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A CRITIQUE OF "IDENTIFYING STRUCTURAL EQUATIONS THROUGH THE NONLINEARITY OF A SINGLE PRICE GRADIENT" AND "IDENTIFYING STRUCTURAL EQUATIONS WITH SINGLE MARKET DATA" BY ROB MENDELSOHN

by

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I. What is the underlying utility-maximization model?

One can distinguish four different models depending on whether there is a single "attribute" (i.e., z is a scalar) or there are many attributes (z is a vector) and whether or not "other goods" are included as arguments in the utility function.

A. Other goods are arguments and z is a scalar

Each individual consumer has a utility function $u(z, x; s)$, where s is a vector of shift variables (age, sex, etc.).

1. If x is a scalar (a Hicksian composite commodity) and is taken as the numeraire, the individual solves

$$\underset{z,x}{\text{maximize}}\ u(z, x; s)$$

subject to $x + c(z) = y$, where y is his total expenditure/income. This leads to

$$c_z(z) = \frac{u_z[z, y - c(z); s]}{u_x[z, y - c(z); s]} \tag{1a}$$

$$x = y - c(z). \tag{1b}$$

2. When x is a vector, $x = (x_1, \ldots, x_N)$ and x_N is taken as the numeraire, the individual solves

$$\underset{z,x_1,\ldots,x_N}{\text{maximize}}\ u(z, x; s) \tag{2}$$

subject to

$$\sum_{1}^{N-1} p_i x_i + x_N + c(z) = y.$$

This leads to

$$c_z = \frac{u_z\left(z, x_1, \ldots, x_{N-1}, y - c(z) - \sum_{1}^{N-1} p_i x_i; s\right)}{u_N\left(z, x_1, \ldots, x_{N-1}, y - c(z) - \sum_{1}^{N-1} p_i x_i; s\right)} \tag{3a}$$

$$p_i = \frac{u_i\left(z, x_1, \ldots, x_{N-1}, y - c(z) - \sum_{1}^{N-1} p_i x_i; s\right)}{u_N\left(z, x_1, \ldots, x_{N-1}, y - c(z) - \sum_{1}^{N-1} p_i x_i; s\right)} \quad i = 1, \ldots, N - 1 \tag{3b}$$

$$x_N = y - c(z) - \sum_{1}^{N-1} p_i x_i. \tag{3c}$$

B. Other goods are arguments and z is a vector, $z = (z_1, \ldots, z_k)$

1. When x is a scalar, the first-order conditions are

$$c_k(z) = \frac{u_k[z, y - c(z); s]}{u_x[z, y - c(z); s]} \qquad k = 1, \ldots, K \tag{4a}$$

$$x = y - c(z). \tag{4b}$$

2. When x is a vector, if the maximization problem is (2), the first-order conditions are given by equations (3b), (3c), and

$$c_k(z) = \frac{u_k\left(z, x_1, \ldots, x_{N-1}, y - c(z) - \sum_1^{N-1} p_i x_i; s\right)}{u_N\left(z, x_1, \ldots, x_{N-1}, y - c(z) - \sum_1^{N-1} p_i x_i; s\right)} \qquad k = 1, \ldots, K. \quad (5)$$

Suppose, however, that u is separable in x and z:

$$u(z, x; s) = w[g(z; s), x]. \qquad (6)$$

Then we can imagine the following two-stage procedure:

a. $\max_{z} w[g(z; s), x]$ (7)

subject to $c(z) = y_z$

$\Rightarrow$ (in principle) $z = z(y_z)$.

b. $\max_{x, y_z} w\left(g[z(y_z); s], x\right)$

subject to $\sum_1^{N-1} p_i x_i + x_N + y_z = y$.

