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SP-4P: RESOURCE MANAGEMENT ISSUES

DISCUSSANT: STEPHEN F. HAMILTON

**Empirical Analysis of Pastoral Migration Decisions:
Gabra Herders in Northern Kenya**

John McPeak

The paper addresses the land use decisions of pastoral herders in Sub-Saharan Africa. Some major findings are that food aid creates an incentive for more intensive grazing in zones around village distribution points, that the time spent near villages decreases with herd size, and that the proportion of time spent near town increases with rainfall. This latter result is taken to indicate that traditional migration patterns, which involve increased production around village wells during drought years, has been disrupted. Overall, the paper makes a clever use of available data and attempts to answer an interesting question.

There are several issues to discuss. First, I am unclear on the relationship identified in the introduction between changing pastoral migration patterns and common property problems. These seem to be separate issues. Certainly, the sub-optimality of a common property resource does not depend on the distribution of animals between regions nor, for that matter, does it require that the carrying capacity within a region remain fixed. Rather, the common property problem indicates a failure by individual producers to recognize shadow values that arise in future production periods, which leads to entry of additional animals until contemporaneous benefits are exhausted (i.e. through overgrazing). With variable climatic conditions, it seems entirely plausible for current benefits to be exhausted in each period through a program of variable stocking levels

over time. Thus, the common property problem does not go away unless one constructs a dynamic model and subjects it to rather restrictive assumptions that constrain the biological growth rate of the population. To formally address the common property problem, one would need to appropriately define a steady state optimal stocking level and show that biological lags in the herd rebuilding process following drought seasons precludes over-stocking. Nonetheless, I enjoyed the paper, as changes in migration patterns are interesting in their own right and certainly have an effect on grazing pressure in local regions (regardless of the relationship with common property issues).

In the theoretical model, each herder maximizes the utility of consumption, where consumption depends on revenue from milk sales, the fraction of milk consumed at home, and food aid, where home consumption is taken to be the numeraire commodity. Consumption is then substituted into the utility function, which formally makes the appropriate utility measure that of indirect utility, not the direct utility measure pursued in the paper. Accordingly, the food aid variable should be expressed in *value* terms.

In the empirical section, the system of equations (6) is estimated recursively. There are a few potential problems here that merit discussion. First, I am unclear how data is generated on the share of milk sold. Data certainly exists on the *amount* of milk sold, but how does one know total milk production when a significant fraction of the milk is consumed. Was this asked in a survey (if so, it would help to provide it in an appendix)? Second, a related point is that there is likely to be simultaneity bias in the estimation equations. For example, the error components in the milk production and share sold equations are, at least potentially, correlated, as random factors that reduce

milk production are also likely to decrease the share of milk sold for subsistence reasons.¹

Third, I am not entirely convinced that the censoring limits chosen for the Tobit model are appropriate. The censoring limit is derived from an observation that herders never send more than 50% of their labor force to a satellite camp, but it is not clear from the discussion on page 7 why a satellite camp cannot be in zone one if the base camp is in zone two. Is it possible that, when one camp exists in each zone, the camp in zone one is incorrectly identified in the data as the base camp, when it is in fact the satellite camp? If so, how sensitive are your results to the choice of censoring limit? Fourth, the divergence of results between the models with and without fixed effects is disconcerting. This may be due either to simultaneity problems or, more likely, to multicollinearity, which leads the discussion to my final point. The multicollinearity problem is potentially very severe, as the explained portion of tzl^* is a function of the same variables (pasture availability, herd size, and herd characteristics) that are used to explain mp^* and s^* .

Overall, the paper addresses an interesting problem, uses a clever methodology, and presents several, potentially important results.

Optimal Management of Multi-Value Renewable Resources: An Application to the African Elephant

William R. Sutton and Lovell S. Jarvis

This paper presents a dynamic management model for African Elephants. Unique features of the model include the analysis of external effects created by the elephant population on agro-pastoralists and management expenses incurred by the park service.

¹ To correct for simultaneity bias in a Tobit model, see Pagan and Vella (*J. Applied Econometrics*, 1989) and Vella (*Int. Econ. Rev.*, 1993).

In the model, land is either allocated to wildlife reserve or to agricultural production and serves as a constraint on the control variable of 'land allocation'. Land allocated to wildlife reserve influences the growth function of the elephant population, which completes the dynamic specification of the problem. In the growth function, F , the total population of elephants, x , has either a positive or negative effect, $F_x > \text{or} < 0$, a feature that presumably depends on whether or not the population exceeds the maximum sustainable yield (technically $F_x = 0$ is also admissible). However, this ambiguity of sign is inconsistent with the assumption that $F_{xW} > 0$. That is, if $F_x < 0$, then an increase in land allocated to elephant habitat, W , increases the marginal growth rate if and only if the cross-partial satisfies $F_{xW} < 0$ (not > 0).

