

## Higher-order truths about chess

Daniel C. Dennett

© Springer Science+Business Media B.V. 2006

**Abstract** Many projects in contemporary philosophy are artifactual puzzles of no abiding significance, but it is treacherously easy for graduate students to be lured into devoting their careers to them, so advice is proffered on how to avoid this trap.

**Keywords** a priori truth, chess, graduate students, Hebb

Philosophy is an a priori discipline, like mathematics, or at least it has an a priori methodology at its core, and this fact cuts two ways. On the one hand, it excuses philosophers from spending tedious hours in the lab or the field, and from learning data-gathering techniques, statistical methods, geography, history, foreign languages ..., empirical science, so they have plenty of time for honing their philosophical skills. On the other hand, as is often noted, you can make philosophy out of just about anything, and this is not always a blessing.

Consider, as a paradigm of a priori truths, the truths of chess. It is an empirical fact that people play chess, and there are mountains of other empirical facts about chess, about how people have been playing it for centuries, often use handsomely carved pieces on inlaid boards, and so forth. No knowledge of these empirical

facts plays an indispensable role in the activity of working out the a priori truths of chess, which also exist in abundance. All you need to know are the rules of the game. There are exactly 20 legal opening moves for white (16 pawn moves and four knight moves); a king and lone bishop cannot achieve checkmate, and neither can a king and lone knight, and so forth.<sup>1</sup> Working out these a priori truths about chess is not child's play. Proving just what is and is not possible within the rules of chess is an intricate task, and mistakes can be made that get perpetuated. For instance, a few years ago, a computer chess program discovered a mating net—a guaranteed win—consisting of over 200 moves without a capture. This disproved a long-standing “theorem” of chess and has forced a change in the rules of the game. It used to be that 50 moves without a capture by either side constituted a draw (stalemate), but since this lengthy mating net is unbreakable, and leads to a win, it is unreasonable to maintain the fifty-move stalemate. (Before computers began playing chess, nobody imagined that there *could* be a guaranteed win of anywhere near this length.) All this can be pretty interesting, and many highly intelligent people have devoted their minds to investigating this system of a priori truths of chess.

Some philosophical research projects—or problematics, to speak with the more literary types—are rather like working out the truths of chess. A set of mutually agreed upon rules are presupposed—and seldom discussed—and the implications of those rules

---

This piece grew out of informal discussions with graduate students attending the Brown University Graduate Philosophy Conference on February 16, 2002, and my own graduate students at Tufts. I thank them, and colleagues at Tufts and elsewhere, for valuable reactions and suggestions.

---

D. C. Dennett (✉)  
Center for Cognitive Studies, Tufts University, Medford,  
MA 02155, USA  
e-mail: Daniel.Dennett@tufts.edu

---

<sup>1</sup> A few days after I wrote this, the chess column in the Boston Globe published a special case in which it is, in fact, possible to achieve checkmate with a lone knight. But in general, it is not possible. The special case is shown in the accompanying figure.

are worked out, articulated, debated, refined. So far, so good. Chess is a deep and important human artifact, about which much of value has been written. But some philosophical research projects are more like working out the truths of *chmess*. *Chmess* is just like chess except that the king can move two squares in any direction, not one. I just invented it—though no doubt others have explored it in depth to see if it is worth playing. Probably it isn't. It probably has other names. I didn't bother investigating these questions because although they have true answers, they just aren't worth my time and energy to discover. Or so I think. There are just as many a priori truths of *chmess* as there are of chess (an infinity), and they are just as hard to discover. And that means that if people actually did get involved in investigating the truths of *chmess*, they would make mistakes, which would need to be corrected, and this opens up a whole new field of a priori investigation, the *higher-order* truths of *chmess*, such as the following:

1. Jones' (1989) proof that  $p$  is a truth of *chmess* is flawed: he overlooks the following possibility ...
2. Smith's (2002) claim that Jones' (1989) proof is flawed presupposes the truth of Brown's lemma (1975), which has recently been challenged by Garfinkle (2002) ...

Now none of this is child's play. In fact, one might be able to demonstrate considerable brilliance in the group activity of working out the higher-order truths of *chmess*. Here is where Donald Hebb's dictum comes in handy:

If it isn't worth doing, it isn't worth doing well.

Each of us can readily think of an ongoing controversy in philosophy whose participants would be out of work if Hebb's dictum were ruthlessly applied, but we no doubt disagree on just which cottage industries should be shut down. Probably there is no investigation in our capacious discipline that is not believed by some school of thought to be wasted effort, brilliance squandered on taking in each other's laundry. Voting would not yield results worth heeding, and dictatorship would be even worse, so let a thousand flowers bloom, I say. But just remember: if you let a thousand flowers bloom, count on 995 of them to wilt. The alert I want to offer you is just this: try to avoid committing your precious formative years to a research agenda with a short shelf life. Philosophical fads quickly go extinct and there may be some truth to the rule of thumb: the hotter the topic, the sooner it will burn out.

One good test to make sure you're not just exploring the higher-order truths of *chmess* is to see if people aside from philosophers actually play the game. Can anybody outside of academic philosophy be made to *care* whether you're right about whether Jones' counterexample works against Smith's principle? Another such test is to try to teach the stuff to uninitiated undergraduates. If they don't "get it," you really should consider the hypothesis that you're following a self-supporting community of experts into an artifactual trap.

