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Bridging the gap between research traditions: on what we can really learn from Clifford Geertz

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ABSTRACT
Interpretive researchers, also in our field, often refer to Geertz’ work. They focus on taking the natives’ point of view, thick description and reading culture as a text. In this paper, it is argued that these guidelines cannot provide good reason to accept one interpretation of a social phenomenon over any other interpretation. On closer inspection, however, Geertz’ work displays a stronger approach. This approach on important points fits Lakatos’ ideal of sophisticated falsificationism. This reexamination of Geertz’ actual approach urges to reconsider the strict dichotomy that is often made between interpretive and other types of research in Public Administration and Political Science.

KEYWORDS
Political Science; public administration research; methods; interpretive research; Geertz

Introduction
Interpretative studies in the field of Public Administration and Political Science remarkably often refer to work by Clifford Geertz. His most cited text is Deep Play. Notes on the Balinese Cockfight (Geertz 1973a). Two other essays to which many references can be found are Thick Description (Geertz 1973b) and From the Natives’ Point of View (Geertz 1983). Some scholars refer to these texts in a general way for their inspirational approach (e.g. Rhodes 2011, 202, 208; Schwartz-Shea and Yanow 2012, vii; Stivers 2008, 1011; Thompson 2001; Yanow 2006, 5). In course books and overviews, these texts are often presented as classics (e.g. Brower, Abolafia, and Carr 2000; Dodge, Ospina, and Foldy 2005; Schwartz-Shea and Yanow 2012, 49; Wagenaar 2011, 18; White 1999, 37, 49, Yanow 2000, 7, 40). They are also routinely mentioned in research articles (e.g. Aagaard 2012, 735; Fleming 2008, 622; Geddes 2012, 950; Van Hulst 2012, 302; White 1999, 111). It is not difficult to grasp the attraction of Geertz’ texts, especially that of Deep Play. This text tells the fascinating story of two researchers who arrive in a Bali village [‘malarial and diffident’ (Geertz 1973a, 413)] to study local customs. They get caught up in a police raid while attending an illegal cockfight (a circumstance that helped them gain the villagers’ trust). Deep Play also presents a colorful picture of Balinese practices (e.g. of men toddling cocks, of cockfights and the associated gambling) and offers an encompassing interpretation of these cultural phenomena, suggesting that cockfights are fundamentally a dramatization of status concerns (Geertz 1973a, 437). Thus, his work is a combination between literature and a research report. With humor and some suspense, Geertz moves in
a clear style from description and interpretation to theoretical reflection on doing interpretative research. He teaches and, at the same time, practices the idea that research should be focused on the meanings that actions have for people, not on pointing out causal mechanisms. He writes that such understandings must be presented in ‘thick descriptions’ of ‘webs of meaning’ and that culture should be read as a text.

The scholars that refer to Geertz tend to use him as an example of a particular way of doing research. This interpretive approach offers an alternative to causal explanatory, data-focused, measuring and falsificationist types of study (for such categorizations, often in the form of dichotomies, see Brower, Abolafia, and Carr 2000; Raadschelders 2013; Ricucci 2010; Schwartz-Shea and Yanow 2012, 113; Yanow 2000, vii).

Given the exemplary status that is given to Geertz, the interesting question is whether his interpretation of the meaning of Balinese Cockfights in Deep Play is true, and if so, why is this the case? What, according to Geertz, makes an interpretation a good interpretation? Some commentators claim that Geertz, in fact, does not offer many useful criteria to determine whether an interpretation is good. According to Geertz, ‘a good interpretation of anything – a poem, a person, a history, a ritual, an institution, a society – takes us into the heart of that of which it is the interpretation’ (1973b, 18). He also claims ‘a study is an advance if it is more incisive than those that preceded it’ (1973b, 25). A good interpretation brings us ‘in touch with lives of strangers’ (1973b, 16). These are very vague criteria, as Geertz himself acknowledges (Jones 1998, 39; Martin 1993, 278; Shankman 1984, 263). That also seems to hold for his most-cited advice about interpretative research: that we should try to understand people ‘as they understand themselves’ and that we should use thick description to confer their meanings. According to critics, such as Jones, ‘Geertz’ interpretive analysis of the Balinese cockfight is fascinating and ingenious. It makes marvelous reading. It is also entirely arbitrary’ (Jones 1998, 45). If Jones is right, scholars who follow Geertz’ example seem to be in serious trouble. Their interpretation of the meaning of actions of politicians, civil servants or other actor might not be better than any other interpretation (see Zwart 2002). Are the critics right, or are there convincing criteria for distinguishing between better and worse interpretations to be found in Deep Play?

