Annotated Bibliography II

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## Introduction and Selection Criteria

In this annotated bibliography, I focus on the literature related to corporate branding, the development of artistic “superstars.” I have also researched various modern market structures (Spotify, Airbnb, and Uber) to gain a better anecdotal understanding of how reputation develops in the gig economy.

### The Economics of Superstars

Rosen S (1981). “The Economics of Superstars.” *The American Economic, Review*, *71*(5), 845-858. ISSN 0002-8282, <URL:, <https://www.jstor.org/stable/1803469>>.

1. This paper provides a model of how income distribution can be more concentrated in the few than the distribution of the observable underlying talent. He calls these high earners “superstars.” Rosen motivates his work by saying that many markets have superstars, and often obtaining relevant data is impossible, so between the prevalence and lack of empirics, models must be used.
2. Rosen positions his analysis as bridging the gap between the superstar effect and traditional economic theory that says that “more talent means more pay.” Rosen agrees with that sentiment, but Rosen tries to explain why talented shoemakers don’t become superstars, but musicians can (Bronnenberg et al., 2019, p. 845). Rosen uses flexible personal market size (so product differentiation) which he claims stands out from traditional perfect competition theory.
3. There is no data used in this paper.
4. The methodology, I have learned, is a fairly standard approach to finding market equilibrium. Rosen first solves the consumer’s utility maximization problem taking as given firm behavior, then Rosen solves the firm’s profit maximization problem taking as given consumer behavior. Then Rosen finds the intersection of the quantity demanded and quantity supplied and this becomes the market equilibrium. From there, Rosen describes the characteristics of this equilibrium and discusses some comparative statics.  
     
   In a bit more detail, the consumer gets to choose a quality of final good, and the number of goods of that quality they consume (they don’t consume multiple qualities). Quality and quantity are assumed to be substitutal according to where is the number of units consumed at quality . This convenient functional form assumption implicitly defines the price function (which is a function of quality) based on a simple differential equation. TI believe that this says that the market is large enough where neither the consumer nor the producer has price setting ability, but I’m honestly not sure.  
     
   From there, the producer chooses a quantity to produce taking as given their underlying ability,so the firm’s decision is one dimensional. The firm has a convex cost function in their quantity produced. I will leave the explanation there because those are the most important parts to the analysis I intend to conduct.
5. One main result is the convexity of the revenue function in the artist’s underlying talent. With this convexity, small increases in talent lead to disproportionately large increases in revenue. This is the justification that Rosen uses to explain how this leads to salary concentration and superstars.
6. Opportunities for further research (which I intend to explore) include moving the analysis to a multiperiod model to see if salary concentration can be maintained over time. A multiperiod model should include a decoupling of the artist’s underlying talent and their present reputation, and Rosen’s paper does not do this. Rosen assumes that talent is perfectly inferrable (though not directly observable) through the artist’s production choices. The final element that could be added to Rosen’s analysis is a stochastic production function. This is one way to obfuscate the talent of the artist from their current reputation. Artistic “talent” is a fickle thing, so adding a reputational element is important. One element that I think that Rosen should have clarified is the importance of a convex cost function on his results. It seems as though if costs are not treated as convex, then the convexity of revenue falls.

### The Economics of Rising Stars

MacDonald GM (1988). “The Economics of Rising Stars.” *The American, Economic Review*, *78*(1), 155-166. ISSN 0002-8282, <URL:, <https://www.jstor.org/stable/1814704>>.

1. This paper is overtly derivative from (Rosen, 1981). It models artist’s wages based on underlying talent. This paper differs from Rosen by incorporating multiple periods (two) and uncertainty about the talent of each artist.
2. The work of (Rosen, 1981) is a “hedonic” model—a model in which preference over idiosyncratic elements of a good affect its utility. MacDonald explains that these hedonic models allow for right skewed income distribution, but do not require them (say if the talent is sufficiently skew left). MacDonald’ frames his paper’s deepends our understanding of how the distribution of talent affects revenue outcomes. In particular, he introduces uncertainty (by both consumers and producers) about underlying talent to see how this affects revenue. It also provides a short implementation of reputation development over two periods, new to the “Superstar” literature.
3. There is no data used in this paper.
4. Similar to Rosen, this is a market equilibrium model/. Both consumers and producers (artists) are assumed to not know the talent of a given artist. In each of two periods, the artist performs which can either be good or bad. The probability of a good performance is increasing in the artist’s talent. As such, in period one, no one has information about the artist, so everyone is of the same type and receives the same wage, however after performing once, the artist can be new to the market (quality fully unknown), good type (having one good performance), or bad type (having one bad performance). Further, artists can leave the market after period one. The model then looks at expected profit and utility maximization for consumers and artists and finds the resulting equilibrium ticket price for untested, good, and bad performers. The consumer maxmizes over their choice of whether or not to attend a performan e of a certain type (given their ticket price). The artist maximizes over whether or not to perform or take an outside option given the induced ticket price and number of tickets sold.  
     