Here is one way the trap works. Philosophy is to some extent an unnatural act, and the more intelligent you are, the more qualms and reservations you are likely to have about whether you get it, whether you're "doing it right," whether you have any talent for this discipline and even on whether the discipline is worth entering in the first place. So bright student Jones is *appropriately* insecure about going into philosophy. Intrigued by Professor Brown's discussion, Jones takes a stab at it, writing a paper on hot topic  $H$  that is given an "A" by Professor Brown. "You've got real talent, Jones," says Brown, and Jones has just discovered something that might make suitable life work. Jones begins to invest in learning the rules of this particular game, and playing it ferociously with the other young aspirants. "Hey, we're good at this!" they say, egging each other on. Doubts about the enabling assumptions of the enterprise tend to be muffled or squelched "for the sake of argument." Publications follow.

So don't count on the validation of your fellow graduate students *or* your favorite professors to settle the issue. They all have a vested interest in keeping the enterprise going. It's what they know how to do; it's what they are good at. This is a problem in other fields too, of course, and it can be even harder to break out of. Experimentalists who master a technique and equip an expensive lab for pursuing it often get stuck filling in the blanks of data matrices that nobody cares about any longer. What are they supposed to do? Throw away all that expensive apparatus? It can be a nasty problem. It is actually easier and cheaper for philosophers to re-tool. After all, our "training" is not, in general, high-tech. It's mainly a matter of learning our way around in various literatures, learning the moves that have been tried and tested. And here the trap to avoid is simply this: you see that somebody eminent has asserted something untenable or dubious in print; Professor Goofmaker's clever but flawed piece is a sitting duck, just the right target for an eye-catching debut publication. Go for it. You weigh in, along with a dozen others, and now you must watch your step, because by the time you've all cited each other and

responded to the responses, you're a budding expert on How to Deal with Responses to Goofmaker's minor overstatement. (And remember, too, that if Goofmaker hadn't made his thesis a little too bold, he never would have attracted all the attention in the first place; the temptation to be provocative is not restricted to graduate students on the lookout for a splashy entrance into the field.)

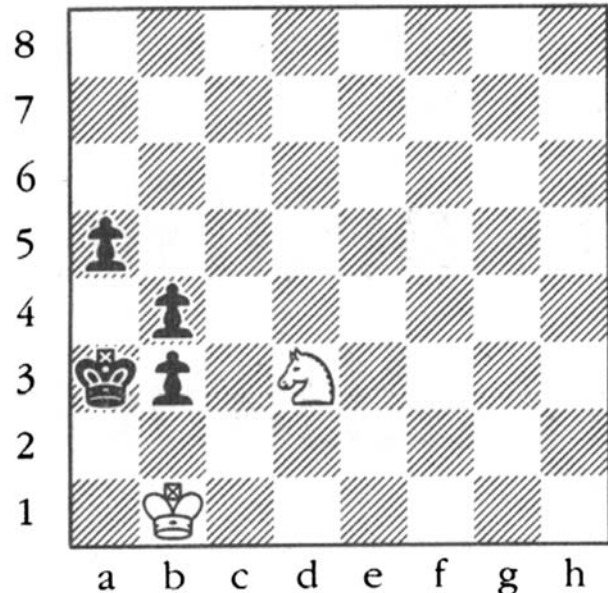
Of course some people are quite content to find a congenial group of smart people with whom to share "the fun of discovery, the pleasures of cooperation, and the satisfaction of reaching agreement," as John Austin once put it (see Austin 1961, p. 175), without worrying about whether the joint task is worth doing. And if enough people do it, it eventually becomes a phenomenon in its own right, worth studying. As Burton Dreben used to say to the graduate students at Harvard, "Philosophy is garbage, but the history of garbage is scholarship." Some garbage is more important than other garbage, however, and it's hard to decide which of it is worthy of scholarship. In another lecture published in the same book, Austin gave us the following snide masterpiece:

It is not unusual for an audience at a lecture to include some who prefer things to be important, and to them now, in case there are any such present, there is owed a peroration. ("Ifs and cans," pp. 230–31)

Austin was a brilliant philosopher, but most of the very promising philosophers who orbited around him, no doubt chuckling at this remark, have vanished without a trace, their oh-so-clever work in ordinary-language philosophy duly published and then utterly and deservedly ignored within a few years of publication. It has happened many times.

So what should you do? The tests I have mentioned—seeing if folks outside philosophy, or bright undergraduates, can be made to care—are only warning signs, not definitive. Certainly there have been, and will be, forbiddingly abstruse and difficult topics of philosophical investigation well worth pursuing, in spite of the fact that the uninitiated remain unimpressed. I certainly don't want to discourage explorations that defy the ambient presumptions about

## Chess SHELBY LYMAN



### WHITE TO PLAY

Hint: Checkmate in 2 moves.

**Solution:** 1. Nb2 a4 (the only legal move) 2. Nc4 mate (from a study by Gurevich).

what is interesting and important. On the contrary, the best bold strokes in the field will almost always be met by stony incredulity or ridicule at first, and these should not deter you. My point is just that you should not settle complacently into a seat on the bandwagon just because you have found some brilliant fellow travelers who find your work on the issue as unignorable as you find theirs. You may all be taking each other for a ride.

### Reference

Austin JL (1961) "A plea for excuses," in his *Philosophical papers*. Oxford University Press, Oxford, pp. 175–204.