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Adequate Formalization and De Morgan's Argument

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Summary

Lampert and Baumgartner (2010) critically discuss accounts of adequate formalization focusing on my analysis in (Brun 2004). There, I investigated three types of criteria of adequacy (matching truth conditions, corresponding syntactical surface and systematicity) and argued that they ultimately call for a procedure of formalization. Although Lampert and Baumgartner have a point about matching truth conditions, their arguments target a truncated version of my account. They ignore all aspects of systematicity which make their counter-example unconvincing.

1. Introduction

Logical formalization assigns formulas to ordinary language sentences or inferences, aiming at a transparent representation of a logical form suitable for use in rigorous validity proofs. In (Brun 2004), I discussed the conceptual underpinnings of this project and reconstructed criteria of adequacy guiding accepted practice of formalization. I argued that these criteria all raise problems and ultimately call for a procedure of formalization. Lampert and Baumgartner (2010; henceforth referred to by unqualified page numbers) argue that the criteria I discussed rely on incoherent distinctions between formalization and semantical analysis, and between validity in virtue of logical form and in virtue of semantical relations among non-logical expressions ("material" or "analytical" validity). Specifically, they attack my case-study of an inference traditionally attributed to De Morgan. Here is the argument with four candidates (C1)–(C4) for formalizing the conclusion (numbered (9)–(12) on p. 93–4):

(P) Every horse is an animal.
(C) Every head of a horse is a head of an animal.

(P1)	$\forall x(Hx \rightarrow Jx)$	Fx: x is a head of a horse
(C1)	$\forall x(Fx \rightarrow Gx)$	Gx x is a head of an animal
(C2)	$\forall x(\exists y(Hy \wedge Ixy) \rightarrow \exists y(Jy \wedge Ixy))$	Hx: x is a horse
(C3)	$\forall x \forall y(Hy \wedge Ixy \rightarrow Jy \wedge Ixy)$	Ixy: x is a head of y
(C4)	$\forall x(Hx \wedge \exists y Iyx \rightarrow Jx \wedge \exists y Iyx)$	Jx: x is an animal

This reply analyses the controversy over these formalizations without elaborating on deeper running differences between Lampert and Baumgartner's and my approach to theories of formalization. Sections 2 and 3 briefly sketch how my criteria are applied to (C2) and (C3), concentrating on aspects Lampert and Baumgartner ignored and on points of disagreement. Section 4 investigates their key example (C4) and section 5 their main line of argument.

2. Truth conditions and surface rules

This paper focuses on formalizations in classical zero- and first-order logic: Given a sentence S in an ordinary language **A,** a formalization Φ of S in a logic **L** is an ordered pair $\langle \phi, \kappa \rangle$ with a formula ϕ of **L** and a correspondence scheme $\kappa = \{\langle \alpha_1, a_1 \rangle, \ldots, \langle \alpha_n, a_n \rangle\}$ that specifies one-to-one for each non-logical symbol α_i ($1 \leq i \leq n$) in ϕ an expression a_i of **A** (augmented by auxiliary expressions for representing argument-places of predicates).[1]

In (Brun 2004:pt. III), I reconstructed three types of adequacy criteria guiding accepted practice of formalizing. Firstly, formalizations must be correct in the sense that they do not allow validity proofs for inferences which are definitely invalid by informal standards ("*i*-valid" and "*f*-valid" will abbreviate "valid according to informal standards" and "valid according to the standards of a logical formalism"). In the context of semantical formalisms, this requirement (called "TC") can be framed in terms of truth conditions as follows: A formalization $\langle\phi, \kappa\rangle$ of a sentence *S* in **L** is correct iff for every condition *c,* for every **L-**interpretation $\langle\mathcal{D}, \mathcal{I}\rangle$ corresponding to *c* and κ, $\mathcal{I}(\phi)$ matches the truth value of *S* in *c.* An **L-**interpretation corresponding to a condition *c* and a correspondence scheme $\{\langle\alpha_1, a_1\rangle, \ldots, \langle\alpha_n, a_n\rangle\}$ is an **L-**structure $\langle\mathcal{D}, \mathcal{I}\rangle$ with a domain $\mathcal{D}$ and an interpretation-function $\mathcal{I}$ such that $\mathcal{I}(\alpha_i)$ matches the semantic value of a_i ($1 \leq i \leq n$) in c. A second type of criteria requires some syntactical correspondence of sentences and formulas. Two examples are: the logical symbols in a formalization Φ must have a counterpart in *S;* Φ's correspondence scheme must not include ordinary language expressions not occurring in *S.* Thirdly, formalizing should be systematic. Ideally, adequate formalizations should be producible by a procedure of formalization.

