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On the *BCI*-Admissibility of an 'Abelian' Rule

0. Preamble. In late 2004, the last time I was thinking about *BCI* – alias the implicational fragment of (classical or multiplicative) linear logic – I was concerned with formulas of the form

$$A \to (B \to B).$$

Specifically I was wondering whether for every *BCI*-provable formula A there is a B for which the inset formula was provable. If you want to read about this issue, which I did not succeed in settling, you can do so (soon) in the forthcoming paper [Hu2]. (As its title suggests, the issue does indeed have a connection with Curry's Paradox, but not a connection I'd care to explain here.) Now I'm going to adjust the bracketing, and consider instead formulas of the form

$$(A \to B) \to B.$$

The question I'm interested in is whether it's the case that whenever such a formula is *BCI*-provable, so is the formula A. In other words, I am asking whether the rule:

$$\frac{(A \to B) \to B}{A}$$

is admissible for *BCI.*

Notes: (1) This is a "rule form" of the Abelian axiom of Meyer and Slaney's Abelian Logic (or the Axiom of Relativity, as they call it in [MS]).

(2) The rule is obviously not a derivable rule of *BCI* (considered as axiomatized with axioms *B*, *C*, *I* and *Modus Ponens*) – since it is easily seen not to be admissible in numerous extensions of *BCI* (e.g., *BCK* and *BCIW*). The difficulties I had over settling the admissibility status of this rule for *BCI* account for the informal title (see above) under which this talk was given.

(3) In [Hu1] I described A as *anticipating* B in any logic with at least → amongst its connectives, when A → B provably implied B in the logic. ([Hu1] then investigates an extension of intuitionistic propositional logic in a language with an additional 1-ary connective ***a*** with, for any formula B, the formula ***a***B as the strongest formula anticipating A.) The admissibility of the rule above for *BCI* amounts to what in that terminology would be put by saying that in *BCI* the only formulas that anticipate anything are the provable formulas. Why is this question of interest? Well, the general question of whether *BCI* is structurally complete has been raised in [RO]; see further

Endnote 1 below. Structural completeness (of a proof-system) here means that every admissible substitution-invariant rule is derivable, so investigating the admissibility of (A → B) → B / A is investigating a possible counterexample to that property. For a further connection with some of the published literature, let us note that [Av2] shows that whenever A is a *contracting* formula in *BCI* in the sense that for all formulas C, A → (A → C) provably implies A → C, then A is provable. (Actually Avron is working with more extensive fragments of linear logic but this is a consequence for the implicational fragment.) If our rule is indeed admissible then we can say something much stronger: if for *even one* formula C, A → (A → C) provably implies A → C, then A is provable (taking the 'B' of the our formulation of the rule as A → C). [[Incidentally, Example 1.13 in [Av2] is mistaken, as is pointed out in [Hu2], but this does not affect the result just cited.]]

1. Sequent Calculus. Girard's proof of cut elimination for his sequent calculus linear logic offers the prospect of a simple proof-theoretic argument for the admissibility of the above rule, which we can now think of as the claim that whenever a sequent ⊢ (A → B) → B is provable, so is the sequent ⊢ A. (I don't personally favour the use of "⊢" as a sequent separator, in view of its use to stand for consequence relations, but to reduce novelty, I'll employ it here. I am also assuming that the formulas to the left of this separator – and we can use the intuitionistic format here, eschewing anything but a single formula to its right – are conceived as collected into multisets.) The idea would be that we ask how a cut-free proof of ⊢ (A → B) → B might have been obtained. Since this is not an initial sequent (i.e., of the form A ⊢ A) and it has no formulas on the left, it must have arisen by (→ Right) from

$$A \to B \vdash B.$$

But where how could this sequent – again not an initial sequent – have been proved? Only by an appeal to (→ Left), in which the premiss sequents must have been ⊢ A and B ⊢ B, so ⊢ A is indeed provable, as we wanted to show.

