

Referee Report on
“Fiscal Policy and Imperfect Competition: A Survey,”
by L. F. Costa & H. Dixon.

This long manuscript has two differentiated parts. The first one (Section 2: Static models, based on lecture notes by the first author) presents a by now conventional model of Dixit-Stiglitz-Bertrand competition with a continuum of varieties and Cournot competition within each variety. It then describes the implications for the Keynesian multiplier as developed mainly by Dixon (1987) and a few subsequent papers. The second part (Section 3, Intertemporal models, based on lecture notes by the second author), describes a dynamic model with appealing parallelisms with the static model of Section 2 and analyzes the dependence of the Keynesian multipliers on the degree of market power. Taken together, they comprise a useful text, which could, after revision, be accepted for publication.

But I do have some objections. First, the title (and the abstract) are grossly misleading. The submission is not a “survey of fiscal policy and imperfect competition,” but rather an exposition of a small fraction of the literature, namely a fraction closely related to Dixon’s undoubtedly distinguished contributions. The submission neglects or cursorily references a substantial amount of relevant published work that helped develop the approach of Sections 2 and 3. Bénassy, who has widely and deeply contributed to the issues in numerous articles, books and book chapters, gets a single mention for a single paper. D’Aspremont, Dos Santos Ferreira and Gérard-Varet get only slightly better credit.

This disregard is rationalized as follows. In page 2, we read “... the systematic treatment of the problem, isolated from further considerations, assumptions and results related to the general equilibrium, can only be found in the second half of the 1980’s.” And page 13 states “We may affirm that the first works exclusively dedicated to this topic are Dixon (1987) and Mankiw (1988).” Thus, it appears that a piece of work may be omitted even if closely related to the topic as long as the piece also covers unrelated material. This is a puzzling criterion. I do support organizing the results around a single, specific model, and I do not suggest contending the issue of priority for every single idea. But a more scholarly approach would be to mention

qualitatively similar results found in the literature when discussing a result formally deduced from the model of the paper,

Second, the endogeneity of the markup μ (or of the degree of monopoly) is a main virtue of these models, μ being determined by demand, costs and market structure. For instance, we learn in page 13 that, in the first models presented, the markup is the reciprocal of σ , the elasticity of substitution in consumption, making the markup constant. But in fact the markup is endogenous: it is the parameter σ that is exogenous. The equilibrium conditions of the model then endogenously determine μ as $\mu = 1/\sigma$.

Nevertheless, the markup is often described throughout the submitted text, explicitly implicitly, as exogenous. For instance, we read in page 27 that “up to this point, we considered that μ was an exogenous variable.” This language is misleading, and should be purged throughout the paper, including in the comparative statics “with respect to μ ” (as in pages 35 ff).

Third, I would suggest forgetting about Ψ , the “unproductive labour hired by the government,” which the text (p. 5) justifies as follows. “This is only a device used by Mankiw to simulate public-debt financing of government expenditure in a static model.” The submitted text does have a dynamic model, namely that in Section 3, the natural setup to discuss public-debt financing if so desired. In my view, Mankiw’s device is one of the main drawbacks of his simplistic model. In the present submission, Ψ is zero in all interesting scenarios, including the dynamic model of Section 3. It is only positive in Case I of the static model (p. 6), where the budget constraint of the government is satisfied, given the government’s decisions on expenditure and tax-parameters, by adjusting Ψ , a story hard to swallow. Hence, I recommend deleting Ψ , and Case I, from the text.

For the most part, the submission is competently written. Some minor suggestions and comments follow.

p. 1. Capitalize “hart”. In what follows, words such as “walrasian, keynesian, cournotian” or “schumpeterian” should also be capitalized.

p. 2. “by Dixon and Rankin.”

- p.3. There is no need for U in (1).
- p. 4, line 6: “direct taxes are an affine function of...”
- p. 5, line 11. “is undetermined” instead of “cannot be undetermined ex ante” (there is no uncertainty in the model).
- p. 6 ff. The notation “ $i(j)$ ” is confusing, because it looks like functional notation. Firm i in industry j could be simply denoted by the double index (i, j) .
- p. 7. Immediately after (13), it should be indicated that the firm views $p(j)$ as a function, among other things, of its output decision. Otherwise, (13) appears as the program of a Walrasian firm.
- p. 8. (14) implies that the fixed cost is avoidable. Hence, profits are nonnegative,
- p. 8, line 16. “given by equations (11) and (12).
- p. 8, (18) $\forall s \in [0, n], s \neq j$.
- p. 9. Section 2.1.5. Simply state that you focus on symmetric equilibria.
- p. 9, next-to-last line “value”
- p. 10, footnote 15. “...unemployment equilibria in our model.”
- p. 11 (27) The notation $(G, .)$ is unsatisfactory. “Other variables” should not appear as arguments for the equilibrium values of wages, labour or profits.” If there is a parameter that should be made explicit for the sake of posterior comparative statics, then it should be listed following, say, a vertical line.
- p. 12. (29), and elsewhere. it is preferable to maintain round parentheses $()$ for functional notation.

p. 30, I imagine that (36) is of the form “ $\max \{expression, 0\}$,” where the “, 0” has been lost. This would agree with (14). But then profits are nonnegative, contrary to what is stated in the last line of page 31.

p. 33, line 9: $m(j)$ instead of $m(\tau)$.

p. 33, two lines after (41): “it will be a straight line” (instead of “linear”)

p. 42. I imagine that Λ stands for the more conventional Δ .

p. 45. Kiyotaki.