In this paper, I want to show that Geertz’ approach in Deep Play can indeed be exemplary for interpretive studies in the social sciences. However, the true epistemological strength of his approach lies in a different aspect than what is most often cited. The most important part of his approach is not ‘taking the natives’ point of view’, presenting a thick description, or reading culture as a text. Instead, the most crucial part of his approach is the way in which Geertz shows that his interpretation can do what another interpretation cannot. His approach proves to be much closer to the one that is mostly associated with the opposite camp: positivist falsificationism. This insight urges us to revise the often-presented dichotomies between interpretative approaches (qualitative, meaning and making sense) and positivist (quantitative, causal explanation and testing) approaches.

Geertz’ Deep Play has been the subject of efforts of unmasking or deconstruction, a fate of every classical text. Some have noted the rhetoric skill that Geertz employed in this text to present his conclusions as evident (Watson 1989, 29; compare Warnke 2011). Others have focused on his unsubstantiated claims concerning Balinese sentiments and orientations, claims that seem to express sexist, western and colonialist
prejudices (Crapanzo 1986; Lichterman 2011; Roseberry 1982). My intention, however, is not deconstruction or even to challenge the work of Geertz. The intention of this paper is instead to better understand his approach to show wherein his true example lies.

In the Participating observation and thick description section, the items in Deep Play that are often cited as exemplary will be examined, and it will be shown that neither offers a convincing criterion for judging which interpretation is best. In the Comparing interpretations section, a closer inspection of Geertz’ actual argument in Deep Play will bring about a more convincing way of judging the epistemological status of an interpretation. In the last section, implications of this analysis for research in Public Administration and Political Science will be discussed.

**Participating observation and thick description**

Three aspects of Deep Play are most often cited as exemplary for interpretive research in the social sciences:

- trying to understand people as they understand themselves;
- offering understandings by presenting thick descriptions and
- using text-analysis as the paradigm for studying societal meanings.


In this section, I will take a closer look at each of these factors and consider whether and to what extent they provide criteria for determining the quality of an interpretation.

**Understanding people as they understand themselves**

Deep Play opens with a fascinating narrative of Geer tz and his wife, both anthropologists, arriving in a small Balinese village in the late 1950s. Although they were officially accredited by the Indonesian government and accommodated by the village chief, the villagers treated them as if they were not there, as if they were nonpersons. After 10 days, however, they visited a local cockfight and their status abruptly changed. Cockfights are illegal in Bali. One day, while attending a cockfight, Geertz and his wife find themselves, during the third match, in the middle of a police raid. As the villagers started to run in all directions, they decided to do the same, and they took shelter with other villagers in one of the compounds. When the police later wanted to question those present, Geertz and his wife joined in on acting innocent. After this incident, the villagers completely changed their attitude toward the researchers. They no longer treated them as outsiders, and the whole village opened up to them. This represented a turning point in their relationship with the community. ‘[W]e were quite literary “in”’ (1973a, 416). Geertz continues his story by explaining how important this entrance into the community was for his research. It enabled the anthropologist to get ‘the kind of immediate, inside view grasp’ of the mentality of the people he was studying (1973a, 416). He expresses this
necessity of anthropological fieldwork’ (1973a, 416) time and again, in different wordings: ‘One has to learn how to get access to [societies and lives that] contain their own interpretations’ (1973a, 453). One needs to determine ‘[h]ow to get in touch with the lives of strangers’ (1973b, 16). That means crossing ‘somehow, some moral or metaphysical shadow line’ (1973a, 413). If you succeed, you can ‘read over their shoulder’ and have ‘a revelation of what being a Balinese ‘is really like’’ (1973a, 452, 417). Of course, getting almost caught in a vice raid was helpful for Geertz and can hardly be presented as general methodological advice. Yet, the gist is clear, one has to become, in whatever way possible, an insider.

It should be the researcher’s ambition, according to Geertz, to get in and then to grasp the native’s point of view. He should try to understand how they understand themselves. His own study of cockfights reports ‘a Balinese reading of Balinese experience. A story they tell themselves about themselves’ (1973a, 448). ‘Societies (...) contain their own interpretations’ (453). ‘The Balinese peasants are quite aware of all this [what cockfights are all about] and can, at least to an ethnographer, do state most of it in approximately the same terms as I have’ (440, see also 433: ‘for the Balinese, though naturally they do not formulate it in so many words’).

‘Getting in’ as a criterion for interpretation?