With respect to the first two types of criteria, I basically agree with Lampert and Baumgartner that the standard formalization (C2) and its rival (C3) fare equally well (p. 93–5). I will therefore not discuss the problems raised by these criteria nor comment on Lampert and Baumgartner's exposition, although the latter contains some misunderstandings. I only mention why (TC) and surface rules do not decide which of the two non-equivalent formalizations (C2) and (C3) is adequate. (TC) is not distinctive enough if materially *i*-valid inferences are involved. Deciding between (C2) and (C3) would require us to settle by informal reasoning whether (C) was true even if not all horses were animals but some heads of non-animal-horses were also heads of an animal. However, we must reject the idea of taking that decision because it informally makes no sense to assess truth values with reference to semantically impossible conditions. Surface rules threaten to classify a great deal of standard formalizations as inadequate and hence cannot be taken as strict requirements but must be interpreted liberally. Either way, they fail to decide between (C2) and (C3).

The third approach to adequacy focuses on the requirement that formalizing be systematic rather than arbitrary and ad hoc (Brun 2004:chs 12.4, 13.6). This is ignored in Lampert and Baumgartner's paper (except for (HSC), cf. below), but it plays a crucial role in addressing their criticisms.[2] Section 3 briefly outlines the missing elements.

3. Formalizing systematically

A strategy of formalizing analogous sentences analogously is well entrenched in philosophical practice. Philosophers generally aim at finding ways of formalizing not individual sentences but sentences of a certain type. Relying on a principle of "parity of form" (Russell 1905:483), undisputedly adequate formalizations are used as models for less clear cases. It is, for example, standard practice to formalize "All … are …"-sentences as instances of $\forall x(\phi x \rightarrow \psi x)$ even if the

[1] For the sake of simplicity, I assume that we formalize sentences. Inferences – i.e. sequences of sentences $\langle$premise$_1$, …, premise$_n$, conclusion$\rangle$ – are formalized as sequences of formulas using one common correspondence scheme. (Cf. Brun 2008.)

[2] Those omissions may be the result of their narrower notion of criteria: "criteria are no criteria if they are open for discussion" (Baumgartner/Lampert 2008:113).

extension of the subject-term is empty. The problem is that this presupposes an account of parity of form.

Standard practice also uses a technique of formalizing step-by-step. While formalizing analogously is systematic in linking adequate formalizations of different sentences, the step-by-step method is systematic in relating different formalizations of the same sentence. In example (C), we may start with (C1), formalize "*x* is a head of a horse" and "*x* is a head of an animal" as $\exists y(Hy \wedge Ixy)$ and $\exists y(Jy \wedge Ixy)$, substitute the results for Fx and Gx in (C1), and (C2) results. Two problems are that the vague instruction of formalizing step-by-step produces inadequate results, and that it calls for explicitly formalizing not only sentences but also their parts and the way they make up the original sentence. There is, however, sound motivation for the step-by-step strategy in arguments of compositionality, which combine the two senses of "systematicity" mentioned. Such arguments call for theories capable of accounting for the productivity of ordinary languages, dealing not only with the specific examples that guided theory development but with an unlimited number of sentences and inferences.

The common theme behind surface rules and the principles of analogous and step-by-step formalization is that they all become more convincing the more we can spell out in a precise and general manner how sentences are to be formalized based on some syntactic description. If this analysis of principles guiding standard practice is correct, it reveals an ideal not typically thought to be present: formalizations should be the product of an effective procedure, an algorithm which accounts for the systematic, compositional, nature of ordinary language. Without such a procedure, the criteria discussed remain problematic and fail to provide a sufficient condition for adequate formalization (cf. Brun 2004:chs 12.4, 14).

