed for the purpose of testing and measuring aspects of belief-dependent motivation (e.g., how to elicit, or disclose/induce, beliefs, as well as "Other forms of data").

**Game-form (in)dependence of psychological motivations** A related methodological issue is the following. As discussed above, standard game theory can work with preferences over (lotteries of) outcomes/consequences, which can (in principle) be identified independently of the game form in which agents are going to interact. This allows to predict responses to institutional changes, which determine, or modify the game form. Such exercises in mechanism design seem very hard, or even impossible, according to PGT, which apparently features belief-dependent utility functions intrinsically linked to the game form. To what extent is this a problem?

First note that many models of preferences that can be incorporated into traditional game theory refer to the strategic situation, i.e., depend on the game form. Also note that some (axiomatized) models of decision under uncertainty such as the celebrated maxmin model of Gilboa & Schmeidler (1989) suffer from the same problem, as they intrinsically mix personal traits like ambiguity aversion with situational features (what probability measures over states are allowed for in the given situation?). Also, one must distinguish between the framework (PGT) and the models that can be expressed in such framework (more on this below). Some models of belief-dependence (e.g., guilt aversion) represent "portable" game-form independent personal traits that can be appended to any game form, as explained in the methodological paper by BC&D. However, some preferences, including some belief-dependent preferences, are situation dependent. The preference for reciprocation is a case in point: the range of available distributions is key to determine the "kindness" of an action. Intuition, folk-psychology, and evolutionary psychology all suggest that situational cues are important in shaping motivation. So, as a first start, we had to make do with situation-dependence. What is important is to model it in a systematic way. We should aim at a theory where how motivations are shaped, or even (dis)activated by situations is included in an agent's personal features. This is an important theme for future research, and it does not only concern game forms played by agents who carry belief-dependent preferences.

**Does PGT have predictive power?** It is often argued that game theory can explain almost everything because its predictions depend on fine details about the order of moves, the information structure, etc. that can hardly be identified in the field. Yet, it is further argued that GT at least provides a sense of what it takes to get a particular outcome: given knowledge of the game form and players' (identifiable) preferences over outcomes, standard solution concepts provide predictions. Such conditional predictions seem much harder to achieve with PGT. For example, as shown in the seminal article by GP&S, belief-dependent utilities allow for the possibility of multiple non-equivalent equilibria in games with only one active player. With this, PGT seems able to explain just about everything even in controlled settings. To what extent is this a problem?

We think that such critiques and doubts often stem from a misunderstanding of what (psychological) game theory is. Like traditional game theory, PGT *is not a theory in the usual sense*, but rather a language that allows us to build mathematical models of interaction, to obtain some useful abstract results, to frame issues. The models depend on some (occasionally implicit) assumptions. For example, von Neumann & Morgenstern (1944) did not bother representing the information of inactive players (something that, objectively, exists, as it is implied by the rules of real games), because they took for granted that it is irrelevant. Other game theorists like Kuhn followed suit, without ever even mentioning the issue until (to our knowledge) the Gilboa & Schmeidler (1988) article we cite. Similarly, simultaneous moves have not been given a direct and explicit formalization in general extensive-form representations of games for several decades, because the difference with sequential moves without flow of information between them was (in this case, explicitly) deemed irrelevant.

The intuitions ascribed to traditional game theory are developed (with the help of solution concepts) within models, or classes of models. The same can be done, and is done in applying the methods of PGT. The example of GP&S just illustrates the differences between traditional game theory and PGT, nothing more than that. It cannot be used to infer that PGT allows for everything. This would be as naïve and wrong as criticizing traditional game theory because, say, it allows for too many outcomes when used in the theory of industrial organization.

**Is PGT too complex?** The methodological papers by GP&S, B&D, and BC&D may seem to suggest that once we enter the realm of psychological games and belief dependence, we have to deal with every technical complexity and deviation from standard economic modeling at once: Dynamic programming cannot always be used. As argued above, traditional solution concepts like Nash and sequential equilibrium are even harder to justify, making alternatives like rationalizability and self-confirming equilibrium more relevant. Incomplete information is more pervasive than in traditional theory, as co-players' belief-dependent motivations are much harder to know. Agents who have no decisions to make can affect outcomes so that it is not even clear who the players are. Utility functions may be history-dependent, making dynamic inconsistency endemic. Isn't this too much complexity to impose on modelers?

We do not mean to suggest that all sorts of complexity have to be faced at once. We hope our paper does not give this impression. Actually, we like work that deliberately abstracts from some realistic aspects, and highlights others. Why take on everything at once? To give a case in point, take Dufwenberg & Nordblom's (2022) work on "Tax Evasion with a Conscience," which incorporates guilt-from-blame in a 4-player $2\times1\times2\times1$ Inspection Game form and shows how it matters whether tax returns are private or public, a consideration which affects information across end-nodes. The passive players are, respectively, unaware of a tax rule and an observing neighbor. While they cannot actively choose, they are crucial to the analysis via their beliefs. The analysis abstracts from many things, including that life isn't a $2 \times 1 \times 2 \times 1$ game form, that guilt sensitivities are not commonly known, and that in fact people care for more than guilt and money (e.g., anxiety and getting even). The authors nevertheless think their analysis is useful (and tractable).

Once more we have to remember that PGT (like traditional game theory) cannot be more than a framework/language to build models where interacting agents have (in the case of PGT) belief-dependent preferences. Different models have different non-standard features. For example, our work clarifies that—barring changes in belief-dependent preferences—a necessary condition for dynamic inconsistency is own-plan dependence, which is featured by some, but not all PGT models.[11]

Let us now consider the claim that "agents who have no decisions to make can affect outcomes so that it is not even clear who the players are". On the contrary, who the relevant agents (the "players") are is (relatively) clear in experiments such as on the Dictator Game: some agents are active at some nodes, others receive material payoffs that depend on the behavior of active agents, but are not active at any node.[12] These are objective features of the game form designed by the experimenter. For interactions occurring out of the lab some of the features of the real game form may be hard to identify, as is very often the case when we try to understand the objective features of interactive situations in real life, independently of whether we want to use PGT models. What is different between traditional game theory and PGT is that features of the real game form that do not matter according to traditional game theory may matter in PGT. The information players have when they are inactive is a prominent example, the relevance of which is demonstrated by several experiments. Such information is necessarily present in the real game form, but it is irrelevant according to standard GT, which may have been the reason that von Neumann & Morgenstern (1944) decided not to include it in their extensive-form formalism. (An amended formalism that represents such information and gives a direct representation of simultaneity is about as complex as the formalism of von Neumann & Morgenstern and Kuhn. Actually, we find it easier to understand because it has a less artificial flavour.)

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[11] Changes in belief-dependent preferences may be due standard reasons, like non-exponential discounting, or to less standard ones, like the action tendencies of emotions triggered by the observation of non-fully expected events. See Section 6 of BC&D.

[12] It can be argued that in some experiments subjects regard the experimenter as a kind of passive player.

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