Geertz uses a story to explain the value of ‘getting in’. Neither in Deep Play nor in his other essays does he offer much of a method for anthropological fieldwork. It seems safe, however, to understand him as an advocate of long-term residence among the people one studies, participating in observation and being versed in the local language and idiom (Gottowik 2004, 158). One issue is, of course, whether one, as a researcher, can truly understand the natives’ point of view. In a later essay, Geertz explained that taking the natives’ point of view has its limits. A researcher can and must try to understand (‘verstehen’) what actions, such as cockfights, mean to the people he or she studies. That does not mean, however, that he or she must, or even can, appreciate (‘einfühlen’) these actions in the way the subjects do (1983, 13). To understand the natives’ point of view, one thus does not (completely) need to go native.

Yet, even given this distinction between understanding and appreciation, it is still problematic to present the understanding that people have of themselves. The point is illustrated by the interpretation Geertz himself gives of Balinese cockfights in Deep Play. He tells us that these cockfights are ‘fundamentally a dramatization of status concerns’ (1973a, 437). This, of course, can hardly be the language and idiom of Balinese villagers. In fact, this is acknowledged by Geertz: ‘for the Balinese, though naturally they do not formulate it in so many words’; ‘the Balinese peasants are quite aware of all this and can, at least to an ethnographer, do state most of it in approximately the same terms as I have.’ (1973a, 433, 440; the emphasis is mine). In another essay, he maintains that ‘anthropological writings are themselves interpretations, and second and third ones to boot’ (as they are based on ‘our own interpretations of what our informants are up to or think they are up to’) (1973b, 15). They are ‘our own constructions of other people’s constructions of what they and their compatriots are up to’ (1973b, 9). Here, Geertz concedes that the interpretation of a researcher is inevitably his interpretation. There is a logical distinction between the subject’s and the researcher’s understanding. This need not be a fatal flaw for interpretive research. Yet, it is a problem for those who would like
to maintain that a good (or true or best) interpretation is the one that offers the best self-understanding of the subjects under study (compare on this point the work of Warnke 2011, 48). Whatever following the rules anthropological fieldwork can accomplish, it cannot lead to ‘the natives’ point of view’ and therefore cannot be a criterion for the truth or quality of an interpretation.³

**Thick description**

After the presentation, in *Deep Play*, of his entrance into the Balinese community, Geertz turns to the phenomenon of cockfights. He shows that cocks and cockfighting have a manifest role in Balinese language and folktales. He provides colorful sketches of the ways in which, in popular understanding, cocks and their behavior are linked to manhood. He describes the activities and worries of owners and trainers of cocks between the fights and explains how cocks are prepared for fights. He also gives us a lively picture of the cockfight itself: the rules, the roles of the officials, the spectators, the complicated betting system (including center bets between cock owners and side bets between spectators) and, of course, the fights between the cocks. Sharp spurs are fixed to the cock’s legs, making the fights bloody and often lethal.

Geertz presents us not with data but with what he calls, after Ryle, a thick description (1973b, 6). For Geertz, offering an understanding of what a cockfight is about is different from saying that it comes down to ‘a chicken hacking another mindless to bits’ (1973a, 449). To make the meaning of human activities understandable, one must do more than describe basic characteristics. It is only by a thick description that one can distinguish the (meaningful) wink from a mere twitch of the eye (1973b, 7). To bring the meaning of actions to the level of understanding, the researcher must show the intentions, expectations, circumstances, settings and purposes involved (Greenblatt 1999, 16). Geertz, with approval, cites Weber, saying ‘that man is an animal suspended in webs of significance he himself has spun’ (1973b, 5). Showing what someone means by something demands making the many threads of the web of meaning visible.

**Thick description as a criterion for interpretation?**

Thin description, for Geertz, means presenting an act without providing it with any meaning. A ‘thin description’ consists of only meaningless data – to the extreme: a set of figures. A thick description, on the other hand, provides us with more than a minimal external depiction of an action. Yet, when is one thick description better than another one? Put differently, when does a thick description make for a better interpretation of the meaning of an action? Is a longer description, a more detailed description, a description that encompasses more levels of meaning or a description that is more complex, in that it includes more social activities, better? And, which of two different interpretations should we prefer if they contain the same level of detail, complexity and the like? Or, again, when have we included all relevant aspects in our thick description, when is it complete? Geertz does not give us answers to such questions, nor are such answers evident. He might be quite right, as Ryle’s example of the twitch and the wink shows, that the researcher who wants to convey meaning should not limit himself to a thin description of a phenomenon. That in itself, however, does not provide us with the necessary criteria to pick the better (thick) interpretation
of what that phenomenon means (compare Jones 1998; Martin 1993; Roseberry 1982; Shankman 1984).