   The interesting part of the model comes in the analysis of the equilibrium wage and the composition of sellers in period two (how many artists left and how many entered).
5. MacDonald finds that on the consumer side, behavior is separated into three categories based on the difference in utility between good and bad performances.People who don’t see much difference between good and bad performances don’t attend performances at all. In the middle, consumers choose to buy tickets from unknown performers in period one. The most “discerning” consumers wait until period two and only take the higher ticket price for performers with a good track record. On the artists’ side, MacDonald finds that artists are willing to accept lower returns than an outside option in period one while hoping for a good performance and higher wages in period 2. The most critical assumption for this to hold is the general form of the model. The binary outcome of the performance is really central. While the author acknowledges that extensions could address a more nuanced performance outcome space (by varying the repertoire of the performer), the effects of haiving only good or bad outcomes is not clear.
6. Overall, I believe that this is a well executed paper, but the one critique that I would pose is that extending the analysis to more than two period would have been valuable. This paper allows for a non-binary talent distribution, but does not leverage it. More periods would allow consumers to better separate higher talent artists from lower talent artists. As a final note, this paper’s section analysis of deviations from the steady state may prove useful in my work.

### Peer-to-Peer Markets

Einav L, Farronato C, Levin J (2016). “Peer-to-Peer Markets.” *Annual, Review of Economics*, *8*(1), 615-635. doi:, 10.1146/annurev-economics-080315-015334 (URL:, <https://doi.org/10.1146/annurev-economics-080315-015334>), <URL:, <https://doi.org/10.1146/annurev-economics-080315-015334>>.

1. This paper discusses and briefly models how peer-to-peer (P2P) markets differ from traditional markets. The author’s motivate their work using the emergence and dominance of digital P2P services like eBay, Uber, etc.
2. This paper puts P2P literature into three buckets: P2P’s ability to match buyers to sellers, flexible pricing strategies, and the big one to me, the explicit incorporation of trust and reputation. Their work tries to address primarily address the first two. They don’t explicitly point to other theoretical papers about P2P markets, making it seem like this paper provides a baseline model. Most of the work in this field seems to be experimental, so this paper provides some theory to back the modelling.
3. There is no data used in this paper.
4. One thing that I really like about this paper is the way that it identifies the key aspects of P2P markets (the buckets given above) that it wishes to include in a model before diving into the model itself. The model seeks to find how many P2P sellers enter the market. This model is intended to provide a comparison, and so the model that the author’s build uses traditional businesses as a baseline. For example, the model assumes that traditional businesses have non-zero startup costs (they must build a hotel), whereas P2P sellers have zero startup costs (you already have a house to rent out for Airbnb). However, marginal costs for P2P sellers is higher than traditional sellers. Another interesting feature of the model is exogenous stochastic demand functions. The artists simply assume that the demand curve takes one of a continuum of possible realizations.    
   After the model concludes, the authors discuss the implications of their model to legislation.
5. One of the conclusions is that random increases in demand benefit P2P sellers because they can enter the market freely and their higher marginal cost is compensated by the higher willingness to pay. The authors also find that there is an equilibrium where neither traditional nor P2P sellers wish to enter or exit the market. They also show that this equilibrium is unique and maximizes consumer surplus. The final conclusion from their model that I will point out is that markets with high fixed costs (they call frictions) like taxi licensing costs are ripe for P2P entrance.
6. This is a really nice paper that is fairly parsimonious and still gets at the deep insights of P2P markets. The only component they leave out is the development of reputations (which is unfortunately why I was interested in this paper). Incorporating reputation would have to substantially alter their proposed model, so I can see why it was excluded, but reputation plays an important role in modern P2P markets.

### Relationships between artistic movements and careers of modern artists: evidence from hedonic regressions with auction data

Hodgson DJ, Hellmanzik C (2019). “Relationships between Artistic, Movements and Careers of Modern Artists: Evidence from Hedonic, Regressions with Auction Data.” *Journal of Cultural Economics*,, *43*(2), 309-337. ISSN 1573-6997, doi: 10.1007/s10824-019-09343-6 (URL:, <https://doi.org/10.1007/s10824-019-09343-6>), <URL:, <https://doi.org/10.1007/s10824-019-09343-6>>.