The model is de-composed into two separate problems: that of the park service and that of agro-pastoralists. In the park services objective function, the nature of harvest revenues deserves some discussion, as elephants will tend to leave their ivory behind when they die. This leads me to believe that there may be a missing component in the park services' problem and, later, in the social planning problem. A uniquely interesting feature about elephant herd management is that consumptive use benefits stemming from the harvest of ivory do not necessarily require the physical culling of elephants from the stock. Certainly, the harvest of an elephant by the park service brings in revenue, but, then, so does a non-harvest program with *postmortem* collection of ivory. In this light, the maximum sustainable yield may coincide with zero harvest at the maximum sustainable stock, a feature I have never seen in a dynamic model but which has interesting conceptual possibilities. Also, in the sub optimization problem, the fact (noted in footnote 3) that poaching is not a function of the ivory price due to a low

opportunity cost of hunters, does not imply that poaching effort is not responsive to price. Rather, it indicates that a potentially interesting threshold effect may exist.

The agro-pastoralist's problem is a nice complement to that of the park service, and their surplus measure is incorporated in the social planning problem. A feature of the agro-pastoralist problem that is not explored, and which relates to the idea of a threshold price effect on the opportunity cost of poaching, is that it may be possible to endogenize the decision to poach. That is, the level of poaching effort may be inversely-related to the profitability of agro-pastoral production; if crop production is not profitable due to high elephant populations and external damages, then the opportunity cost of poaching decreases (and vice versa). This, in turn, would effect the price responsiveness of poachers in the ivory market through the threshold effect.

The results of the model are potentially interesting, although space considerations presumable did not allow the derivations to be presented. I am not certain how the results are effected by the previously noted sign error on the F_{xW} term, but $F_x < 0$ is assumed. Also, the choice of the region $F_x < 0$ seems somewhat inconsistent with the statement in the introduction that the elephant population was halved in the 1980's. With a constant land allocation, the symmetric growth function specified on page 5 implies that a halving of the population from any point below the carrying capacity is sufficient for $F_x > 0$. Conversely, if the land allocation variable is free, then the carrying capacity varies with W , which implies that the sign of the growth function at a given point depends on the equilibrium value of W^* . In either event, this restriction should be relaxed to allow for other cases.

Overall, the model is creative and interesting. With minor modifications, extending the analysis in an empirical direction seems both productive and worthwhile.

**An Analysis of the Economic Efficiency of Thoroughbred
Breeder/Owner Incentive Policies**

J. Shannon Neibergs and Richard Thalheimer

This paper addresses the effectiveness of alternative thoroughbred breeder/owner incentives to promote horse breeding and ownership. The major finding is that, while the current state-administered programs have a positive effect on breeding and ownership, efficiency gains could be realized by reallocating funds to open purses. Essentially, this result indicates that a market-based approach is more effective at raising breeder revenue than a policy of direct breeder subsidization.

The title and introduction seem to unhinge from the remainder of the analysis: certainly, agricultural subsidies are known to be economically inefficient. A competitive market without externalities allocates resources efficiently and a subsidy, whether allocated as a direct payment to breeders or in the form of subsidized purses, serves only to disrupt market efficiency. Although the argument is made in the introduction that incentive programs promote the preservation of green space, an external benefit that might conceivably make a subsidy efficient, the focus of the paper is on the effectiveness of breeder incentive programs on increasing breeder revenue. It is possible, of course, that increasing breeder revenue is economically inefficient. Consequently, I find that the paper does not address economic efficiency in the classic sense of a first-best economic optimum, but considers efficiency in the second-best context of achieving a desired revenue (or supply) target at minimum cost. Still, the problem is interesting.

The model is conveniently estimated due to the recursion of supply and demand equations (1), (2), and (3), which leads to elegant empirical analysis. A few points of discussion are necessary to qualify this recursion. First, it is not particularly clear in the paper how expectations are revised. The supply of registered foals is taken as a function of lagged yearling prices, breeder awards, and stallion awards. What does this imply about the formation of future price expectations? Do producers act myopically? The question is important, and should be noted in the text, because the recursive nature of the model depends on the manner in which expectations are formed. If breeders have rational expectations, expected prices might follow an autoregressive process, in which case price effects are persistent and the model, at least potentially, is no longer recursive. Second, in the foal to yearling transfer equation, it is noted that death loss is negligible, so that each foal becomes a yearling in the subsequent period. It is natural to ask how sensitive the subsequent results are to changes in the (constant) rate of death? A few auxiliary regressions could be run to determine whether the estimates in the supply and demand equations are robust to non-zero death rates.