**Reading culture like a text**

The researcher must try to grasp the meaning of actions of the subjects, and to do so, must ‘read over their shoulders’, as Geertz expresses it at one point (1973a, 452). This ‘reading’ is a telling expression: in the last part of *Deep Play*, Geertz explains that analyzing cultural forms should be understood as ‘parallel with penetrating a literary text’ (1973a, 448). Anthropology should not follow the example of the sciences (‘dissecting an organism, diagnosing a symptom, deciphering a code, or ordering a system’, idem). It should not dwell on ‘social mechanics’ but should instead concentrate on ‘social semantics’. The proper example is textual analysis. This means that cultural forms, according to Geertz, should be treated as texts (Geertz 1973a, 449). ‘Doing ethnography’, he maintains in another essay, ‘is like trying to read (…) a manuscript – foreign, faded, full of ellipses, incoherencies, suspicious emendations, and tendentious commentaries, but written not in conventionalized graphs of sound but in transient examples of shaped behavior’ (1973b, 10).

Geertz places his approach in a tradition that also includes the work of Ricoeur, Spinoza, Nietzsche and others: ‘all offer precedents, if not equally recommendable ones’ (1973a, 448n36, 449). Elsewhere, he draws on Dilthey and his notion of the hermeneutic circle to understand texts: ‘a continuous dialectical tacking between the most local of local detail and the most global of global structure in such a way as to bring them into simultaneous view’ (1983, 69). In line with this idea of hopping back and forth between the whole and the parts seems to be his claim that an ethnographer should not try to ‘capture primitive facts in faraway places’ but that he should try ‘to reduce puzzlement’ on the way men live there (1973b, 16).

**Textual analysis as a criterion?**

Geertz notes a clear parallel between social analysis as an understanding of meaning and textual analysis: both are concerned with offering an interpretation of meaning instead of a causal explanation. Yet, what makes a textual analysis an analogy for a better interpretation? Geertz is very brief on this issue. His suggestions include following a particular procedure (hermeneutic circle), a criterion of coherence (following from the manuscript-analogy and the hermeneutic circle), and the notion of reducing puzzlement.

The procedure for reaching an understanding of a text by going back and forth between the parts and the whole, however, does not provide much of a criterion for distinguishing between better and worse interpretations. In *Deep Play*, Geertz does not provide his readers with a report of his process of going back and forth in trying ever new (modified) interpretations. This, at least, leaves the reader without the material to test whether he can accept Geertz’ interpretation as the one that (logically) follows from a convincing process.

The idea of dialectically going back and forth between the parts and the whole to arrive at a good or better interpretation implies a criterion of coherence: the interpretation should combine the different elements in a comprehensible, logical or coherent
way. This indeed seems to be a necessary condition of a good interpretation; without coherence, an interpretation cannot be reasonably understood as a whole. Yet, is coherence in itself enough? Geertz himself explicitly argues that it is not: ‘coherence cannot be the major test of validity for a cultural description’ (1973b, 17). He argues that the story of a swindler or the delusion of a paranoid can also be coherent. It must, therefore, be something else that makes a coherent interpretation a good or better interpretation. His expression of this deciding characteristic is disappointingly vague; however, ‘A good interpretation (...) takes us into the heart of that of which it is an interpretation’ (1973b, 18).

A third possibility suggested in Geertz’ essays is the idea that a good interpretation offers clarification and reduces puzzlement (1973b, 16). He offers it when he asks the very question that this paper focuses on: ‘how can you tell a better account from a worse one?’ (idem). So, what does it mean ‘to reduce puzzlement’? Again, Geertz’ elaboration is disappointing. We must measure the cogency of our explications, he says, ‘against the power of the scientific imagination to bring us into touch with lives of strangers’ (idem). Reducing puzzlement might be the key to distinguishing interpretations, yet Geertz does not provide us with that key, at least not at this point.

In this section, we took a closer look at aspects of interpretative research that all might have their value but cannot be accepted as (sufficient) criteria for distinguishing between better and worse interpretations. On crucial points, moreover, Geertz in his explanatory essays is quite vague. In particular, his elaboration of the claim that a good interpretation means reducing puzzlement is disappointing. A closer look at what Geertz actually does in Deep Play to present his interpretation as good can help clarify his approach.