That argument is fallacious. Recall that A and B are any formulas for which A → B ⊢ B is provable, so in particular B itself could be of the form C → D. Thus the claim that this sequent could have been derived "only by an appeal to (→ Left)" is quite wrong. It could have been obtained by (→ Right), with a premiss – writing out B more explicitly:

$$A \to (C \to D), C \vdash D.$$

We would be making the same mistake again if we argued by induction – say assuming A → B ⊢ B was a provable sequent with unprovable A than which no sequent had a shorter (cut-free) proof of the form in question – that that the sequent just displayed must have come by (→ Left) inserting the main → of the formula A → (C → D) thus

$$\frac{-\vdash A \qquad\qquad -, C \to D \vdash D}{A \to (C \to D), C \vdash D}$$

where C occupies precisely the position indicated by one of the dashes as a side formula. From that point on one would say if it is the left premiss sequent's dash that is replaced by this side formula then the right premiss has the form $C \to D \vdash D$ and has a shorter proof than the original $A \to B \vdash B$ so by the inductive hypothesis $\vdash C$ must be provable, and thus, the left premiss being now $C \vdash A$, we have $\vdash A$ provable (since cut is admissible): a contradiction. Thus it is the right premiss sequent's dash that marks the spot occupied by C in which case the left premiss is $\vdash A$ to start with: again a contradiction.

While "from that point on" this pleasant argument is available, we have, as I said, made the earlier mistake again in treating C and D as atomic. But these too can be again be implicational formulas, derived by (→ Left) or (→ Right) respectively, and we have lost control over the possible proof history of our sequent. However, the initial argument does indeed establish one very special case of what we were after: the provability of $A \to B \vdash B$ *for atomic* B does indeed imply the provability of $\vdash A$. We should also record the fact that the rule we are considering is *vacuously* admissible for the case of atomic A, since with A atomic (which here amounts to: A's being a propositional variable or 'sentence letter') the formula $(A \to B) \to B$ is never *BCI*-provable as it would have an odd number of occurrences of some propositional variable, which is (well known to be) impossible.

With regard to the rule in its general form, one might well think that a still more general form should be considered for an inductive argument on the length (or 'height' of proofs; for a step I tried to take in this direction, see Endnote 3 below.

2. De-Prefixing. Apart from the failed attempt at tracing back the possible proofs of $A \to B \vdash B$, one might consider another inductive strategy, now modulating back to the axiomatic setting, given that we do at least know that when $(A \to B) \to B$ is provable for atomic B, so is A. The idea would be to represent B in the form

$$B_1 \to (B_2 \to \ldots \to (B_n \to p_i)\ldots)$$

with p_i a propositional variable, and argue by induction on n, the basis case of $n = 0$ being what we have established. The inductive step would then appeal to the *Rule of De-Prefixing*:

$$\frac{(C \to D) \to (C \to E)}{D \to E}$$

We should need an argument to the effect that this rule is admissible for *BCI* in order to employ it in the following manner, but let us describe the employment and return to the credentials of the rule after that. The inductive hypothesis now tells us that the provability of

$$[A \to (B_1 \to (B_2 \to \ldots \to (B_n \to p_i)\ldots))] \to (B_1 \to (B_2 \to \ldots \to (B_n \to p_i)\ldots))$$

suffices for the provability of A and we are now considering the provability of

$$[A \to (B_1 \to (B_2 \to \ldots \to (B_n \to (B_{n+1} \to p_i))\ldots))] \to$$
$$(B_1 \to (B_2 \to \ldots \to (B_n \to (B_{n+1} \to p_i))\ldots)).$$

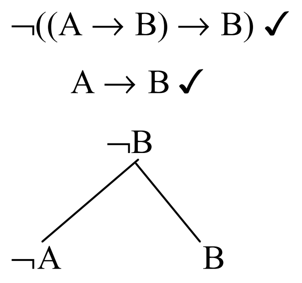
If this is provable then we may permute the innermost occurrences of B_{n+1} in the antecedent and the consequent out to the front and then erase them by appeal to De-Prefixing (B_{n+1} now playing the role indicate by C in the schematic formulation of that rule), which gives us the provability of what the inductive hypothesis says suffices for the provability of A, and we are done.

