

# Herbrand Award Acceptance Speech \*

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## Abstract.

This is a slightly enhanced version of the acceptance speech given by the author after receiving the Herbrand Award at the 19th International Conference on Automated Deduction (CADE-19) in Miami, Florida, on August 1, 2003. Historical matters related to Herbrand's Theorem, higher-order logic, and the author's work are discussed. Contributions by others which have been helpful to the author are noted.

**Keywords:** Herbrand's Theorem, higher-order logic, TPS

First of all I'd like to express my deep appreciation for all the wonderful work that's been done by those who worked as research assistants on the TPS Theorem Proving System over the years. Here they are, in chronological order:

Eve Longini Cohen	1974 - 1980
Dale A. Miller	1978 - 1983
Frank Pfenning	1980 - 1986
Sunil Issar	1984 - 1990
Carl Klapper	1984 - 1987
Dan Nesmith	1987 - 1991
Hongwei Xi	1992 - 1995
Matthew Bishop	1992 - 1999
Chad E. Brown	1999 -

Some of them never met each other, but they were essentially able to work effectively as a team in developing TPS over a period of almost thirty years. This was greatly facilitated by the fine organization for our file system which Frank Pfenning developed when we were converting everything to Common Lisp.

Sometimes different people have different ideas about the best ways to do various things. We often resolved such situations by implementing both ideas, and having flags in TPS which allowed us to experiment with the alternatives.

One of the great advantages of having bright research assistants is that your ambition is not inhibited by concerns about the difficulties of the project you have in mind. I no longer remember who implemented proofwindows in TPS, but I do remember that when I realized that

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\* This work was supported by NSF grant CCR-0097179.



it would be very nice to have a proofwindow to display and update a proof as one worked on constructing it interactively, I had no idea how such a thing could be done. Nevertheless, the research assistant helping me at the time soon came up with a way of doing it.

The research assistants working on TPS did far more than implement ideas which I proposed. They also developed and implemented their own ideas, which have contributed enormously to TPS. Dale Miller, Frank Pfenning, Sunil Issar, and Matthew Bishop all wrote theses [17, 35, 38, 43] and associated papers [16, 34, 39, 40, 42, 44] whose ideas have been implemented in TPS. Chad Brown is currently working to finish up his thesis, which will also give TPS a great step forward. You may recall that Chad presented a paper [19] at the CADE in Copenhagen last summer. The contributions of the research assistants to TPS are also reflected in a variety of joint papers [6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 18, 41].

I thought it might be appropriate at this time to discuss some historical matters, particularly matters related to my work, to higher-order logic, and to Herbrand's Theorem. In the process I will have opportunities to make it clear how contributions by others have been very helpful to me.

When I was an undergraduate at Dartmouth I was concerned about how various problems in the world might be solved, or at least alleviated, and I became aware of how complicated many of these problems are. Attempts to solve them can have unforeseen side-effects, and create new problems. We need very sophisticated methods of thinking about complex problems. Our technology and scientific knowledge progress steadily, but are we any better at thinking than Socrates or Pythagoras?

At about this time I took my first logic course, which was taught by John Kemeny. Kemeny was a remarkable person. While a student at Princeton he was Einstein's assistant. At Dartmouth he was Chairman of the Mathematics Department and later President of the college. He developed the programming language Basic, and was very active teaching and writing. When he was a graduate student at Princeton, he undertook to prove rigorously that Zermelo Set Theory was equivalent to Type Theory in logical strength. This was generally believed at the time. He found it difficult to do this, and in frustration he exclaimed to his advisor, Alonzo Church, "I don't even think it's true." To this Church replied, "All right, prove that", and Kemeny soon had a proof that the consistency of Type Theory could be proved in Zermelo Set Theory, from which it follows by Gödel's Second Theorem that Zermelo Set Theory must be stronger than Type Theory. Kemeny was a wonderful teacher, and I was enthralled by the discovery that one can

actually study the mysterious process of reasoning in a mathematically rigorous way.

I went to Princeton for graduate work, where Alonzo Church was also my advisor. Of all the things I have to thank Church for, I think the most important one was inventing the system of simple type theory which he introduced in 1940 [20].

Of course, Bertrand Russell had developed type theory [49, 60], and I'm very grateful for this. However, Russell was concerned about avoiding semantic paradoxes such as Grelling's paradox<sup>1</sup> as well as more mathematical paradoxes such as Russell's paradox, so in *Principia Mathematica* he used ramified type theory, with Axioms of Reducibility to alleviate some of the ramification. The result of this and other features of Russell's formulation of type theory made it seem quite complicated, so in spite of the enormous influence of *Principia Mathematica* as a landmark in the development of logic, it was the book that everyone talked about, but practically no one read.

Church's type theory is much simpler, and is at the same time a richer, more expressive language, since it recognizes functions as first-class objects which do not have to be regarded as sets of ordered  $n$ -tuples, and it has  $\lambda$ -notation for functions and sets. It permits one to express mathematical ideas in ways that are very close to traditional mathematical notation. Nevertheless, the feeling that type theory is complicated persists, and many people who are otherwise quite logically sophisticated shy away from it. A simple introduction to type theory can be found in the last three chapters of my book [5].