Comparing interpretations

Deep play’s puzzle

In the introduction of Deep Play, when Geertz tells his story about the police raid, he also informs the reader about the elite’s understanding of cockfights. The Indonesian elite, just like the Dutch colonial government of a previous time, worried ‘about the poor, ignorant peasant gambling all his money away, about what foreigners will think, about the waste of time better devoted to building up the country’ (1973a, 414).

In the central part of the text, Geertz turns to the betting that always accompanies Balinese cockfights (‘an aspect I have thus far studiously ignored’ (1973a, 425). It is in this part that Geertz is most specific: he provides an extensive and detailed presentation. He tells us that he has observed many fights and is able to note particular regularities. Cock owners and important clan members make bets with one another; the spectators that stand around the spectacle also gamble with one another. The participants in the outer circles never bet against the cock of a clan-member. In the center bets (the bets of the cock owners and villagers of a higher ranking), large sums of money are involved. The bets in the outer group are lower. The inner bets, moreover, always have even odds. This is guaranteed by giving the stronger birds a disadvantage in the way the spurs are fixed. The consequence is that the outcome of the center bets is as unpredictable as possible; at the same time, these bets are very large, and losing has substantial financial
consequences for the gamblers (1973a, 425–432). Such betting-behavior is incomprehensible if one considers the betting at cockfights to be simply a game for pastime. In fact, it is the type of gambling that Bentham called ‘deep play’. In technical utilitarian terms, the marginal utility of such games is so low that participation is irrational. These are games in which reasonable people would not engage. Bentham concluded that men (‘the irrationals, the fools, the savages, the children, and so on’) should be protected against themselves and that the government should legally ban such an activity (1973a, 432–3). This, as we saw, is exactly what the Indonesian government and the Dutch colonial Government before it did.

Yet, despite the logical force of a utilitarian argument, the Balinese engage in such deep play. This observation poses ‘a critical analytical problem’ for understanding cockfighting, and gambling more specifically, as reasonable pastimes. From such an interpretation, one must consider the Balinese villagers as simpleminded fools or one is left in puzzlement.

That puzzlement is resolved, however, by the alternative interpretation that Geertz provides: the cockfights and the betting are a dramatization of status concerns. ‘(T)he explanation lies in the fact that in such play, money is less a measure of utility (…) than it is a symbol of moral import’ (1973a, 433). The high-stakes gambling is inevitable if one really wants to bring status and honor into play. What money causes to happen is ‘the migration of the Balinese status hierarchy into the body of the cockfight’. The cockfight is thus a simulation of the social matrix, and the money allows real prestige to become involved in enacting it. Social positions are and at the same time are not at stake in the cockfights. No one’s status really changes through the outcomes of the cockfights, but it is, momentarily, affirmed or insulted. To be sure, it is not to say that money does not matter and that the Balinese do not care about winning or losing it. It is only because it does matter that status really is at stake (1973a, 433, 436).

Geertz continues by pointing out how a whole range of observations (especially on supporting your own kin group or village) fit this alternative interpretation of what cockfighting is all about. He concludes by citing some folktales that also support this theory (1973a, 437–442).

‘Three-cornered fights’

In sum, the power of Geertz’ argument for his interpretation of the practice of Balinese cockfights seems to lie in the way in which he can show that his interpretation is better than the alternative interpretation in light of the observations. His meticulously gathered data on betting during many fights (‘fifty-seven matches for which I have exact and reliable data’ 1973a, 426) do not fit the interpretation that this practice is a more or less utilitarian pastime that provides maximum pleasure at a low cost. It is a puzzling anomaly if one does not want to see the villagers as foolish children. Yet, these observations fit the other interpretation.

He also shows that his alternative interpretation encompasses the other one. The betting on cockfights does not cease to be gambling for money, the loss of which hurts. [As Geertz notes, there are gambling addicts in Bali. The other villagers regard them as fools or as being sick (1973a, 43n28).] Yet, for a complete understanding of what gambling at cockfights is about, we need to see the place it has in the wider world of
Balinese culture (1973a, 429). Further, Geertz can give support to his alternative interpretation in reference to other types of findings, for example, the gambling behavior of the spectators and the way in which status and playing with fire is expressed in Balinese folktales (1973a, 442). 7