We now turn to the status of the De-Prefixing rule. Unfortunately for the argument we have just presented, it turns out that this rule is not *BCI*-admissible. (A counter-example is included in Endnote 2.) Thus the argument doesn't work. It would have been nice to reverse things and show that the $(A \to B) \to B$ / A rule is not admissible because if it were then so would De-Prefixing be: but I wasn't been able to find a way of showing this. (A side point: the special case of De-Prefixing with D and E atomic is indeed admissible. If D = E then the conclusion is provable and if D $\neq$ E then the premiss cannot be, by parity considerations: D is a variable occurring an odd number of times in the premiss, as also is E. Admissibility here means admissibility for *BCI*, of course; the 'atomic consequent' form of the rule is not *BCIW*-admissible, in view of the well known $\mathbf{R}_{\to}$-theorem $((p \to q) \to p) \to ((p \to q) \to q)$).)

3. Semantics. The simplest way to show the admissibility of an underivable rule is via a semantic argument. One needs a semantics w.r.t. the logic is sound and complete to do this. For example, consider the obvious "adjunction of a new point" argument to show the admissibility for the smallest normal modal logic, **K**, of the rule of De-Necessitation ($\Box$A / A). I know of no sufficiently tractable semantic account of *BCI* which has been able to assist with this (e.g., the Urquhart style semantics in terms of commutative monoids). In [Av1] Avron was able to show the unprovability of various sequents in *BCI* and larger fragments of linear logic (and more interestingly various "joint unprovability" results such as the non-existence of any A for which both $\vdash$ A and also $\vdash$ A, A were provable: note that here we are working with multiple succedent sequents), using the Meyer–Slaney integer matrix – he calls this 'concrete semantics' as opposed to 'abstract semantics' – for Abelian logic (and some generalizations of this matrix). But we can't show the admissibility of a rule such as the one we are considering without a semantics w.r.t. which the logic is not only sound (as in Avron's discussion) but also complete. (One can convert the observation just credited to Avron into an admissibility claim for a rule, namely the sequent-to-sequent rule with schematically indicated premisses $\vdash$ A and $\vdash$ A, A and conclusion $\Gamma \vdash \Delta$, since we know not every sequent is provable. This is analogous to the vacuously admissible rule for the modal logic **K**: ◊A / B, for which again we need only the soundness and not also the completeness of **K** w.r.t. the class of all frames.)

4. Trees. In the case of classical propositional logic, the tree or tableau style of proof system is obviously very closely connected to the intended truth-table semantics. In the 1970s several papers by Michael McRobbie and others were published giving such systems for relevant logics and related systems, which in the pure implicational case have a simple adaptation to *BCI.* The ('decomposing') rules for → are just that on A → B one branches to a branch headed ¬A and a branch headed B, and proceeds, while for ¬(A → B) one proceeds without branching to A and then below it ¬B. You may wonder what ¬ is doing sticking its nose into a proof system for a purely implicational logic. The answer is that it is really functioning only to "negatively sign" the formula it follows, and not as an embeddable connective in its own right. A branch closes when a propositional variable and its 'negation' appear on that branch, and a tree closes when every branch closes and every formula in the tree has been used exactly once. Here a formula is said to be *used* if a decomposing rule has been applied to it or it is one of a pair of formulas in virtue of which a branch is closed. A proof of the formula A is a closed tree constructed with ¬A as starting formula.

Here is a hand-waving argument using the tree method for the admissibility of the rule (A → B) → B / A. Suppose the tree on ¬((A → B) → B) closes. This tree begins thus, the ticks indicating that the formula ticked has been used in the development as displayed:



Our supposition is that this tree eventually closes, and the right branch (or the subtree it starts) will close eventually in virtue of the fact that it has both B and ¬B on it. (Eventually, not immediately, since we are requiring that if B is not atomic, it and its negation be further decomposed; I am not completely sure of the significance of this requirement.) But that means the left branch will also close, and since all formulas above ¬A on that branch have already been used (and each formula must be used exactly once), a tree started from scratch on ¬A will also close: thus A is provable if (A → B) → B is.