I was in graduate school in the early 60's, when pioneering work in automated theorem proving was being done, but the resolution method hadn't yet been invented. As I came to appreciate what a marvelously expressive language Church's type theory is, I realized that what interested me most was the development of a sufficiently deep understanding of how to prove theorems of this system that one could, in principle, automate the process. I knew that one of the fundamental theorems underlying proof procedures and decision procedures for first-order logic was Herbrand's Theorem.

As we seek to understand how to prove theorems efficiently, one of the things we can ask is "*Why* is a particular wff a theorem?" A theorem is not simply a wff which happens to have a proof. It has special structural properties which guarantee that it is true in all interpretations.

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<sup>1</sup> An *autological* adjective applies to itself. For example, the word "polysyllabic" is polysyllabic, so it is an autological word. A *heterological* adjective is one which does not apply to itself. For example, the word "long" is not long, so it is a heterological word. Grelling's paradox concerns the question whether the word "heterological" is heterological.

The essential idea underlying Herbrand's Theorem is to focus on this structural aspect of theorems.

Herbrand's Theorem plays a fundamental role in the pioneering papers of Quine [47], Gilmore [27], Prawitz [45], Davis and Putnam [22], and Davis [21]. In his paper [48] introducing the resolution method, Robinson referred to his Resolution Theorem as a form of Herbrand's Theorem.

Herbrand was by all accounts a brilliant mathematician. He died in a mountaineering accident in the Alps at the age of 23. This was a tragic loss to logic and mathematics. Herbrand's proof of his theorem was in his thesis [30].

While I don't want to get into too many technical details, let's review the most important results in this thesis.

Herbrand introduced a system of first-order logic which we shall call  $\mathcal{H}$ . It can be described as follows:

- (1) All quantifier-free tautologies are axioms of  $\mathcal{H}$ .

The rules of inference of  $\mathcal{H}$  are the following:

- (2) Rules of Passage [31, pp. 74, 225] for pulling out or pushing in quantifiers, as when transforming to prenex normal form or miniscope form.
- (3) Universal Generalization.
- (4) Existential Generalization.
- (5) Simplification: From  $[\mathbf{P} \vee \mathbf{P}]$  infer  $\mathbf{P}$ .
- (6) Modus Ponens: From  $\mathbf{P}$  and  $[\mathbf{P} \supset \mathbf{Q}]$ , infer  $\mathbf{Q}$ .

The rule of Alphabetic Change of Bound Variables is also an implicit rule of inference of  $\mathcal{H}$ .

It is easy to see that  $\mathcal{H}$  is equivalent to a traditional Hilbert-style system of first-order logic.

We shall use  $\mathcal{G}$  as a name for the system obtained from  $\mathcal{H}$  by deleting Modus Ponens from the list of rules of inference and replacing the Simplification rule by:

- (5') Generalized Simplification: Replace  $[\mathbf{P} \vee \mathbf{P}]$  in a theorem by  $\mathbf{P}$ .

Thus,  $\mathcal{G}$  can be described as follows:

- (1) All quantifier-free tautologies are axioms of  $\mathcal{G}$ .

The rules of inference of  $\mathcal{G}$  are the following:

- (2) Rules of Passage for pulling out or pushing in quantifiers, as when transforming to prenex normal form or miniscope form.
- (3) Universal Generalization.
- (4) Existential Generalization.
- (5') Generalized Simplification: Replace  $[\mathbf{P} \vee \mathbf{P}]$  in a theorem by  $\mathbf{P}$ .

The rule of Alphabetic Change of Bound Variables is also an implicit rule of inference of  $\mathcal{G}$ .

Note that  $\mathcal{G}$  is a cut-free system, but unlike a Gentzen-style system, it has no rule of Conjunction Introduction.

Herbrand showed how to associate with a wff  $\mathbf{P}$  of first-order logic certain quantifier-free wffs which we shall call *Herbrand expansions* of  $\mathbf{P}$ . Let us say that a wff  $\mathbf{P}$  has the *Herbrand property* iff some Herbrand expansion of  $\mathbf{P}$  is tautologous. Actually, for technical reasons the Herbrand property appears in three forms in Herbrand's thesis: Property A, Property B, and Property C. As part of the proof it is established that they are equivalent to each other.

Herbrand's proof involved establishing the following claims about any wff  $\mathbf{P}$  of first-order logic:

- (1) If  $\mathbf{P}$  has the Herbrand property, then  $\vdash_{\mathcal{G}} \mathbf{P}$ .
- (2) If  $\vdash_{\mathcal{G}} \mathbf{P}$ , then  $\vdash_{\mathcal{H}} \mathbf{P}$ .
- (3) If  $\vdash_{\mathcal{H}} \mathbf{P}$ , then  $\mathbf{P}$  has the Herbrand property.

The first claim follows from an analysis of the relation between a tautologous Herbrand expansion of a wff and the wff itself. The second claim is a trivial consequence of the fact that the rules of inference of  $\mathcal{G}$  are all primitive or derived rules of inference of  $\mathcal{H}$ . The third claim is the most difficult to establish; the proof involves showing that each rule of inference of  $\mathcal{H}$  preserves the Herbrand property.

