10 November 1989

Dear Van,

Section 2.6 of my June draft of <u>Parts of Classes</u> ended with the conclusion that the project of 'Ramsifying out the singleton function' fails because we can't at that stage of the game quantify over relations. If we try it by saying 'there are some pairs' we need a prior notion of ordered pair; but we can't at that stage of the game use a set-theoretical definition of pairing, and primitive pairing would be much of a muchness with the primitive singleton function that we were trying to do away with.

As you know, that conclusion was wrong. Given plural quantification and mereology, Hazen has shown how we can, in effect, quantify over ordered 'tuples of atoms; and you have finished the job by showing how to get from there to ordered 'tuples of fusions of atoms. So we can say, in effect, that there are some ordered fusion-atom pairs that satisfy the appropriate conditions for a singleton function.

Independently, and near enough simultaneously, John Burgess found a quite different way to accomplish the same thing.

I'd like to include these new developments in <u>Parts of Classes</u>. Here is how I'd like to do it, if you and the others consent. In the first place, Section 2.6 should end not with the statement that there's no satisfactory way to introduce pairing, but rather with the statement that introducing pairing is 'unfinished business'. Then I'd like to add a co-authored appendix to the book, under the names (in alphabetical order) of Burgess, Hazen, Lewis, and Quine. I would like to write it myself, in the same style as the rest of the book, and submit it to the three of you for approval. The appendix would give the Burgess method of introducing pairing, then the Hazen-Quine method, then some brief discussion of what difference the possibility of introducing pairing makes to things said earlier.

I have just signed a contract with Blackwell to publish <u>Parts of Classes</u>, and I have given them a manuscript. (Not including appendix or adequate correction of 2.6.) The schedule is a bit tight: I'm to check the copy-edited manuscript in December or January and read proof in May or June, for a publication date in September 1990. I've talked to Stephan Chambers at Blackwell about the possibility of adding an appendix, and he says that it can be done if I'm quick about it, and if the co-authors can agree. He would prefer (but this is negotiable) an arrangement on which you and Hazen and Burgess each had a separate contract with Blackwell, receiving a fixed fee rather than a fraction of the royalties. He would rather like it if the rest of you were willing to let me take responsibility for checking edited manuscript and proof of the co-authored appendix, but he is very far from insisting on this.

Chambers doesn't know yet, by the way, that you are one of the proposed co-authors. Hazen told me that 'a correspondent' had suggested the extension to 'tuples of fusions, and I guessed wrong who this correspondent was.

Burgess and Hazen consent in an unspecific way to the co-authored appendix, though we haven't yet talked about fees or royalties, whether they want to check the proof, and so on. Whether I can write something they are happy to put their names to remains to be seen. I'd have to be very careful not to say as spokesman for the four some of the things I myself would say. Especially things about the ontological innocence of plural quantification and mereology: Hazen, at least, dissents vigorously.

Would you be willing to join me in such a plan? Or in a plan somewhat resembling it?

Yours,

David Lewis