But I don't trust this argument. We could give a similar argument to show the admissibility of De-Prefixing. Suppose the tree on $\neg((C \rightarrow D) \rightarrow (C \rightarrow E))$ closes. I must begin like this:

$$\neg((C \rightarrow D) \rightarrow (C \rightarrow E)) \checkmark$$
$$C \rightarrow D \checkmark$$
$$\neg(C \rightarrow E) \checkmark$$
$$C$$
$$\neg E$$
$$\neg C \qquad\qquad\qquad D$$

Now the left branch already has C and $\neg$C on it so it must close, using them both; thus the right branch must close using $\neg$E and D and no external resources. But that means the tree on $\neg(D \rightarrow E)$ would close, since the vertical ordering ($\neg$E before D or D before $\neg$E) is not significant. Thus the provability of $(C \rightarrow D) \rightarrow (C \rightarrow E)$ implies the provability of $D \rightarrow E$. We have already seen that this is false, so the general method of argument involved here must be fallacious. (Like the sequent calculus argument, it ignores the internal complexity of formulas represented by single schematic letters, as well as considerations about the order of application of the tree construction rules.)

5. Natural Deduction. A different approach (kind of proof-system, I mean) seems more promising, though I still don't regard the following considerations as decisive. I'm thinking of the natural deduction approach, especially as in the Lemmon style system for *BCI* (to be found, for instance, in [Hu2]). The assumption dependency column records a multiset rather than a set of assumptions. Suppose we can prove $\vdash (A \rightarrow B) \rightarrow B$. What would the proof look like? Well, one thing we know is that if we are wondering whether we can prove this without also being able to prove $\vdash$ A, then A and B are themselves implicational formulas, so I'll now write than as $A \rightarrow B$ and $C \rightarrow D$. The second last line of the proof (prior to an application of $(\rightarrow I)$, or (CP) as Lemmon calls it) must have the formula D sitting on it, and it must depend on the assumptions $(A \rightarrow B) \rightarrow (C \rightarrow D)$ and C. So the proof begins:

1	(1) $(A \rightarrow B) \rightarrow (C \rightarrow D)$	Ass.
2	(2) C	Ass.

and it must work its way down to D with depending on 1 and 2. That means 1 and 2 both have to appear on the left, so both have to be used in deriving D, and what's more, each has to be used exactly once in this (or else we'd have dependency record 1,1,2 or 1,2,2 etc., considered as multisets: we want just 1,2). To use C we'd need to get the consequent of (1) and apply (→E), but we can't use C also to obtain A → B or we would have used C twice, so A → B must already be provable outright, i.e., derivable from no (undischarged) assumptions.

This intendedly general description obviously omits some possibilities. Maybe it was C that could be proved outright, for instance, rather than A → B. Still, for the envisaged application of (→I), we do need to have C as an assumption, and as one on which D depends before that application. I don't know what to say about the possibility that (1) is used not as a major premiss but as a minor premiss for an application of (→E), and don't feel at all confident that even the several cases mentioned here exhaust the possibilities for a natural deduction proof – though I do feel *somewhat* more confident that they do bring out the problem of deriving C → D from (A → B) → (C → D) with A → B being unprovable.

6. Monothetic *BCI*. An interesting extension of *BCI*, considered in the 1980s (independently) by J. Kabzinski and M. Bunder – detailed references may be found in [Hu2] – is what we call monothetic *BCI*, in which any two theorems are provably equivalent. (A question left open in [Hu2] was whether this logic could be axiomatized as *BCI**, where *I** is (A → A) → (B → B), and *Modus Ponens* is the sole rule – or *Modus Ponens* and Uniform Substitution if one prefers to work with three individual axioms instead of three axiom-schemata. [BR] offered, in an earlier paper, an algebraic proof that this is indeed the case, which I had not seen when writing the main body of [Hu2] and of which I was somewhat suspicious in any case in view of another mistake in [RvA1] – see note 30 of [Hu2], as well as [RvA2]. A purely syntactic proof of this fact has since been obtained in [KB], however. [This summary may be an oversimplification. The trouble is that Raftery and van Alten are really considering various different consequence relations, whereas (here, at least) I am not.])