The first-order conditions for the first stage (7) are

$$\frac{c_k(z)}{c_K(z)} = \frac{g_k(z; s)}{g_K(z; s)} \qquad k = 1, \ldots, K - 1 \qquad (8a)$$

$$y_z = c(z). \qquad (8b)$$

C. Other goods are not arguments and z is a scalar. The indidividual solves

$$\max_{z} u(z; s) \tag{9}$$

subject to c(z) = y. This is an uninteresting problem because the optimal value of z is determined solely by the budget constraint. The solution is

$$z = c^{-1}(y),$$

and it is independent of the form of the utility function u(z; s).

This leads to the first point I want to make. When Rob treats z as a scalar (e.g., "Nonlinearity . . ." paper, pages 2-6), he must be assuming implicitly that other goods are arguments of the utility function, i.e., he must be adopting implicitly model (A) rather than model (C). This has implications for the function on the right-hand side of the first-order condition, (1a), which Rob writes as

$$P(z) = F(z, s). \tag{10}$$

[My $c_z(z)$ is his P(z), and my (u_z/u_x) is his F(•); I have changed the shift variable from his "Y" to my "s."] Comparing equation (10) with (1a), one observes that F(•) should include x or [y - c(z)] as an argument.

D. Other goods are not arguments and z is a vector. A similar objection to Rob's model applies in this case, but the argument is more complicated. One could postulate the maximization problem

$$\max_{z} u(z) \tag{11}$$

subject to $c(z) = y$. Unlike (9), this is an interesting maximization problem, and it leads to first-order conditions that are similar to (8a) and (8b). However, I am not sure what problem (11) means. It makes sense to me in only two circumstances.

1. The utility function also contains x's, and the model really is (B) in which case the first-order conditions are given by (4a, b) or (3b, c), and (5); then, as before, the marginal rate of substitution function on the right-hand side of (4a) or (5) should include as an argument x or $y - c(z)$.
2. The utility function in equation (11) is really a subutility function which is part of an overall, separable utility function. In this case, (11) corresponds to the first stage of a two-stage maximization procedure, and the appropriate first-order conditions are given by (8a) and (8b). Again, I have problems with the way in which Rob writes his first-order conditions. He writes ("Nonlinearity . . ." paper, page 6):

$$P_k(z) = F_k(z;\ s). \tag{12}$$

Comparing (12) with (8a), we observe that p(z) must be interpreted as the ratio of partial derivatives of the cost function,

$$P_k(z) \equiv \frac{c_k(z)}{c_K(z)}; \tag{13}$$

and, similarly, $F_k(z;\ s)$ must be interpreted as the ratio of derivatives of the (sub-) utility function. The point is, one has the impression from Rob's paper that there are K equations, such as (12), whereas there really are (K - 1) such equations. Moreover, the system of (K - 1) equations in (8a) or (12) is <u>incomplete</u>; there is a <u>K</u>th equation, which is given by (8b). The system (8a) without (8b) is meaningless--an infinity of z vectors would satisfy it. If one wants to have a system similar to (8a) that is logically complete, one has to solve (8b) for z_K as a function of y_z and $z_1, \ldots, z_{K-1}$

$$z_K = c^{-1}(z_1,\ \ldots,\ z_{K-1},\ y_z) \tag{8b'}$$

and substitute this into equation (8a) to obtain

$$\frac{c_k[z_1, \ldots, z_{K-1}, c^{-1}(z_1, \ldots, z_{K-1}, y_z)]}{c_K[z_1, \ldots, z_{K-1}, c^{-1}(z_1, \ldots, z_{K-1}, y_z)]}$$

$$= \frac{g_k[z_1, \ldots, z_{K-1}, c^{-1}(z_1, \ldots, z_{K-1}, y_z)]}{g_K[z_1, \ldots, z_{K-1}, c^{-1}(z_1, \ldots, z_{K-1}, y_z)]} \quad k = 1, \ldots, K - 1. \quad (8a')$$

The system in (8a') is a logically complete system, but it differs from Rob's system (12) in that y_z appears as an argument on both sides and, also, in the special way in which the left-hand side of (8a') is derived from the underlying cost function $c(z)$ via (8b').

Summary. Following Brown and Rosen, Rob has written the first-order conditions in the form of equation (10) when z is a scalar and in the form of (12) when z is a vector. I have argued that neither of these equations is consistent with utility maximization unless further structure is imposed.