I agree with Lampert and Baumgartner that syntactic descriptions of naive grammar are not a suitable basis for specifying satisfactory criteria and formalization procedures (the "misleading form thesis", p. 80, 88). But this is neither sufficient reason for ignoring considerations of systematicity nor does it follow that rigorous procedures of formalization are impossible (p. 80), only that they must rely on a far more sophisticated analysis of ordinary language. This in turn calls for empirical investigations of language structures in relation to logical formalisms as pioneered by, for example, Montague and Davidson. This research and other contributions drawing on Chomsky's theory of language also suggest that formalisms other than first-order logic, such as generalized quantifier theory, may be more promising. That we still lack comprehensive formalization procedures does not justify dismissing this research as hopeless.

Furthermore, the argument that criteria of adequacy are conceptually prior to formalization procedures is not as conclusive as Lampert and Baumgartner assume (p. 80–1). Of course, not just any procedure of formalizing is acceptable, but only one with results we are ready to accept as adequate. But if (TC) and surface rules do not discriminate between alternative formalizations, such as (C2) and (C3), I fail too see why we should not appeal to considerations of systematicity embodied in a procedure of formalization as additional criteria of adequacy. And if we admit that informal judgements about truth conditions come in degrees of firmness, we have reason to welcome that systematicity sometimes overwrites assessments of (TC)-correctness.

Even without satisfactory formalization procedures, we can argue about the adequacy of formalizations by pointing out that they could (not) plausibly be the product of a systematic procedure. Specifically, I suggested a criterion of hierarchical structure (called "HSC"; Brun 2004:ch. 13; cf. p. 90), which incorporates core aspects of the step-by-step method and compositionality: at least one of two non-equivalent formalizations of the same sentence must be inadequate if neither is more specific than the other and there is not a third adequate formalization more specific than both. A formalization $\langle\phi, \kappa\rangle$ is more specific than a formalization $\langle\psi, \kappa\rangle$ iff ϕ can be generated from ψ by substitutions $[\alpha/\beta]$ such that either (i) α is a sentence-letter occurring in ψ and β is a formula containing at least one sentential connective or a predicate-letter, or (ii) α is an n-place predicate-letter occurring in ψ and β is an open formula with n free variables containing at least one sentential connective, quantifier or predicate-letter with more than n places. (HSC) specifies a necessary, negative condition of adequacy, which can only be applied if we have two rival formalizations, but does not determine which one is inadequate. The following discussion illustrates its use.

4. A new formalization of De Morgan's argument?

According to (HSC), (C3) is not an adequate formalization of (C) if (C1) is adequate (cf. Brun 2004:ch. 13.6). (C1) and (C3) are not equivalent and no substitution for predicate-letters will turn (C1) into (C3), (C3) into (C1) or both into the same formula. In specifications of (C3), two $\forall$s range over the rest of the formula, but in specifications of (C1) only one $\forall$ does. (C2), on the other hand, is more specific than (C1). It results from applying $[Fx/\exists y(Hy \wedge Ixy), Gx/\exists y(Jy \wedge Ixy)]$ to (C1). Hence its adequacy is compatible with the adequacy of (C1). Formalizing step-by-step, we can develop (C2) but not (C3) from (C1).

Lampert and Baumgartner admit that within my tenets this argumentation in favour of (C2) is correct (p. 94–5). However, they attack my discussion of De Morgan's example by introducing (C4), which is equivalent to (C3) and results from (C1) by substituting $[Fx/Hx \wedge \exists yIyx, Gx/Jx \wedge \exists yIyx]$. Their further argumentation depends on the claim that (C4) is an adequate formalization of (C) according to my account. I agree that (C4) and (C2) fare equally well with respect to (TC) and surface rules (for reasons as given for (C2) and (C3) in sect. 2). I also agree that (HSC) rules out that (C2) and (C4) are both adequate without telling us which one is inadequate. However, Lampert and Baumgartner's diagram is incorrect (p. 95; cf. Brun 2004: 353). It places (C3) and (C4) on the same specification path, but (C3) is unrelated to (C1) and (C4). Lumping (C3) together with (C4) also leads to the false claim that "[C2] and [C4]/[C3] perform equally well on all counts" (p. 96). If this were true, the inadequacy of (C3) would imply that (C2) and (C4) are inadequate as well.