Thus, Herbrand asserted that the following are equivalent:

- (a)  $\mathbf{P}$  has the Herbrand property.
- (b)  $\vdash_{\mathcal{G}} \mathbf{P}$
- (c)  $\vdash_{\mathcal{H}} \mathbf{P}$

Note that this implies that Modus Ponens is a derived rule of inference of the system  $\mathcal{G}$ , a result that is closely related to Gentzen's Cut-Elimination Theorem [25].

It seemed natural to try to extend Herbrand's Theorem to Church's type theory. There are elegant proofs of Herbrand's Theorem using semantical concepts. For example, there is one in section 35 of my book [5]; this proof is inspired by Quine's 1955 paper [47]. However, the semantics of type theory involve certain matters which do not arise for the semantics of first-order logic, so I thought I should see if a purely syntactic proof of Herbrand's Theorem could be extended to higher-order logic. This meant working with Herbrand's original proof.

I started work developing a proof of a Herbrand Theorem for type theory, using Herbrand's proof of his theorem for first-order logic as a guide. However, I kept running into difficulties, and reformulating my approach. Finally, I decided that I didn't understand Herbrand's proof well enough, so I looked at it more carefully. As van Heijenoort remarks in his anthology, "Herbrand's thesis bears the marks of hasty writing. ... Herbrand's thoughts are not nebulous, but they are so hurriedly expressed that many a passage is ambiguous or obscure." [59, p. 525].

I kept trying to make sense of Herbrand's proof, but finally I told Professor Church that there seemed to be a gap in the proof.

I should remark that the problem in Herbrand's proof was with Lemma 3.3 of Chapter 5. For each positive integer  $p$ , Herbrand defined what it meant for a wff to have Property C of order  $p$ . One can describe this at least roughly by saying that a wff has Property C of order  $p$  iff the Herbrand expansion of the wff using all terms from the Herbrand universe with depth of nesting less than  $p$  is a tautology. The lemma asserted that for each positive integer  $p$ , the Rules of Passage preserve Property C of order  $p$ .

Professor Church advised me to consult Burton Dreben at Harvard, who was the greatest authority on Herbrand's work, at least in the United States, and whose work involved applications of Herbrand's Theorem to solvable cases of the decision problem. So I wrote to Dreben. For the sake of historical clarity, some of our correspondence is shown in the figures below.<sup>2</sup>

In my letter of April 9 (Figures 1 and 2), I described the problem with the proof, and provided an example where a certain part of Herbrand's argument did not work.

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<sup>2</sup> Dreben's letters to the author are published here with the kind consent of his widow, Juliet Floyd.

Walnut 1-9534  
 116 Linden Lane  
 Princeton, New Jersey  
 1962 April 9

Professor Burton S. Dreben  
 Philosophy Department  
 Harvard University  
 Cambridge 38, Massachusetts

Dear Professor Dreben,

My thesis advisor, Professor Church, has suggested that I write to you about what I believe is a mistake in Herbrand's thesis, *Recherches sur la Théorie de la Démonstration*. I have spent a considerable amount of time and effort trying to justify Herbrand's argument or to find an alternative finitary argument proving the same result, and have not thus far succeeded. I therefore would be most grateful if you would tell me whether you believe my objections to Herbrand's argument are valid, and if so, whether you see any way the proof may be patched up. If you do not regard my objections as valid, I hope you will point out to me what I have overlooked.

Let us consider Chapitre 5, 3.3, pp. 101-104, the proof that if a proposition has the property  $C$  of order  $p$ , then a proposition obtained from that one by replacing a well formed part  $((\exists x)\phi x) \vee p$ , where the individual variable  $x$  does not occur in  $p$ , by  $(\exists x)(\phi x \vee p)$ , also has the property  $C$  of the same order. I am concerned about the third paragraph on p. 103:

"Remplaçons pour cela la réduite de  $P_1$  par une autre, obtenue comme suit:  $a_1, a_2, \dots, a_n$  étant les éléments des champs  $C_1^{(2)}, \dots, C_p^{(2)}$  on définira dans ces champs  $f_y(x_1, \dots, x_n)$  comme étant  $f_y(x_1, \dots, x_n, a_i)$  pour un  $i$  que nous choisirons tout à l'heure et qui dépendra de  $x_1, \dots, x_n$ . Ceci conduirait à prendre une partie de ces champs comme champs  $C_1^{(1)}$ ."