II. Could these functions actually be polynomials?

When, following Brown and Rosen, Rob writes his first-order conditions in the form of (10) or (12) with polynomial equations on the left-hand side and right-hand side of these equations, the question arises: Are these polynomials consistent with utility maximization? When z is a scalar, the answer is yes but only in certain special cases. When z is a vector, the answer is even less likely to be yes (although it still could be). The point is that I consider the case where these functions are polynomials to be so specialized--if not implausible--that I do not think it deserves the attention it has received.

Suppose that z is a scalar. Given that c(z) is a polynomial, a sufficient condition for the right-hand side of (1a) to be a polynomial in z is that utility be a monotonic transformation of either

$$u(z, x) = h(z) + kx \tag{14}$$

or

$$u(z, x) = h(z) + k \ln x, \tag{15}$$

where h(z) is some polynomial and k is a positive constant. However, if utility is a monotonic transformation of

$$u(z, x) = h(z) + k(x), \tag{16a}$$

where h(z) and k(x) are polynomials, the right-hand side of (1a) takes the form

$$\frac{u_z}{u_x} = \frac{h'(z)}{k'[y - c(z)]}, \tag{16b}$$

which is a ratio of polynomials in z; in general, this ratio will not be a polynomial (although it could be one).

When z is a vector, the problem is even worse. Unless c(z) has the special form

$$c(z) = \lambda(z_1, \ldots, z_{K-1}) + \delta\, z_K \qquad \delta > 0, \tag{17}$$

where $\lambda(\cdot)$ is polynomial in $z_1, \ldots, z_{K-1}$ and utility is a monotonic transformation of either

$$u(z) = h(z_1, \ldots, z_{K-1}) + \gamma\, z_K \qquad \gamma > 0 \tag{14'}$$

or

$$u(z) = h(z_1, \ldots, z_{K-1}) + \gamma \ln z_K \qquad \gamma > 0 \tag{15'}$$

where h(•) is a polynomial, the left-hand side and right-hand side of (8a') are unlikely to be polynomials.

Even if utility does have the form (14) or (15) when z is a scalar or the forms (14') and (15') when z is a vector, there is another problem that concerns me. When the utility function involves polynomials of degree two or higher, there is no guarantee that it is quasiconcave--or even monotonic. Similarly, if c(z) is a polynomial of degree two or higher, there is no guarantee that it is quasiconvex. Moreover, even if c(z) were quasiconvex, the budget constraint x + c(z) is not necessarily quasiconvex in (x, z). [If c(z) were convex, then x + c(z) would be quasiconvex in (x, z)].

Accordingly, although the first-order conditions (1a) and (8a') may be necessary conditions for a solution to the consumer's utility-maximization problem, they are not sufficient conditions.[1] The assumption that the consumer does not have convex preferences, in particular, seems to me somewhat specialized and lacking in general interest--but perhaps I am old-fashioned.

III. The issue of Marshallian demand functions and inverse demand functions

For simplicity, I will focus on the case where z is a scalar and the first-order conditions are given by equation (1a).

If u(z, x) is strictly quasiconcave and x + c(z) is quasiconvex, (1a) has a unique solution for z which is the utility-maximizing level of attributes. If u(z, x) is quasiconcave, the maximum may not be unique. If u(z, x) is

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[1] I am ignoring the possibility of corner solutions for x [in which x = 0 or x = y and assuming that $c(z) \geq 0$]; also, if there is some constraint on the feasible set of z's, I am ignoring the possibility of corner solutions for z. If there is a corner solution, (1a) and (8a') are not even necessary conditions.

not quasiconcave and/or $x + c(z)$ is not quasiconvex, not every solution of (1a) is a solution to utility maximization.

A separate question is whether the solution to equation (1a) can be expressed in closed form as

$$z = \phi(y; s), \tag{18}$$

which I will call a Marshallian demand function. It can happen that there is a unique maximum, but it is not expressible in closed form as in (18).