While we don't in *BCI* have the contraction principle, there are cases in which we have both a formula A → (A → B) and also A → B provable. As remarked in the preamble, if we had the implication from the former to the latter provable in *BCI* then this would be a counterexample to the admissibility of the rule we are considering, as long as A was not itself provable. Now in *BCI**, any case in which A → (A → B) and also A → B are provable automatically gives a case in which the 'contracting' implication

$$(A \to (A \to B)) \to (A \to B)$$

is provable. So to show that our rule is not admissible for *BCI** we simply have to produce formulas A → (A → B) and A → B with A unprovable. We can very easily find such pairs of formulas already in *BCI*, for example using the fact (mentioned in [Hu2]) that the converse of any *BCI*-theorem is a Mingle formula (a "Mingler" as it is put in [Hu2]), i.e., is a formula C for which C → (C → C) is provable. Since C → C is automatically provable, we have our provable A $\rightarrow$ (A $\rightarrow$ B) and A $\rightarrow$ B for the above argument, providing C is not provable. The shortest such candidate C, i.e., unprovable converse of a provable formula is the 'Abelian' formula (p $\rightarrow$ q**.** $\rightarrow$ q) $\rightarrow$ p, in which I've used a dot to reduce bracketing, and which below will be further abbreviated to "(pq.q)p". (Note that this formula is not *BCI**-provable.) Thus in *BCI**, we have the following counterexample to the admissibility of our (A $\rightarrow$ B) $\rightarrow$ B / A rule:

$$(\underline{(pq.q)p} \rightarrow ((pq.q)p \rightarrow (pq.q)p)) \rightarrow ((pq.q)p \rightarrow (pq.q)p) \,/\, \underline{(pq.q)p}$$

in which the occurrences of the formula playing the 'A' role are underscored.

Actually we can simplify the example a bit in the sense of reducing the length of the premiss formula, by using another fact mentioned in [Hu2], namely that in *BCI* the converse of any *BCI*-theorem provably implies a self-implication, which in the case of our (pq.q)p can be taken as the formula p $\rightarrow$ p. Since (pq.q)p $\rightarrow$ (p $\rightarrow$ p) and also its consequent are provable in *BCI*, in *BCI** the implication from former to the latter is provable, giving another counterexample to the admissibility of the rule, shorter than that given above and also with the premiss not having the 'contracting implication' form:

$$((pq.q)p \rightarrow (p \rightarrow p)) \rightarrow (p \rightarrow p) \,/\, (pq.q)p.$$

For the record, there is also a counterexample intermediate in complexity between these two, which, like the first though not the second, has a contraction premiss, namely:

$$((pq.q)p \rightarrow ((pq.q)p \rightarrow (p \rightarrow p)) \rightarrow ((pq.q)p \rightarrow (p \rightarrow p)) \,/\, (pq.q)p.$$

7. Last Comment. Returning from *BCI** to *BCI* itself, but continuing with the above abbreviated notation, I tried for quite a while to find a *BCI*-theorem of the form

$$((pq.q)p \rightarrow B) \rightarrow B$$

which would refute the admissibility claim for our rule in *BCI*, but was unable to find a suitable B. Why is this choice of the Abelian axiom a likely candidate for the 'A' role in the current rule? Well, one thing in its favour is that although not *BCI*-provable, it has an even number of occurrences of each of its propositional variables, without which the inset formula would have no chance of being provable (since the rest of the formula has all variables occurring evenly many times thanks to the two occurrences of B), and further, it is one of the simplest candidates meeting both these requirements. Finally, we are familiar with the fact that many rules, when added to *BCI* or other implicational logics, yield the 'theorem' form of the rule – as a provable premiss-to-conclusion implication, that is, and our rule is the rule form of the Abelian axiom. So this would be the most straightforward way that such a 'theorematizing' effect might manifest itself in the present instance. (E.g., the contraction rule A $\rightarrow$ (A $\rightarrow$ B) / A $\rightarrow$ B theorematizes in the setting of *BCI*: take A = p, B = (p $\rightarrow$ (p $\rightarrow$ q)) $\rightarrow$ q and permute antecedents, after applying the rule. Another example is given in Endnote 2.)