When one looks at the manner in which  $a_i$  is chosen for a given choice of values for  $x_1, \dots, x_n$ , as described on page 104, one sees no reason to believe that if  $x_1, \dots, x_n$  are all in  $C_1^{(1)} \cup C_2^{(2)} \cup \dots \cup C_r^{(1)}$ , then  $a_i$  is in  $C_1^{(2)} \cup \dots \cup C_r^{(2)}$ . That is, from the fact that  $f_y(x_1, \dots, x_n)$  is in  $C_1^{(1)} \cup \dots \cup C_s^{(1)}$  one (apparently) cannot infer that  $f_y(x_1, \dots, x_n, a_i)$  is in  $C_1^{(2)} \cup \dots \cup C_s^{(2)}$ . (You will recognize that I am following Herbrand's convention of letting the same symbol stand for an element of  $C_{il}^{(1)}$  and for the corresponding member of  $C_j^{(2)}$ . I trust it will be evident that the mild

Figure 1. First page of 1962 April 9 letter from Andrews to Dreben.

ambiguities inherent in this convention are in no way involved in my argument.) In particular, it might happen that  $f_Y(x_1, \dots, x_n)$  is in  $C_1^{(1)} \cup \dots \cup C_p^{(1)}$ , but  $f_Y(x_1, \dots, x_n, a_1)$  is not in  $C_1^{(2)} \cup \dots \cup C_p^{(2)}$ . This is the heart of the difficulty.

Suppose, for example, that  $n$  is 1, the number  $p$  is 4, and  $a_1$  is  $a_N$  (a member of  $C_4^{(2)}$ ) in all cases which we shall consider below. Let  $a_1$  be a member of  $C_1^{(1)}$  (and hence of  $C_1^{(2)}$ ). Then  $f_Y(a_1)$  is in  $C_2^{(1)}$ , but the corresponding element  $f_Y(a_1, a_N)$  is in  $C_5^{(2)}$ . Also  $f_Y(f_Y(a_1))$  is in  $C_3^{(1)}$ , but  $f_Y(f_Y(a_1), a_N)$  is in  $C_6^{(2)}$ .  $f_Y(f_Y(f_Y(a_1)))$  is in  $C_4^{(1)}$ , but  $f_Y(f_Y(f_Y(a_1), a_N), a_N)$  is in  $C_7^{(2)}$ . In order that Herbrand's proof work it seems to be necessary to show that this sort of thing cannot happen. For if it does happen, the proof that  $\overline{\eta}$  implies  $\eta$  does not work.

For many purposes it would be adequate to know that if  $P_1$  has the property  $C$  of order  $p$ , then  $P_2$  has the property  $C$  of order  $q$ , where  $q$  is an explicitly given function of  $p$ . I do not see how Herbrand's proof can be used to yield even this result, however.

I hope that with Herbrand's thesis before you, my remarks above will be sufficiently clear to be understandable. I am sorry that I did not have a chance to talk with you when you were in Princeton.

Herbrand's proof is of more than historical interest to me, for I have been looking into the problem of generalizing Herbrand's theorem in some form to type theory, and it at one time seemed that Herbrand's finitary proof might generalize more readily than other proofs of the theorem. I have recently become more thoroughly aware of the severe difficulties involved in attacking the problem from this direction, but I still have not ruled it out as a reasonable approach. Therefore I hope very much that the proof can be patched up, or that I have been misreading it and that it needs no patching up.

I shall be looking forward to hearing from you.

Respectfully,

*Peter Andrews*

Peter B. Andrews

Figure 2. Second page of 1962 April 9 letter from Andrews to Dreben.

In his reply of May 18 (Figures 3 and 4), Dreben ascribed my difficulty understanding Herbrand's argument to a slight ambiguity in it.

On May 31 I pointed out (Figure 5) that there still seemed to be a problem with the proof.

We agreed that we needed to discuss this matter face-to-face, so I drove to Cambridge and we had a long discussion of Herbrand's proof on June 19, 1962. Dreben tried to show me that while Herbrand's argument was obscure at some points, it was essentially correct. I've learned



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DEPARTMENT OF PHILOSOPHY

EMERSON HALL  
CAMBRIDGE 38, MASSACHUSETTS

May 18, 1962

Dear Mr. Andrews,

The difficulty you are having with pages 103 and 104 of Herbrand results from a slight ambiguity in his argument.

Let us distinguish between the réduite  $R(P)$  of an expression  $P$  over a domain  $D_p = C_1 \cup C_2 \cup \dots \cup C_p$  and the evaluation  $\Pi(P)_p$  of  $R(P)_p$  over  $D_p$ . In  $R(P)_p$  there are no quantifiers but, in general, there are function letters. In  $\Pi(P)_p$  the functional expressions no longer appear. However in the forming of  $\Pi(P)_p$  from  $R(P)_p$ , if an argument  $a_{i_j}$  of  $f_y(a_{i_1}, \dots, a_{i_n})$  is an element of  $C_p$ , then the expression  $f_y(a_{i_1}, \dots, a_{i_n})$  must be replaced by (the name of) an element in  $C_{p+1}$  and not by an element in  $D_p$ . See page 101 lines 11-13.

Now let  $R(P_1)_p$  be the réduite of  $P_1$  over  $D_p^{(1)}$ , let  $R(P_2)_p$  be the réduite of  $P_2$  over  $D_p^{(2)}$ , and let  $R'(P_1)_p$  be the réduite of  $P_1$  over  $D_p^{(2)}$ . Moreover, let  $\Pi(P_1)_p$  be the evaluation of  $R(P_1)_p$  over  $D_p^{(1)}$ , and  $\Pi(P_2)_p$  be the evaluation of  $R(P_2)_p$  over  $D_p^{(2)}$ . By hypothesis, both  $\Pi(P_1)_p$  and  $\Pi(P_2)_p$  are given. Herbrand's problem is to specify an evaluation  $\Pi'(P_1)_p$  of  $R'(P_1)_p$  over  $D_p^{(2)}$  in such a way that:

1)  $\Pi(P_1)_p \supset \Pi'(P_1)_p$  is truth functionally valid

and 2) If  $\Pi(P_2)_p$  is not truth functionally valid, then  $\Pi'(P_1)_p$  is not truth functionally valid.