Suppose that (18) can be inverted to obtain

$$y = \phi^{-1}(z; s) \equiv \psi(z; s). \tag{19}$$

When Rob presented his work at Maryland, I think I expressed disbelief in this function, which he writes as $A(z)$. If so, I was wrong. I am willing to believe that (19) might exist, and <u>I can see circumstances in which one might wish to estimate (19) rather than (18)</u>. This is because, when $u(z,x)$ is not strictly quasiconcave and/or $x + c(z)$ is not quasiconvex, the first-order conditions (1a) can have multiple solutions as noted above. Even if these solutions for z can be written in closed form, (18) will be a <u>correspondence</u> rather than a <u>function</u>. However, in this case, it can still happen that the inverse, equation (18), is a function rather than a correspondence. For this reason, it becomes more convenient to work with the inverse.

Suppose, for example, that

$$u(z, x) = a_0 z + (a_1/2) z^2 + \ln x \tag{20}$$

$$c(z) = b_0 z + (b_1/2) z^2. \tag{21}$$

Then equation (1a) becomes

$$b_0 + b_1 z = (a_0 + a_1 z)(y - b_0 z - \frac{b_1}{2} z^2)$$

$$= a_0 y + a_1 zy - a_0 b_0 z - \left(\frac{a_0 b_1}{2} + a_1 b_0\right) z^2 - \frac{a_1 b_1}{2} z^3. \quad (22)$$

Because (22) is a third-degree polynomial in z, it has up to three roots--i.e., the solution in the form of (18) is a correspondence. However, the inverse demand (19) is a single-valued function and takes the form[2]

$$y = \psi(z) = \frac{b_0 + (b_1 + a_0 b_0) z + \left(\frac{a_0 b_1}{2} + a_1 b_0\right) z^2 + \frac{a_1 b_1}{2} z^3}{(a_0 + a_1 z)}. \quad (23)$$

Therefore, the good news is that I now believe in the inverse demand function. The bad news is that I do not believe it is necessarily a polynomial as Rob assumes. The right-hand side of equation (23) is not a polynomial, although it would become one if $a_1 = 0$. More generally, given that c(z) is a polynomial and u(z, x) has the form of (15), $y = \psi(z)$ will be a polynomial in z only if h(z) in (15) is a polynomial of the first degree.

To summarize, Rob makes three assumptions: (i) c(z) is a polynomial in z, (ii) (u_z/u_x) is a polynomial in z, and (iii) $\psi(z)$ is a polynomial in z. On pages 7 through 9, I have argued that assumptions (i) and (ii) are very restrictive and rather implausible. Here I am pointing out that (i) and (ii)

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[2]Let me emphasize that this is just one example. There can be cases in which $z = \phi(y; s)$ and $y = \psi(z; s)$ are both correspondences. I am not asserting that $\psi(\cdot)$ is always a function.

do not necessarily imply (iii), and, therefore, assuming (iii) is even more restrictive.

With this out of the way, I now turn to the main question: What is Rob trying to prove, and is his proof correct?

IV. Rob's theorems

I think Rob's two papers, "Single Markets . . ." and "Nonlinearity . . .," can be treated as a single unit. I will focus on the case in which z is a scalar because it reveals his argument the most clearly.

Rob's question. *Suppose you estimate separately the hedonic price equation, c(z), and the inverse demand function, $y = \psi(z; s)$. Can you then use the first-order condition (1a) to recover the underlying marginal rate of substitution function, u_z/u_x?*

Before discussing Rob's answer, let me explain why I find this to be a peculiar question. Rob's premise is that the first-order condition (1a) is not exploited when one estimates the inverse demand function; it comes into play only after $y = \psi(z; s)$ has been estimated. I cannot see why one would want to do this. As I mentioned last summer, to me the logical procedure is to exploit the first-order condition when estimating the (inverse) demand function. Specifically, I proposed that one estimate the hedonic price equation

$$y - x = c(z), \tag{1b}$$

and then postulate a functional form for u(z, x; s) and set up the first-order conditions using this c(z),

$$c_z(z) = \frac{u_z[z, y - c(z); s]}{u_x[z, y - c(z); s]}. \tag{1a}$$

If (1a) can be solved for $z = \phi(y;\, s)$ or $y = \psi(z;\, s)$ as closed-form functions, one estimates either of these functions; otherwise, one estimates (1a). [In fact, (1a) and (1b) should be estimated simultaneously although this might involve an iterative process.]