The model is estimated using panel data from a three-state sample. Not surprisingly, the point elasticities are consistent with those estimated by Neibergs and Thalheimer (1997). In the long-run model, the supply equation (1) is substituted into demand equation (3), which results in a first-order difference equation either in P or in $RFOAL$. Long-run supply and demand elasticities are then calculated at the steady-state equilibrium point using the estimated parameter values. The implication is an interesting one: allocating funds to open purses both shifts supply and increases price, while breeder

subsidies shift only the supply function and therefore lower prices. Consequently, breeder revenue increases following a transfer of revenue from subsidies to open purses.

The Dynamics of Reintroducing, Supplementing and Controlling Endangered Predator Populations

David Rondeau

This paper presents a dynamic model that addresses potential steady-state equilibria associated with the reintroduction of endangered predator populations. Multiple equilibria are shown to exist and the properties are characterized. The problem is an important one, particularly given the recent conflicts associated with the reintroduction of wolves in Yellowstone and the Adirondacks. The research question is natural: should Bubu get another picnic basket?

The model, at first blush, captures the essential elements of the problem: the predator population creates a stream of (discounted) nonconsumptive benefits and creates social damage. Harvest of the predator population, which is allowed to take positive or negative values, is the control variable that allows human activities to adjust the level of the stock. Accordingly, the benefits from harvesting the population turn to costs when the harvest rate is negative (does this imply that a spot market exists for purchase of predators?). However, a potentially missing component of the model is the interaction between the predator population and its target prey. While it is natural to ignore features that needlessly complicate the model, I find this to be an important omission. The existence of a predator species has a stabilizing effect on prey populations, which can eliminate the need to selectively cull deer, rabbit, and other prey populations (adding a

predator in the food chain also tends to increase species diversity). Uncontrolled growth of these populations also causes economic damage and may necessitate “pest” control.

The technique used to discriminate between dominated programs at different initial stock and harvest levels is intriguing. Given the external damages caused by the predator species are not bounded in the model, parameterizations can be found in which it is never beneficial to reintroduce the species: marginal external cost may always exceed marginal benefit. A standard specification of concave benefits and convex external costs allows the point to be made by assessing the net return on the first animal introduced: if marginal external damage from the first animal exceeds its marginal benefit, so must the net benefit of each additional animal be negative. In other circumstances, positive predator populations characterize the steady-state equilibria. In this case, the problem becomes one of choosing the desirable equilibrium from multiple candidate equilibria, where the globally optimal trajectory leads to different steady-state population values depending on the level of the initial stock. This leads naturally to the following question. Given the species is being reintroduced, it stands to reason that the initial stocking level can be *chosen* by the wildlife manager (i.e., $X(0) = X_0$ *free*, as opposed to a fixed initial stock). Conceptually, the fixed cost of reintroduction, which would depend on the initial stocking level, could then be specified as $C(X_0) = N(Y_0)$, where $-X_0 = Y_0 \geq Y_{min}$. This would allow policy implications to be drawn as to the extent of an optimal reintroduction plan under various circumstances of population and benefit/cost dynamics.

My last point of discussion is related to an earlier issue.¹ The model regards predator populations, yet there is no species to predate. Without modifying the model to include an additional state equation for a prey species, it is possible to interpret

nonconsumptive benefits in such a way as to include the control of prey populations. However, this is problematic, because X is expressed as a proportion of carrying capacity, which would then depend on the underlying prey population. Moreover, conflicts between predator species and human economic activities typically result from habitat encroachment. That is, without human intervention, a predator population may reach a carrying capacity that depends on various factors such as the population of its natural prey and the total land area. Provided the predator population is small relative to its natural prey, I would think that predators would provide an external benefit and not impose external costs on society. When natural food sources are abundant, Coyotes may prefer to dine on rabbits rather than raid chicken coops. The problem of a negative externality may arise only when the stock of predators exceeds the carrying capacity of the area for which it is intended. For example, if the rabbit population is hunted to a low level by a stock of Coyotes that exceeds the carrying capacity of the habitat, starvation in an unmanaged resource tends to reduce the population back to its natural limits. When human settlement overlaps with the natural habitat, it may be the case that Coyotes engorged on rabbits do not create external diseconomies, while hungry packs of Coyotes greatly impinge social well being. An element that might be considered is the idea of variable carrying capacity that occurs through overlapping human and predator habitats.

¹ With dynamic models, in particular, it is always easier to provide comments than to implement them.