And this he does in a straightforward manner once we remember

(1) that, since  $\Pi(P_2)_p$  is given, the crucial element  $a_k$  on page 104 line 14 occurs either in  $D_p^{(2)}$  or in  $C_{p+1}^{(2)}$ , and (2) that the

Figure 3. First page of 1962 May 18 letter from Dreben to Andrews.

over the years that many people think faster than I do, but this was one discussion for which I was well prepared. Every time Dreben proposed another interpretation of Herbrand's argument, I already knew what was wrong with it. We discussed it for hours, but finally Dreben realized that there really was a problem with Herbrand's proof. Our discussion

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part of  $\mathcal{P}(P_2)_p$  represented by line 8 on page 164 occurs once and only once in  $\mathcal{P}(P_2)_p$ .

I apologize again for taking so long to answer. Do not hesitate to write me about any points in Herbrand, and, as I said in my note of May 6, if it is possible for you to come up here I should very much like to see you.

Sincerely,

*Burton Dreben*

Figure 4. Second page of 1962 May 18 letter from Dreben to Andrews.

turned to ways of patching up the proof, but we didn't find a way to do this, and I went back to Princeton.

A few weeks later I received the letter in Figure 6. Dreben had found an actual counterexample to Lemma 3.3. He had found that the wff

$$\begin{aligned} & [\forall y_1 M y_1 \vee \forall y_2 N y_2 \vee \exists x_3 \sim G x_3] \\ \vee & [\exists x_1 \sim M x_1 \wedge \exists x_2 \sim N x_2 \wedge \forall y_3 \sim H y_3] \\ \vee & [\exists x_4 H x_4 \wedge \forall y_4 G y_4] \end{aligned}$$

has property C of order 2, while the wff

$$\begin{aligned} & [\forall y_1 M y_1 \vee \forall y_2 N y_2 \vee \exists x_3 \sim G x_3] \\ \vee & \exists x_1 \exists x_2 [\sim M x_1 \wedge \sim N x_2 \wedge \forall y_3 \sim H y_3] \\ \vee & \exists x_4 [H x_4 \wedge \forall y_4 G y_4], \end{aligned}$$

which can be obtained from it by Rules of Passage, has property C of order 4 but of no smaller order.

A simplification of Dreben's counterexample is discussed in a letter which I sent to Dreben on July 16 (Figures 7 - 8).

$$\sim [\forall x \Phi x \vee \sim \forall y R y] \vee \forall y_1 \Phi y_1 \vee \sim \forall x_1 R x_1$$

has property C of order 2, while the wff

$$\sim \forall x [\Phi x \vee \sim \forall y R y] \vee \forall y_1 \Phi y_1 \vee \sim \forall x_1 R x_1,$$

which can be obtained from it by one application of a Rule of Passage, does not have property C of order 2.

Throughout the fall Dreben kept working on weaker forms of the lemma which would still suffice to prove the theorem, and finding counterexamples to them. In November I received the letter in Figure 9, stating that Herbrand's error was much deeper than Dreben had

116 Linden Lane  
Princeton, New Jersey  
1962 May 31

Professor Burton S. Dreben  
Emerson Hall  
Harvard University  
Cambridge 38, Massachusetts

Dear Professor Dreben,

I certainly appreciate your willingness to answer questions about Herbrand's thesis. As you probably realize, there are not many persons in a position to answer such questions authoritatively.

Your letter of May 18 seems quite clear, but there is one source of difficulty (which I had not yet noticed when I acknowledged receipt of your letter) which makes me wonder whether you wrote what you intended to write, or whether I am interpreting your letter correctly. Permit me to explain.

According to your terminology,  $R(P_1)_p$  is the réduite of  $P_1$  over  $D_p^{(1)}$ , and  $\Pi(P_1)_p$  is the evaluation of  $R(P_1)_p$  over  $D_p^{(1)}$ . Thus the individuals occurring in  $\Pi(P_1)_p$  as arguments of the predicates are members of  $D_{p+1}^{(1)}$ .  $R'(P_1)_p$  is the réduite of  $P_1$  over  $D_p^{(2)}$ , and  $\Pi'(P_1)_p$  is an evaluation of  $R'(P_1)_p$  over  $D_p^{(2)}$ . Hence the individuals occurring in  $\Pi'(P_1)_p$  are members of  $D_{p+1}^{(2)}$ . Thus if the members of  $D_{p+1}^{(1)}$  are distinct from the members of  $D_{p+1}^{(2)}$ , the atomic formulas occurring in  $\Pi(P_1)_p$  are distinct from the atomic formulas occurring in  $\Pi'(P_1)_p$ , and it is not clear why  $\Pi(P_1)_p \supset \Pi'(P_1)_p$  should be a tautology. If, on the other hand, you are tacitly assuming that  $D_{p+1}^{(1)}$  is embedded in  $D_{p+1}^{(2)}$ , the objection I raised in my letter of April 9 seems to apply.