(C4) is inadequate for other reasons. As a colloquial verbalization, we read "Every horse with a head is an animal with a head" (p. 105). This is a different statement than (C). Apparently, the substitution used to get (C4) from (C1) turns heads of horses into horses with heads. Consider the *i*-invalid inference from (C) to (D):

(D) Every head of a horse is a horse.

Using the same correspondence scheme as above, (D) may be formalized as (D1) in analogy to (C1) and if we apply to (D1) the substitution that generated (C4), we get the inadequate (D2):

(D1) $\forall x(Fx \rightarrow Hx)$
(D2) $\forall x(Hx \wedge \exists yIyx \rightarrow Hx)$

If (D2) were adequate, it could be used to prove the *i*-invalid inference from (C) to (D). Furthermore, (D2) violates (TC) since it is a logical truth, whereas informally (D) is false. To block this result while defending (C4), Lampert and Baumgartner must either claim that (D1) is inadequate or explain why specifying Fx by $Hx \wedge \exists yIyx$ should turn an adequate formalization into an inadequate one if applied to (D1) but not if applied to (C1). Both reactions are implausible for reasons of systematicity. (C1) and (D1) perfectly illustrate the strategy of analogous formalizations. And there is no convincing reason which blocks applying the same specification to Fx in both (C1) and (D1). It seems implausible that a general procedure of formalizing could account for compositionality, yet treat (C) and (D) as fundamentally different. Advocating (C4) as adequate rather is a prototypic example of an ad hoc formalization. Against this, Lampert and Baumgartner might argue thus: the fact that (C4) is (TC)-correct, while (D2) is not, suffices to show that a systematic procedure of formalizing must treat (C) and (D) differently. This is not a viable objection to the claim that (C4) is inadequate according to my account because it incorrectly presupposes that considerations of systematicity cannot discriminate between (TC)-correct formalizations.

Finally, we must check whether the arguments against (C4) cannot be levelled against (C2) as well. Applying to (D1) the substitution that generates (C2) from (C1) yields:

(D3) $\forall x(\exists y(Hy \wedge Ixy) \rightarrow Hx)$

In contrast to (D2), (C2) and (D3) do not sanction the *i*-invalid inference from (C) to (D). (D3) can also be defended as (TC)-correct. First, we assume that there is at least one head of a horse. Then (D) is informally false since no head of a horse is a horse, and all corresponding interpretations $\langle \mathcal{D}, \mathfrak{I} \rangle$ must meet two conditions: (i) there are $e_1, e_2 \in \mathcal{D}$ such that $\langle e_1, e_2 \rangle \in \mathfrak{I}(I)$ and $e_2 \in \mathfrak{I}(H)$; (ii) there are no $\xi, \zeta \in \mathcal{D}$ such that $\langle \xi, \zeta \rangle \in \mathfrak{I}(I)$ and $\xi \in \mathfrak{I}(H)$. In such interpretations, $\mathfrak{I}(\exists y(Hy \wedge Ie_1y) \rightarrow He_1)$ is false and hence $\mathfrak{I}(\forall x(\exists y(Hy \wedge Ixy) \rightarrow Hx))$ is false and (D3) has the same truth value as (D). Second, we assume that no heads of horses exist. Then in all corresponding interpretations there are no objects $\xi, \zeta \in \mathcal{D}$ such that $\langle \xi, \zeta \rangle \in \mathfrak{I}(I)$ and $\zeta \in \mathfrak{I}(H)$. Hence, $\mathfrak{I}(H\zeta \wedge I\xi\zeta)$ is false for all ξ and ζ, $\mathfrak{I}(\exists y(Hy \wedge I\xi y))$ is false for all ξ, and $\mathfrak{I}(\forall x(\exists y(Hy \wedge Ixy) \rightarrow Hx))$ is true. This means that (D3) is (TC)-correct if we are ready to informally treat (D) as vacuously true if no heads of horses exist. This is just another instance of the standard practice which treats "All … are …"-sentences as instances of $\forall x(\phi x \rightarrow \psi x)$ and therefore as vacuously true if the subject term's extension is empty. Arguing against this practice would be of no help in defending (D2) or (C4) since they rely on the same assumption about formalizing "All … are …"-sentences.