<u>Rob's answer</u> is given in the following two theorems:

THEOREM I. If no restrictions are imposed on the specific functional forms of $c(z)$, $y = \psi(z;\, s)$ and u_z/u_x, then, in general, one cannot use (1a) to recover u_z/u_x from known $c(z)$ and $y = \psi(z;\, s)$.

THEOREM II. If $c(z)$, $y = \phi(z;\, s)$ and u_z/u_x are assumed to be polynomials in z, then, under certain conditions having to do with the relative degrees of polynomials, it will be possible to use (1a) to recover u_z/u_x from known $c(z)$ and $y = \psi(z;\, s)$. If these conditions are not met, however, it is not possible to recover u_z/u_x.

This statement of Theorem I is my understanding of what Rob says on pages 4 through 6 of his "Single Market . . ." paper. The statement of Theorem II (which omits an explicit account of Rob's conditions) is my understanding of what he says on pages 6 through 8 of his "Single Market . . ." paper and on pages 2 through 6 of his "Nonlinearity . . ." paper. I have omitted reference to what he says about the supply side of the characteristics market because, in my opinion, it completely parallels what he says about the demand side and adds nothing extra.

I will now discuss each theorem in turn.

Theorem I. From equation (1a) we have

$$c_z(z) = \frac{u_z[z, y - c(z); s]}{u_x[z, y - c(z); s]} \equiv F(z, y; s). \tag{24}$$

Let $F^{-1}(z, \cdot; s)$ be the inverse of $F(z, y; s)$ with respect to y, i.e., for any a

$$F[z, F^{-1}(z, a; s); s] \equiv a. \tag{25}$$

Then, (24) can be solved for

$$y = \psi(z, s) = F^{-1}[z, c_z(z); s]. \tag{26}$$

Equation (26) shows how the function $\psi(z)$ is related to the functions $c_z(z)$ and $F^{-1}(\cdot)$; therefore, it imposes substantive restrictions on the form and coefficients of the function $\psi(z)$.

Rob's proof of Theorem I. In view of equation (25), we can write

$$c_z(z) \equiv F\Big(z, F^{-1}[z, c_z(z), s]; s\Big) \tag{25'}$$

$$\equiv F[z, \psi(z, s); s]. \tag{27}$$

Note that, while (25') is an identity that follows simply from the definition of the inverse function $F^{-1}(\cdot)$, equation (26) is an identity that follows from the behavioral relation (24). Differentiating (27) with respect to z, we obtain

$$c_{zz}(z) = F_1[z, \psi(z, s); s] + F_2[z, \psi(z, s); s]\, \psi_z(z, s), \tag{28}$$

where subscripts denote derivatives. Let $\gamma_1 \equiv F_1[z, \psi(z, s); s]$ and $\gamma_2 \equiv F_2[z, \psi(z, s); s]$. Then, equation (28) can be written

$$c_{zz}(z) = \gamma_1 + \gamma_2\ \psi_z(z,\ s). \qquad (28')$$

As pointed out earlier, Rob assumes that we have estimated c(z) and $\psi(z;\ s)$ from the data, and his question is whether or not we can recover F(•) from these two functions. Regarding γ_1 and γ_2 as a pair of scalars, Rob argues that our information about $c_{zz}(z)$ and $\psi_z(z,\ s)$ is not sufficient to permit us to recover γ_1 and γ_2 from equation (28'). In effect, (28') gives us one equation in two unknowns, γ_1 and γ_2.