I think it would be easier to discuss this question in conversation than by letter, and I am quite eager to hear your comments about my thesis topic. In addition, I have been hoping for some time for an opportunity to learn more about your work. Therefore I would like to accept your suggestion that I come to see you. I think I could come virtually any time this summer that is convenient for you, with the exception of the period between June 27 and July 14. Just to be concrete, let me tentatively suggest that I come about June 11, but if this is not a good time for you, please suggest another.

Figure 5. First page of 1962 May 31 letter from Andrews to Dreben.

July 13, 1962  
 DREBEN  
 40 G.B. SEAVEY  
 FORGE VILLAGE,  
 MASS.  
 R.F.D.

Dear Andrews,  
 Here is a counterexample to Herbrand's Lemma 5.2.3

$S$  is  
 $(Y_1/MY_1, v(Y_2/MY_2, v(\exists X_3) \bar{G}X_3 : v(\exists X_1, \bar{M}X_1, \&(\exists X_2, \bar{M}X_2) \&(Y_3) \bar{H}Y_3)$   
 $: v(\exists X_4) \bar{H}X_4 \& (Y_4) \bar{G}Y_4$

$S$  has property C of order 2.

Now consider its equivalent  $S'$ :

$(Y_1/MY_1, v(MY_1/MY_1, v(\exists X_3) \bar{G}X_3 : v(\exists X_1, \bar{M}X_1) [\bar{M}X_1, \bar{M}X_2 \& (Y_3) \bar{H}Y_3]$   
 $: v(\exists X_4) \bar{H}X_4 \& (Y_4) \bar{G}Y_4$

$S'$  has property C of order 4 and of no smaller order.

I think, however, I can prove a corrected  
 version of the lemma. Will write soon.

Best  
 Peter B. Andrews

Figure 6. 1962 July 13 letter from Dreben to Andrews.

previously thought, and mentioning some work by Aanderaa which showed this.

As a result of all this we published the paper [23] giving counterexamples to Herbrand's key lemma.

116 Linden Lane  
Princeton, N. J.  
1962 July 16

Dear Professor Dreben,

Thank you for your letters. I apologize for not writing you sooner, but for the past two weeks I've been off on a trip to California for my brother's wedding and various other business.

Your counter-example to Herbrand's lemma certainly clears up a lot of questions. You make me feel very humble for not finding one myself. As you have probably noticed, it can be simplified somewhat: <sup>(Incidentally, thank you for sending me word of this important discovery without waiting to see if it could be refined!)</sup>  
Let  $S$  be

$\forall y_2 N y_2 \vee [\exists x_2 \sim N x_2 \wedge \forall y_3 \sim H y_3] \vee \exists x_4 H x_4$   
and let  $S'$  be

$\forall y_2 N y_2 \vee \exists x_2 [\sim N x_2 \wedge \forall y_3 \sim H y_3] \vee \exists x_4 H x_4$

$S$  has property  $C$  of order 2, but  $S'$  does not.  $S'$  does have property  $C$  of order 3, however.

It is enlightening to go through Herbrand's proof with this counter-example in mind, and perhaps you will not take it amiss if I write out the details explicitly, in spite of the fact that you have probably already done this yourself. For this purpose let us make some trivial changes in the above

Figure 7. First page of 1962 July 16 letter from Andrews to Dreben.

Dreben pursued this matter vigorously, and a few years later he and John Denton finally managed to prove [24] a weaker form of the lemma which was still sufficient for filling the gap in Herbrand's proof.

This finishes one chapter in the story of Herbrand's Theorem, but there are some footnotes.

2)

example:

$$\text{Let } P_1 \text{ be } \sim [\forall x \Phi x \vee \sim \forall y R y] \vee \forall y \Phi y, \vee \sim \forall x, R x,$$

$$\text{Let } P_2 \text{ be } \forall x [\Phi x \vee \sim \forall y R y] \vee \forall y, \Phi y, \vee \sim \forall x, R x,$$

Let us agree to write  $f_{ij}(a_1, \dots, a_n)$  as  $f_{ij}^{a_1, \dots, a_n}$ , thus letting general variables stand for their functions of indices.

Then

$$C_1^{P_1} = \{\alpha\} \quad C_2^{P_1} = \{y_1, y\}$$

The (evaluated) reduct of  $P_1$  of order 2 is  
 $\sim [\Phi_\alpha \wedge \Phi_{y_1} \wedge \Phi_y \vee \sim R_y] \vee \Phi_{y_1} \vee$   
 $\sim [R_\alpha \wedge R_{y_1} \wedge R_y],$   
 which is a tautology.