The way Geertz, in fact, provides support for his alternative interpretation is remarkably similar to Lakatos’ epistemological ideal of sophisticated falsificationism (1970). Of course, Lakatos focuses on theories as explanatory causal mechanisms, whereas Geertz focuses on interpretations of meanings. Yet, Lakatos also emphasizes the idea that the tests of theories are ‘three-cornered fights’ between rival theories and observations (1970, 115). Theories should not be tested and refuted by comparing them with observations of reality alone. The best theory is the one that (a) can include observations that the other cannot, (b) encompasses the other (i.e. can explain the other’s success) and (c) is confirmed by other observations [in paraphrase of Lakatos (1970, 116)]. Lakatos presented his sophisticated version as a critique to those with an overly simplistic idea of Popper’s falsificationism. Those ‘dogmatic’ or ‘naive’ falsificationists have missed important points. They act from the erroneous presupposition that theories can be tested by confronting them with ‘bare facts’ or ‘raw data’. As Kant has taught, observation is an active process, meaning that ‘data’ rely on observational or background theories. Abandoning a theory, furthermore, because of one anomalous observation is a reckless strategy that very soon leaves one without any theory at all (Lakatos 1970, 97–99, 115, 119). For these reasons, the proper falsificationist approach consists of the comprehensive comparison of theories based on the three points mentioned.

In Deep Play, Geertz practices such an approach. Elsewhere, moreover, he expresses his adherence to a scientific procedure that brings to mind Popper’s notion of ‘conjectures and refutations’ (Popper [1963] 2002. In his essay on thick description, he maintains that ‘(c)ultural analysis is (or should be) guessing at meanings, assessing the guesses, and drawing explanatory conclusions from better guesses, not discovering the Continent of Meaning and mapping out its bodiless landscape’ (1973b, 20). Like Popper, Geertz dismisses a search for ‘the truth’ and opts for an ongoing process of looking for better interpretations.

Geertz might never have gathered the observations on the gambling behavior at cockfights if he had not ‘got in’. His thick description helps give us a comprehensive idea of the actions and frame of mind of Balinese villagers, especially in relation to cockfights. It might very well be the case, furthermore, that Geertz arrived at his interpretation by working and reworking his first hunch in light of what he saw and heard. Yet, the real force of his interpretation seems to lie in the fact that he can show it to be better than the earlier one, in light of his systematic observations.

**Toward better interpretations in the social sciences**

In many handbooks and studies, interpretative approaches are distinguished from other approaches. A concern for finding interpretations of meanings is presented in opposition to being focused on explanatory theories of causal mechanisms. Each of these is often related to particular methodologies (see, e.g. Brower, Abola, and Carr 2000; Raadschelders 2013;
Often, this dichotomy of research traditions is presented in an overview, like the one provided in Table 1.

Geertz seems to offer much support for this dichotomy. As mentioned in the Participating observation and thick description section, he explicitly links interpretative study into the meanings of cultural phenomena to textual analyses and field study. Our closer analysis of his approach in Deep Play, however, told a different story. Interpretations are tested against observation data. The very data that Geertz presents are of a quantitative kind, resulting from his systematic measurements during a large number of games. In the research report that Geertz actually provides in Deep Play, he clearly employs ‘positivist’ elements. This text thus does not support the dichotomy but instead undermines it.

There are more reasons to question this strict dichotomy. As we saw above, in a Popperian theory-oriented approach, ‘facts’ are understood to be always theory laden (i.e. depending on observational or background theories). They are a result of the active involvement of subjects in observation. According to this approach, direct access to reality or raw data is impossible, just as the interpretationists claim. This does not preclude the possibility, however, of testing theories and interpretations in the light of observations. These ‘observation-data’ might be taken as thin descriptions, not radically thin, that is, having no meaning or theory-perspective at all, but thin enough to be used to compare two thickly described interpretations or theories.

Geertz uses data gathered by ‘positivist’ methods (measurement, quantitative data) to compare different interpretations. Similarly, there is no logical barrier to using fieldwork or case studies or qualitative data for testing causal theories (compare e.g. the otherwise quite differently oriented testimonies of Bailey 1992; Flyvbjerg 2006; George and Bennet 2005).

Interpretative studies into cultural meaning, often with reference to Geertz, are supposed to put effort into ‘getting in’, providing thick descriptions and performing a type of textual analysis (see the handbooks and studies mentioned above) and with good reason. To participate in activities and to make particular observations, gaining trust can indeed be an inevitable condition. ‘Being in’, moreover, might be inspiring for coming up with new interpretations. Communicating meanings, furthermore, demands thick description as explaining the meaning of an action or practice cannot be done in numbers or elementary signifiers. Yet, having succeeded in getting in and providing a thick description are not reasons to accept an interpretation. Having followed these guidelines is not enough. It might, in particular cases, not even be necessary for comparing different interpretations in light of observations.