Endnotes

Endnote 1. Raftery and Olson [RO] say that, contrary to a conjecture by Slaney and Meyer (in [SM]), that the $\{\wedge, \rightarrow, t\}$-fragment of **R** is not structurally complete with, the example of the following rule:

$$A \rightarrow t, (A \rightarrow t) \rightarrow t \mathbin{/} A$$

which according to [RO] is admissible but not derivable. As well as asking about structural completeness for (implicational) *BCI*, they ask the same question about *BCIW* (= $\mathbf{R}_{\rightarrow}$), though not in the case of *BCK*; perhaps that has been settled in the literature. ([SM] gave an argument for structural completeness in the case of the $\{\wedge, \rightarrow\}$-fragment of **R**.)

Endnote 2. For a counterexample to the *BCI*-admissibility of De-Prefixing begin with the BCI-provable $((p \rightarrow q) \rightarrow (p \rightarrow r)) \rightarrow ((p \rightarrow q) \rightarrow (p \rightarrow r))$ and then permute:

$$(p \rightarrow q) \rightarrow [((p \rightarrow q) \rightarrow (p \rightarrow r)) \rightarrow (p \rightarrow r)]$$

and permute again (internally):

$$(p \rightarrow q) \rightarrow [p \rightarrow (((p \rightarrow q) \rightarrow (p \rightarrow r)) \rightarrow r)].$$

Now De-Prefixing would yield

$$q \rightarrow (((p \rightarrow q) \rightarrow (p \rightarrow r)) \rightarrow r),$$

to see the upshot of which, permute the q back inside:

$$((p \rightarrow q) \rightarrow (p \rightarrow r)) \rightarrow (q \rightarrow r),$$

i.e., the converse of *B* (from *B*, *C*, *I*), which is not *BCI*-provable. Note that this shows, as it was put in Section 7, that the De-Prefixing rule theorematizes (over *BCI*).

There is a bit more that can be extracted from this derivation. Let us substitute q for r in the last formula inset above, giving

$$((p \rightarrow q) \rightarrow (p \rightarrow q)) \rightarrow (q \rightarrow q),$$

a formula whose antecedent is provably implied, in BCI by $p \rightarrow p$. Thus I^* is provable in the extension of *BCI* by the De-Prefixing rule. Using this, we can in the system, replace the consequent $q \rightarrow q$ of the above inset formula by $p \rightarrow p$. Here we write the result of this replacement but with some permutation in the antecedent:

$$[p \rightarrow ((p \rightarrow q) \rightarrow q)] \rightarrow (p \rightarrow p).$$

Finally, De-Prefixing the ‘p’-antecedents, we obtain the Abelian axiom. Since De-Prefixing is obviously derivable in the Abelian extension of *BCI* (which is monothetic), the converse of every theorem – and thus of *B*, the ‘theorem’ form of De-Prefixing, being provable, we conclude that the result of adding the De-Prefixing rule to *BCI* is precisely the Abelian extension of *BCI*.

Endnote 3. The considerations for a proof-theoretic argument along the lines canvassed in Section 1 above might be refined by introducing the general idea of a *ponential* sequent

$$A_1 \to (A_2 \to \ldots \to (A_n \to B), X_1,\ldots,X_k \vdash Y_1 \to \ldots \to (Y_m \to C))$$

where the X_i and Y_j all appears amongst $A_1,\ldots,A_n$. We call a formula a *lost antecedent* if it is one of the A's not appearing amongst the X's and Y's. (We actually need to keep track of occurrences here, so in the sequent $p \to (q \to (p \to (r \to (s \to t)), p, q \vdash r \to t$ not only s but also p counts as a lost antecedent, since we have two occurrences of p as explicit antecedents in the 'major premiss' (the first listed formula above) but only one appearing either as a left-hand formula by itself or as an implicational antecedent on the right. The idea is then to prove, perhaps by induction on the number of applications of (→ Left) in a cut-free sequent calculus proof, that the lost antecedents of any *BCI*-provable ponential sequents are themselves *BCI*-provable outright. This would give the special case of interest $A \to B \vdash B$ provable implies A (as a lost antecedent) provable, but the more general form would lend itself – I hoped – to the inductive strategy just suggested. But the cases still ramified out of control, and I could not see how to complete the argument because of the number of ways the side formulas were distributed across the two premisses of an application of (→ Left).

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