Also

$$C_1^{P_2} = \{\alpha\} \quad C_2^{P_2} = \{y_1, y^\alpha\}$$

The (evaluated) reduct of  $P_2$  of order 2 is

$$\Pi_2 = \sim \left\{ \underset{T}{[\Phi_\alpha \vee \sim R_{y^\alpha}]} \wedge \underset{F}{[\Phi_{y_1} \vee \sim R_{y_1}]} \wedge \underset{T}{[\Phi_{y^\alpha} \vee \sim R_{y^\alpha}]} \right\} \\ \vee \underset{F}{\Phi_{y_1}} \vee \sim \underset{T}{[R_\alpha \wedge R_{y_1} \wedge R_{y^\alpha}]}$$

which can be falsified by the indicated truth value assignment.

Now according to Henkin p. 104, we must define the 'function'  $f_{ij}$  in the  $P_1$  fields as  $f_{ij}^{a_i}$  in the  $P_2$  fields for an appropriately chosen  $a_i$ . The crucial part

Figure 8. Second page of 1962 July 16 letter from Andrews to Dreben.

HARVARD UNIVERSITY  
DEPARTMENT OF PHILOSOPHY

EMERSON HALL  
CAMBRIDGE 38, MASSACHUSETTS

November 27, 1962

Dear Andrews,

I'm truly sorry to have taken so long on our paper, but just as I was getting back to it I became ill. However, I'll return to work in a day or two.

My new proof of Lemma 5:3.3 is quite complicated, and will entail changing our paper. Herbrand's error is much deeper than I had previously thought. About two months ago, Stal Andersa, a student of mine, showed me that given any number  $p$  we can construct schemata  $S$  and  $T$  such that  $S$  and  $S \supset T$  both have property  $C$  of order  $\beta$  but  $T$  has property  $C$  only of orders greater than  $p$ . My new proof of Lemma 5:3.3 now explains this phenomenon. No function of  $p$  alone will ever be sufficient to give in general the upper bound of a single use of the crucial rule of passage.

How is your thesis going? Is Schutte's paper in the latest J.S.L. relevant?

As ever,

*But Dreben*

Figure 9. 1962 November 27 letter from Dreben to Andrews.

While our paper on false lemmas was being written, I was considering where I should be the following year, and I heard a suggestion that I should find out if there might be any kind of suitable visiting position at the Institute for Advanced Study in Princeton. I wrote to Kurt Gödel about this, and by way of introduction mentioned that I had found a mistake in Herbrand's proof. Gödel subsequently mentioned to Church on the telephone that he had known of errors in Herbrand's work, and when I told Dreben about this, he sent a draft of our paper to Gödel. Gödel chose not to reply to this letter, but when Gödel's papers were examined after his death, it was found<sup>3</sup> that in the early 1940's Gödel had seen the fallacy in Herbrand's argument (though there is no evidence that he had an explicit counterexample to Herbrand's lemma), and he had devised a correction which was in all essentials the same as that in Dreben and Denton's paper [24].

<sup>3</sup> See page 389 of [28].

Warren Goldfarb has speculated [29, p. 113] that one factor which may have contributed to Gödel's reticence to discuss his correction was that Gödel was not sure that Herbrand's lemma (which was much stronger than the corresponding lemma in Gödel's correction) was actually false.

I mentioned Herbrand's error to Georg Kreisel when he visited Princeton in the spring of 1963, and he mentioned it in a letter to Paul Bernays. Bernays replied in a letter<sup>4</sup> to Kreisel that Gödel had mentioned Herbrand's error to him in 1958.

It is interesting to ask where a correct proof of some form of Herbrand's Theorem was first published. A likely candidate is [32, pp. 2-33, 157ff]. Of course, Herbrand's Theorem is closely related to Gentzen's sharpened Hauptsatz [25].<sup>5</sup>

I next turned my attention to formulating and proving a cut-elimination theorem for Church's type theory, and eventually came up with what seemed like a rather nice proof. After checking it carefully I took it to Professor Church. He read it thoroughly with me and agreed that it looked like a good proof.

However, I had heard that Gaisi Takeuti had done some work [56] which might be relevant, and I thought it was time to find out about it. Takeuti was working with a rather different formulation of type theory. He had done extensive work on the cut-elimination problem for this system, which was generally referred to as Takeuti's Conjecture. If one adds an Axiom of Infinity to type theory, one obtains a system in which one can formalize mathematical analysis and much more, so it is appropriate to use Analysis as the name for the logical system consisting of type theory with an Axiom of Infinity. Takeuti had shown that a cut-free system of analysis must be consistent; thus, on the metatheoretic level Takeuti's Conjecture implies the consistency of Analysis [57].

I soon saw that the same basic ideas did indeed apply to the context in which I was working, and that my cut-elimination theorem implied the consistency of my formulation of analysis. Various technicalities involving axioms of extensionality and descriptions arise, and some years later I published a paper [2] explicating these matters. Some work by Robin Gandy plays a key role in that paper.

My proofs of the cut-elimination theorem and the derivation of the consistency of analysis from it were purely syntactic, and it was clear that they could be formalized within the formulation of analysis I was working with. Thus, it seemed clear that I had all the ingredients of a proof of the consistency of this system within itself. By Gödel's Second

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<sup>4</sup> This letter was mentioned in a letter from Andrews to Dreben dated 1963 April 7.