I conclude that the claim "No tenet of Brun's theory provides any reason to give preference to [C2] or [C3]/[C4]" (p. 96) is unfounded.

5. Lampert and Baumgartner's main argument and "ways out"

We are now ready to analyse Lampert and Baumgartner's main line of argument and their five "conceivable ways out". Three of them will not be discussed extensively. I agree that giving up (HSC) (p. 99–100) as well as identifying logical form with a sequence of (non-)logical expressions in ordinary language (p. 100–2) do not permit a plausible account of formalization (Brun 2004:ch. 13.5.2, p. 267). And their (2008) "Tractarian"-programme calls for a discussion which is outside the scope of this paper. It suggests an alternative to the traditional practice of formalization and does not aim at evaluating arguments but at exhibiting their semantic structure based on the assumption that informal reasoning is immune to revision in light of logical theory.

The fourth "way out" (p. 104–5) proposes to ignore all semantical dependencies between expressions occurring in the sentence *S* when applying (TC). This changes the nature of (TC) considerably. If we pretend that the expressions in *S* do not have the meaning they have, we no longer rely on an informal judgement about whether the given sentence S would be true under certain conditions. It is not clear to me what exactly we are being asked to do if we are to assess the truth conditions of (C) under the presumption that its predicates are semantically independent.[3] One interpretation is that ignoring all semantical dependencies between semantically dependent expressions amounts to ignoring their meaning. This boils down to applying (TC) to ordinary-language schemes instead of sentences; that is, to "Every A of a B is an A of a C" instead of (C). However, this cannot be what Lampert and Baumgartner have in

[3] Judgements of semantic independence seem to be tricky. Why should, e.g., "*x* has four single-toed hooves" be any more semantically independent of "*x* is an animal" than "*x* is a horse" (cf. p. 106)?

mind since it is incompatible with the misleading form thesis they subscribe to and once more identifies logical forms with sequences of (non-)logical expressions. An alternative interpretation is that we are being asked to treat (C) as if it had the same meaning as some other sentence with semantically independent predicates, say "Every son of a botanist is a son of a communist". Since (TC) can be applied to this sentence without problems, we appeal to analogy and conclude that (C2) rather than (C3) is a correct formalization of (C). This reading is presumably also inacceptable to Lampert and Baumgartner who discount considerations of analogy. But it fits well into my account since it underlines that (TC) needs to be supplemented with criteria relating to the requirement of formalizing systematically.

My starting point for analysing Lampert and Baumgartner's third "way out" (p. 102–3) and main line of argument is what I take to be the standard definition of formal validity in first-order logic (cf. Brun 2004:ch. 1.2–4; p. 343–4):

(L) An inference *I* is formally *i*-valid relative to first-order logic iff *I* has at least one adequate formalization in first-order logic that is *f*-valid.

Two points are crucial. First, (L) defines a technical term, "formally *i*-valid relative to first-order logic" (for short, "first-order *i*-valid"), designed to single out a subclass of *i*-valid inferences; those which are *i*-valid in virtue of their first-order logical form. (L) does not report but establishes a distinction between first-order *i*-valid inferences and inferences which are *i*-valid for some other reason, for example, in virtue of semantic dependencies between non-logical expressions. Second, (L) calls for a theory of adequate formalization. As I have argued, the criteria of adequacy discussed – all three types – are problematic and do not constitute a sufficient, but only some necessary conditions of adequacy. Consequently, we cannot expect them to yield, together with (L), a sufficient condition for first-order *i*-validity.

Lampert and Baumgartner (p. 89, 92) use two conditionals in place of (L):

(IFVP) If there exists at least one correct and surface faithful *f*-valid first-order formalization of an argument *S*, *S* is *formally i*-valid relative to first-order logic.