**Table 1. Conventional distinction of research traditions.**

<table>
<thead>
<tr>
<th>Knowledge ambition</th>
<th>Positivist</th>
<th>Interpretive</th>
</tr>
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<tbody>
<tr>
<td>Inspiration</td>
<td>Theory (causal mechanism)</td>
<td>Interpretation (meaning)</td>
</tr>
<tr>
<td>Focus</td>
<td>Sciences</td>
<td>Literary analysis</td>
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<tr>
<td>Design</td>
<td>Reality, facts</td>
<td>Self-understandings, constructions</td>
</tr>
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<td>Methods</td>
<td>Large n</td>
<td>Case studies</td>
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<tr>
<td>Data</td>
<td>Surveys, measuring</td>
<td>Fieldwork, participatory observation</td>
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<tr>
<td>Data</td>
<td>Quantitative</td>
<td>Qualitative</td>
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</tbody>
</table>
This argument does not lead to an advocacy for abandoning interpretative research into the meanings of practices and actions. On the contrary, it shows us a way to provide more convincing interpretations: following here the sophisticated falsificationist epistemology.

In some studies and reflections in the field of Public Administration and Political Science, however, positions are elaborated that have some similarities with falsificationism. Flyvbjerg, for example, approvingly quotes Geertz, saying that ‘The Field’ resists certain interpretations. It is a ‘powerful disciplinary force: assertive, demanding, even coercive’ (Geertz 1995, 225 as cited in; Flyvbjerg 2001, 82. See also; Wagenaar 2011, 22). This ‘resistance of the field’ against certain interpretations, of course, is not the same as an intentional comparison of different interpretation in the light of observations in the field.

White, another advocate of interpretive research, goes one step further. He intends to ‘correct misconceptions about the logic of inquiry in public administration and related fields’ (White 1999, 13–14). He maintains that conjecturing interpretations is logically equivalent to coming up with scientific hypotheses (White 1999, 101, also Ch 7 in passim). In elaborating this claim of equivalence, he posits that ‘all theory is fundamentally a matter of storytelling or narration’ (White 1999, 6). This is a claim that can be upheld only using a very vague and uninformative understanding of storytelling. Yet, amidst his concern for narratives, White notes another similarity that will sound familiar: ‘(c)ontesting interpretations can be examined in light of relevant evidence and conclusions can be drawn about the appropriateness of one interpretation over another’ (White 1999, 134/5).

The idea of testing interpretations in the light of observations is also in another way already present in the interpretive literature. Conducting interpretive research often is presented and recommended as an iterative process, involving testing and revising interpretations (See, for example, Wagenaar 2011, ch 9. Also, the Grounded Theory-approaches, in some of its varieties, can count as an example (see Cuncliffe 2000). This idea differs, however, from the falsificationist one, as found in Geertz’ Deep Play. In the latter, the rival theories are both presented and explicitly evaluated in the light of particular observations. A study that follows the grounded theory-approach or employs a similar iterative procedure does not offer such an explicit presentation of the argumentative logic for concluding that this particular interpretation is the better one. The alternative interpretations and the particular judgments the researchers made in the course of their observation and analysis remain implicit and cannot be retraced in logical argument.

A more elaborate position can be found in Bevir and Rhodes. A critical review of their interpretive study into British Governance Downing had raised methodological doubts that are similar to the issues that were brought forward against Geertz’ approach (see the Introduction section). Bevir and Rhodes reacted by presenting a set of criteria to compare rival interpretations. By following these rules, the ‘intellectually honest’ researcher can avoid charges of relativism or arbitrariness and present his interpretation as the best available. These rules encompass: take facts and criticisms that conflict with an interpretation serious, use agreed facts to compare interpretations and try to present new existing interpretations, not ones that merely block-off criticism of existing interpretations (Bevir and Rhodes 2005, 184). These rules are virtually identical to the rules
of sophisticated falsificationism. In fact, Bevir and Rhodes point to Lakatos as their inspiration (idem: 182). What they do not do, however, is pointing out that their rules come down to falsificationism and that, by consequence, the epistemological ideals of interpretive and causal research are identical. After presenting the rules, Bevir and Rhodes, remarkably, continue by claiming that 'Positivist political scientists might reject such an epistemology as relativist because it gives us no reason to assume the narratives that we select as objective will correspond to truth’ (idem: 185; compare Bevir and Rhodes 2016, 11). Here, the tables are turned in a puzzling way: falsificationism is placed on the side of interpretive approaches by Bevir and Rhodes, while the students of causal mechanisms said to be looking for proof and truth. The latter is hardly a position that testers of causal theories will recognize.