<sup>5</sup> See page 106 of [26].



Theorem, this could happen only if this system of analysis were actually inconsistent. Clearly, I had a problem.

I went back and looked at my cut-elimination proof very critically, and eventually I did indeed find an error in it, well hidden in a part of the proof which seemed like the last place one would expect any difficulty. I am grateful to Takeuti and Gödel, for without the benefit of their work I probably would not have reexamined this proof, and eventually someone else would have found the error.

One sees over and over again that mistakes do occasionally occur in mathematical reasoning, and we can look forward to the day when this community builds tools which will make it practical for serious mathematical proofs to be checked routinely.

A few years later Takahashi proved Takeuti's conjecture [55] using semantical methods which could not be formalized within analysis, and by coincidence Prawitz proved the same result [46] at just about the same time. Both proofs relied on some ideas which had been developed by Schütte [50].

Ray Smullyan had developed a very elegant metatheorem which we know as Smullyan's Unifying Principle [52, 53], and I built on the ideas of Schütte, Takahashi, and Smullyan to establish [1] a version of Smullyan's Unifying Principle for the subsystem of Church's type theory which I call elementary type theory. This is Church's system minus axioms of extensionality, descriptions, choice, and infinity. Elementary type theory embodies the logic of propositional connectives, quantifiers, and  $\lambda$ -conversion in the context of type theory. From this Unifying Principle I derived the completeness of a rather weak form of resolution for type theory, as well as a cut-elimination theorem for elementary type theory.

Gérard Huet, who was a student at that time, read this paper, or a preprint of it, and came to talk to me about it. He was interested in the problem of a unification algorithm for type theory, which was still open, and I encouraged him to work on it. He soon devised a unification algorithm for type theory [33]. By coincidence, Jensen and Pietrzykowski [36] devised a similar algorithm about the same time.

Our group developed a theorem proving system called TPS [41] which searched for matings [4] using higher-order unification [33], and translated these into natural deduction proofs [3]. TPS could prove some theorems of type theory as well as theorems of first-order logic, but it was far from complete for higher-order logic.

I then proved the following

**Theorem.** A sentence is provable in elementary type theory if and only if it has a tautologous development.

**Definition.** A *development* of a sentence is a wff obtained from it by a sequence of the following operations, which may be applied to parts of the wff which are not in the scopes of quantifiers:

- Delete an essentially universal quantifier after assuring that its variable is not free in the current wff.
- Duplicate an essentially existential quantifier.
- Instantiate an essentially existential quantifier with an arbitrary wff.
- Apply  $\lambda$ -reduction.

Note that we are working here with provability rather than refutability, so we instantiate existential rather than universal quantifiers.

This theorem is a consequence of the cut-elimination theorem. It is an extension to type theory of Herbrand's Theorem inspired by Herbrand's Property A. The theorem and its proof were eventually published in [15].

This provided a significant step toward developing a general theorem proving system for type theory, but a better representation of higher-order Herbrand expansions was needed. Dale Miller developed the idea of using a tree-like structure called an *expansion tree proof*, otherwise known as an *expansion proof*, to concisely represent the theorem, the substitution terms, the tautology, the associated mating, and the relationships between these entities. Miller proved

**Miller's Expansion Proof Theorem.** A sentence is provable in elementary type theory if and only if it has an expansion proof [38, 40].

This is a very elegant generalization of Herbrand's Theorem to Church's type theory. The proof used Smullyan's Unifying Principle for type theory.

Miller also gave the details of an explicit algorithm for converting expansion proofs into natural deduction proofs, and proved that it works.

In his thesis [43] Frank Pfenning investigated a variety of issues in higher-order proof transformations, and developed an improved method of translating expansion proofs into natural deduction proofs based on the use of tactics.

The ideas developed by Miller and Pfenning lie at the heart of the current version of the automated Theorem Proving System TPS [9, 58], and provide a firm foundation for automated theorem proving in higher-order logic and for the investigation of essential structural features of theorems of higher-order logic.

This completes another chapter in the story of Herbrand's Theorem.

Herbrand's Theorem, and various extensions, enhancements, and refinements of it, have served us very well. Who knows what other fundamental insights into the structures of theorems remain to be discovered? We may think that theorem proving is simply hard, and at least in the realm of classical first-order logic without equality, all we can do is continue to find very efficient ways of searching exhaustively within the context of currently known theoretical frameworks. However, the history of science is full of surprises. In the latter part of the nineteenth century it seemed to many people that all the fundamental discoveries which could be made in physics had been made. Then along came twentieth century physics. Who knows what waits to be discovered in our field?

I'd like to conclude by emphasizing what a wonderful field this is to work in. Logical reasoning plays such a fundamental role in the spectrum of intellectual activities that advances in automating logic will inevitably have a profound impact in many intellectual disciplines. Of course, these things take time. We tend to be impatient, but we need some historical perspective. The study of logic has a very long history, going back at least as far as Aristotle. During some of this time not very much progress was made. It's gratifying to realize how much has been accomplished in the less than fifty years since serious efforts to mechanize logic began. We're making very satisfying progress.

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