(VP) If an argument *S* is formally *i*-valid relative to first-order logic, then there exists at least one adequate *f*-valid first-order formalization of *S.*

The two conditionals do not easily recombine into a biconditional and we no longer have a definition of first-order *i*-validity. Rather, we are presented with two postulates stipulating different conditions for formalizations to be first-order *i*-valid. According to (IFVP), it suffices if there is a formalization which is correct and satisfies surface rules (and is *f*-valid in first-order logic), whereas "adequate" in (VP) requires compliance with additional criteria, such as (HSC).[4]

However, we should not, and I did not, adopt (IFVP) and (VP) instead of (L) because (IFVP) excludes important aspects of adequacy. Via (IFVP), even clearly inadequate formalizations – violating (HSC) or other considerations of systematicity – establish an inference's first-order *i*-validity. (VP) then requires that a theory of formalization provide an adequate formalization for such an inference. (L), on the other hand, excludes such a situation since it defines first-order *i*-validity in terms of *adequate* formalization. It is therefore not surprising that (IFVP) and (VP) allow for counterexamples not affecting (L). (C4) is a case in point. As I argued above, it is not an adequate formalization of (C) and thus it cannot, according to (L), establish the *i*-validity of any inference involving (C). Hence, if we adhere to (L) instead of (IFVP) and (VP), Lampert and Baumgartner's main line of argument fails (because we cannot accept their (P1) on p. 98).

[4] Further complications arise because the two existence-claims in (IFVP) and (VP) are independently stated yet related by an implicit reference to other formalizations in (VP) if adequacy includes (HSC).

Lampert and Baumgartner present two objections to replacing (IFVP) with a stronger criterion. The first is peculiar to their proposal of resorting to a principle (labelled "$IFVP_{adq}$") which employs not the notion of adequacy I developed but a watered-down version that adds just (HSC) to the antecedent of (IFVP) excluding all other considerations of systematicity. On this basis, they claim that (C2) and (C4) are equally adequate, yet cannot both be adequate as they violate (HSC). This brings us back to the discussion of (C4) in section 4, where my criteria of adequacy deemed (C4) inadequate and hence did better than providing "no indication whatsoever" (p. 103) about which of the two formalizations, (C2) or (C4), is adequate. By presupposing that their ($IFVP_{adq}$) incorporates my adequacy criteria (p. 102), Lampert and Baumgartner's critique misses its target. Their objection does not apply to (L) if "adequate" is used as I have explained it.

Secondly, Lampert and Baumgartner raise an "epistemic" problem relevant to any account which includes (HSC):[5] Given an adequate formalization Φ of an inference *I*, how can we ensure there is no rival formalization equally adequate in all respects except that the two formalizations violate (HSC)? If we cannot guarantee that no such formalization exists, we cannot guarantee Φ's adequacy and hence not conclusively establish *I*'s first-order *i*-validity via (L). The answer depends on what approach to formalizing one adopts, specifically on the means of determining formalizations and assessing their adequacy. If we adhere to the traditional practice, we must rely on experience and trial and error to find formalizations, which must then pass criteria of adequacy. This makes it impossible to guarantee that nobody will find new ways of formalizing which will confront us with two otherwise adequate formalizations violating (HSC). Should that happen, we may come to the conclusion that the two formalizations represent two readings of an ambiguous sentence. If this cannot be made plausible, (HSC) forces us to revise other criteria of adequate formalization. The situation is different should we rely on a procedure of formalizing that incorporates our criteria of adequacy. A formalization will then be adequate only if it is the result of applying the procedure (irrespective of whether the procedure was in fact applied). Since any acceptable procedure of formalizing needs to respect (HSC), there is simply no danger of encountering rival formalizations violating (HSC), unless we are dealing with an ambiguous sentence. Consequently, the fact that the traditional practice of formalizing has problems conclusively establishing adequacy provides reason to promote the development of procedures of formalization. As argued in section 3, they are needed in any case if the traditional practice is to be placed on solid ground.

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References

Baumgartner, Michael; Timm Lampert. 2008. "Adequate Formalization". *Synthese* 164, 93–115.

Brun, Georg. 2004. *Die richtige Formel. Philosophische Probleme der logischen Formalisierung* (2nd ed.). Frankfurt a. M.: Ontos.

Brun, Georg. 2008. "Formalization and the Objects of Logic". *Erkenntnis* 69, 1–30.

Lampert, Timm; Michael Baumgartner. 2010. "The Problem of Validity Proofs". *Grazer Philosophische Studien* 80, 79–109.

Russell, Bertrand. 1905. "On Denoting". *Mind* 14, 479–93.

[5] It also affects their proposal of stipulating semantical independence, since that does not make (HSC) redundant.