As I understand it, this is what is meant by Rob's claims that, in general, the structural equation [i.e., F(•)] "cannot be estimated from a single market if only a single price is observed" and "single market data by itself does not contain enough information to reveal the price parameters of unknown (but well-behaved) structural equations."

However, I do not find this proof convincing. It "works" only because Rob treats γ_1 and γ_2 as scalars and ignores the information about the structure of functional forms contained in (26). If you look at (28), rather than (28'), and think in terms of recovering functions $F_1(\cdot)$ and $F_2(\cdot)$, rather than scalars γ_1 and γ_2, his argument does necessarily carry through: while there is an infinity of pairs of scalars γ_1 and γ_2 that would satisfy (28'), there is not necessarily an infinity of functions $F_1(z,\ \psi;\ s)$ and $F_2(z,\ \psi;\ s)$, given $\psi(z,\ s)$, that would satisfy (28). More generally, I would argue that, if you know c(z) and you take the special structure on the right-hand side of (26) into account when you estimate $\psi(z,\ s)$, then, in general, you should be able to recover $F^{-1}(\cdot)$ and, from this, F(•).

Therefore, I do not believe that Theorem I is correct. To the contrary, I believe that, in general, if you know c(z) and exploit equation (26) when you estimate $\psi(z, s)$, you can recover F(•).

To be sure, another part of Rob's paper can be read as agreeing with my assertion. On page 8 of his "Single Market . . ." paper, Rob writes "If the exact functional form of the underlying structural equation is known, non-linearity in the price gradient can lead to unique parameterization" (my italics). However, on the next page, he cautions against "assuming particular functional forms for structural equations."

I do not know how you would interpret this. To me, it seems that Rob is saying that it is all right to assume a particular functional form for c(z) and $\psi(z)$--say, polynomials of some order--but it is not all right to assume a particular form for u(z, x) or F(z, y; s). I do not want to enter into a long debate about what we mean by "knowledge" here, but I think that Rob is being inconsistent.

Theorem II. Rob's intention here is to generalize Brown and Rosen's result to a larger class of polynomials. In their model,

$$c(z) = b_0 z + \frac{b_1}{2} z^2 \tag{29a}$$

$$c_z(z) = b_0 + b_1 z \tag{29b}$$

$$F(z, y; s) = f_0 + f_1 z + f_2 y. \tag{30}$$

On pages 7, 8, and 9 above, I raised the general question of whether or not it is plausible that (29) and (30) could be polynomials. Here, I wish to raise another question: Given (29a), is it plausible that (30) could be a first-order polynomial? Suppose that u(z, x) is a first-order polynomial in z

$$u(z, x) = a_0 z + \ln x. \tag{31}$$

This generates

$$F(z, y; s) \equiv \frac{u_z[x, y - c(z); s]}{u_x[x, y - c(z); s]} = a_0\left(y - b_0 z - \frac{b_1}{2} z^2\right)$$

$$= a_0 y - a_0 b_0 z - a_0 \frac{b_1}{2} z^2, \tag{32}$$

which is a second-order polynomial in z. Observe that Brown and Rosen's argument about nonidentifiability would not apply if (32) were combined with (29). In short, although I cannot prove that it could never happen, I cannot think of any utility function which, when combined with (29), generates a formula for F(z, y; s) that is a polynomial of less than the second order. More generally, to use Rob's notation [see equations (33) and (34) below], if I is the highest power of z appearing in c(z) and (J - 1) is the highest power of z appearing in F(z, y; s), it seems likely that $J - 1 \geq I$.

The problem is that Brown and Rosen--and now Rob--ignore the utility-theoretic derivation of F(z, y; s) and treat it as an arbitrary function; they overlook the fact that it contains c(z) as one of its arguments.