Looking at theoretical reflections on the epistemology of interpretive research, one can conclude that some express a position that comes (very) close to comparative falsificationism.

**Conclusions**

*Deep Play* is an inspiring and exemplary text, although for reasons other than usually presented. In this text, we can see Geertz practicing an interpretative approach that in fact teaches us how to convincingly present a better interpretation of meaning. His approach is in important respects similar to Lakatos’ sophisticated falsificationism. The better interpretation (a) can account for observations that are anomalies from another interpretation’s point of view, (b) can encompass the other interpretation, that is, can explain the (partial) success of the other interpretation and (c) can provide further confirmation for phenomena that have no meaning according to the other interpretation. To perform this comparison, one must rely on systematic observation.

Geertz’ study can be understood as a least likely case to find such a falsificationist approach that is mostly considered to be fit for explanatory theory-focused research: He explicitly claims that investigating meaning in the social sciences demands a way of its own.

The social sciences contain, according to the ready self-interpretation of handbooks and reflective studies, two distinct research approaches. These books and studies provide a dichotomy of approaches, each with its own ontology, epistemology and methods. This paper’s reading of the exemplary interpretive study by Geertz showed that on the level of epistemology, the strict distinction is questionable. A study of meaning can justify its favored interpretations in the same way as a study of explanatory causal theories can. This falsificationist justification, moreover, is not vulnerable to the comments that an inductive justification, like the one Geertz explicitly gives, has to deal with. Designing and reading interpretive studies, like the exemplary one of Geertz, in this way help to overcome the problems of justifying a particular interpretation as the best one that are raised by critics. There is no need for two different epistemologies to the better theory and the better interpretation. Contexts of discovery might of course differ but the types of justification can very well be the same.

Many observers have pointed out that (applied) research in the social domain suffers from a separation, a gap even, between researchers using different methods and approaches (see e.g. Groenevelt 2015; Rhodes 2014). Potential fruitful combinations of methods and insights are often neglected. Insight in causal mechanisms and in
meanings that people have hardly ever are brought together. Readings that split our field in different approaches all the way down might easily block the development of fruitful combinations and inspirations. As long as approaches of others are understood as ‘following a different logic’, the fruitful combinations of methods and the blurring of genres will be difficult to envision.

Notes

1. In the literature, there is some discussion on Geertz’ interpretation of the meaning of Balinese cockfights [see e.g. Martin (1993, 273 ff) and Smith (2011, 25)]. I will not go into that issue here as it is of no consequence for the argument of this paper.

2. This is even more true for the alternative formulations of the meaning of cockfights that Geertz provides, such as ‘a simulation of the social matrix’, ‘a kind of sentimental education’ or ‘the enactment and re-enactment of status hierarchy’ (1973a, 436, 449, 450). These interpretations are understandable only if one is versed in sociology and western literature (see also Jones 1998; Warnke 2011). See on the ambiguity and complications of ‘getting in’ in Geertz’ approach also Marcus (1997).

3. Some argue that interpretive researchers need to go one step further than Geertz does: The researcher should discuss (his) interpretations with the subjects he studied (see e.g. Beuving and De Vries 2014; Flyvbjerg 2001; Wagenaar 2011). This is not the place to discuss dialogical or discursive types of interpretive research. One important issue in such a discussion, however, would also be how different interpretations in the dialogue with subjects are to be evaluated and how might it be determined which interpretation is better. A promising answer to these questions is the one that Geertz actually provides in Deep Play: comparing interpretations in the light of systematic observations (see Comparing interpretations section).

4. I will not go into the issue of whether our total cultural field of ‘webs of meaning’ necessarily is and can be completely coherent.

5. Compare: ‘You either grasp an interpretation or you do not, see the point of it or you do not, accept it or you do not’ (Geertz 1973b, 24).

6. In the last part of Deep Play, Geertz presents his meta-theory of symbolic anthropology. I will not go into this topic here.

7. At one point in the essay Thick Description, Geertz makes a claim that seems to be at odds with what he actually does in Deep Play: “It is not against a body of uninterpreted data, radically thinned descriptions that we must measure the cogency of our explications” (1973b, 16). Geertz does not explain here, however, why we should not do that or why we cannot do that. It is at this point that he claims that “our explications” should be judged “against the power of the scientific imagination to bring us into tough with the lives of strangers” (idem, see also the Participating observation and thick description section).


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