1. This empirical paper explores how the characteristics of an artistic movement, and an artist’s time entering the movement combine to affect the selling value of art. This paper is motivated by a lack of hedonic regression using fine art auction data. That is, a regression incorporating artistic, historical, and physical elements of paintings to predict selling price. The authors recognize the importance of incorporating atypical measures when valuing art, and this paper provides a framework to do so. In addition to the different empirical strategy, the authors are interested in how the place of an artist in a movement (“early bloomer” or “late bloomer”) affects the value of their art over time.
2. The authors note that previous literature has centered around simply what artistic movement an artist comes from as a predictor of auction value over time. That is two artists from the same movement are treated to have similar effects. The authors then explain why artistic movements are important by discussing the sociological and pyschological implications of working with like-minded artist. Subsequently, the authors of this paper discuss art theorists believe that there may be heterogenous effects between early and late artists entering a movement. This paper is intended to test the hypothesis that affects are homogenous within movements. The observable measure of homogeneity in this paper (though not explcitly mentioned in the literature review) is the following. If affects are homogenous within movements, then the time at which prices peak for individuals should not differ substantially from when prices peak for an average of individual artists.
3. The data used in this paper comes from art auctions and was compiled in a different paper by a different author, Hellmanzik (2009,2010). Auctions occur between 1986 and 2010 and were listed on artvalue.com. The artists in the sample were born between 1985 and 1945. The authors sasy there are a total of “34,869 paintings sold at auction” (p. 314). There are 272 unique artists. The data includes the selling price converted to USD, and hedonistic controls like the auction house, size, “support”, medium, and whether or not the work is signed, (p. 134). The authors mention that they have no way of discerning whether or not a painting is auction multiple times. I’m not sure why this would be, as presumably the art has consistent name, author, and date of signing. The authors manually group artists into their respective movements. This is a necessity, as the data does not come coded by movement. Additionally, the authors manually decide which movements to include and which to exclude. The authors list multiple sources that helped them understand the history of art and classify artist’s accordingly. Of course the discretionary nature of their categorization opens the door for data problems, but it seems like they have done their due diligence. Finally, the authors categorize each movement as “umbrella” or “contemporeanous”. Umbrella meaning the term was applied retroactively, and vice versa.
4. The authors clame to use OLS, but as I will now argue they immediately don’t follow up on that. The first thing that the authors mention is that they use weighted least squares, so not OLS. The authors weight each observation by one over the square root of the number of times each artist is represented in the sample. The authors claim that this prevents artists who were more prolific from having undue influence on the results. Why is used is not mentioned explicitly. I find this weighting to be rather strange because the weights won’t sum to one. If there are artists each with items in the sample, the sum of the weights is , they should mention explicitly the normalization that occurs to allow the weights to sum to one. The authors also use White (1980) robust standard errors to control for heteroskedasicity. The authors present 6 models to allow them to test their hypothesis. For the sake of brevity, I will only discuss the thwo most relevant to the main conclusions of the paper. Model 4 has dummies for the artist who made the work, measure of size, dummies for medium, and other hedonic controls. The independent variables of interest are on the quadratic terms of the age of the artist when they produced a work. That is, we have a where is the age of the artist when a work is produced. They don’t justify why age should have a quadratic term, though the understanding would be that the value of an artist’s work might peak partway through their life, and not just increase as they age. Additionally, in this regression the authors force the coefficient s to be the same for any artist in the same group. This regression is essentially the null hypothesis that age effects are homogenous within movement. The alternative hypothesis in this regression is that age effects are heterogenous within movement. This is implemented by running essentially the same regression as before, but allowing the coefficient on the age terms to vary across artists. However, now there’s no controls at all for certain movements, so it seems like the authors overshot what they were looking for. One final econometric technique is the use of Chow-Wald tests which allow the authors to determine whether or not the coefficients on the age terms differ significantly between groups.
5. The author takes the estimates from the latter model (the one where age effects may vary by artist), and apply the Chow-Wald test by movement to see if the coefficients for age of every artist in a movement are the same. The authors find that in most cases, the coefficients for different artists in a movement are significantly different, indicating that there are heterogenous effects within movement. This is the main result of the paper, though I’m not totally sold on it. It seems rather obvious that different artists would have different valuations at different stages in their life. The authors also find that fixing the coefficients by group is also a useful predictor of art value. It seems to me that there is some degree of homogeneity within groups that is not worth ignoring.
6. I have a lot of critiques for this paper, but I will keep it brief. Most pressingly, the main model of the paper (with heterogeneous age affects by artist) has no controls for the movement that an artist is in. Including control for these would allow us to directly compare the contribution of movement to wages when allowing for age effects. Additionally, I would like to see the model estimated without weighting the regression to see if the results are robust to alternative weighting (or to see if the model can be made more parsimonious by getting rid of the weights altogether).

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