This makes it difficult to decide how much weight to place on Brown and Rosen's result or on Rob's generalization of it. I certainly agree that, if equations (29) and (30) were a valid model, it would be impossible to identify f_0, f_1, and f_2 since one has

$$y = \phi(y;\ s) = \frac{f_0 - b_0}{b_1 - f_1} + \frac{f_2}{b_1 - f_1} y$$

$$y = \psi(z;\ s) = \frac{b_0 - f_0}{f_2} + \frac{b_1 - f_1}{f_2} z.$$

Similarly, if one accepts Rob's model as valid, something like his result is certainly correct. However, I have some problem with the details of his theorem. His model is

$$c(z) = \sum_{i=0}^{I} c_i z^i$$
$$c_z(z) = \sum_{i=1}^{I} c_i z^{i-1} \tag{33}$$

$$F(z,\ y) = \sum_{j=1}^{J} \sum_{k=1}^{K} f_{jk} Z^{j-1} Y^{k-1} = \sum_{j=1}^{J} Z^{j-1} \left(\sum_{k=1}^{K} f_{jk} Y^{k-1} \right). \tag{34}$$

My problem is with the notation on the right-hand side of equation (34). Consider the case in which J = K = 2 so that JK = 4:

$$F(z,\ y) = f_{11} + f_{12} Y + f_{21} z + f_{22} ZY. \tag{35}$$

Suppose that $f_{22} = 0$ while $f_{11} \neq 0$, $f_{12} \neq 0$, $f_{21} \neq 0$ so that

$$F(z,\ y) = f_{11} + f_{12} Y + f_{21} z. \tag{36}$$

I would refer to this equation as a special example of JK = 4. However, when Rob presents equation (36) on page 5 of his "Nonlinearity . . ." paper, he refers to it as a case in which JK = 3. *It is true that there are only three* coefficients in equation (36), but the highest powers of Z and Y involved are

$Z^{(2-1)}$ and $Y^{(2-1)}$, i.e., J = 2, K = 2, and JK = 4. If J and K are integers, JK = 3 implies either J = 1 and K = 3 or J = 3 and K = 1; in neither case can (34) generate (36).

The problem is that Rob appears to be inconsistent in his notation. In (34), he implicitly treats J and K as separate, integer-valued powers of Z and Y, whereas he later treats the product JK as the number of nonzero f_{jk}'s.

Accordingly, it seems to me that Rob needs to clear up this inconsistency and restate his Theorem II, viz., that the coefficients of F(z, y) can be identified uniquely only if $I \geq JK$. I have no doubt that some suitably restated version of his theorem is correct.

I want to emphasize how restrictive are the assumptions underlying that theorem: (i) unless u(z, y) has the form of (14) or (15), it is unlikely that F(z, y) is a polynomial; (ii) even if F(z, y) is a polynomial, it is likely that $J - 1 \geq I$ and F(·) will be identified; (iii) unless you impose restrictions that $f_{jk} = 0$ for certain j, k, j > 1 and k > 1, the polynomials in (33) and (34) will not even generate a polynomial for $y = \psi(z)$.

V. Conclusions

1. Both Brown and Rosen and Rob ignore the utility-theoretic foundations of their basic equation

$$c_z(z) = F(z, y).$$

In particular, they do not recognize that it should conform to equation (1a).

2. Rob's proof of his Theorem I that, in general, F(·) cannot be recovered from c(z) and $\psi(z)$ is defective. To the contrary, in general, F(·) can be recovered.

3. As presently restated, Rob's more specialized Theorem II seems to be wrong, but a restated version of the theorem could well be correct. However, the assumptions of the theorem are so restrictive or, indeed, implausible, that it should be of little interest. It is unlikely (although not impossible) that c(z) and F(z, y) are polynomials in Z. It is by no means certain that $\psi(z)$ is a function (as opposed to a correspondence). Even if c(z) and F(z, y) are polynomials and $\psi(z)$ is a function, it requires strong restrictions to ensure that $\psi(z)$ is a polynomial.

4. Rob's theorem II ignores the utility-theoretic restrictions on F(z, y) and its relationship to c(z). When these are recognized, I have been unable to construct an actual example in which the nonidentification problem arises. (Brown and Rosen's example, which Rob seeks to generalize, does not appear to be compatible with utility theory.)