Information, Enforcement Costs and Cartel Stability:

An Empirical Investigation

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Abstract

This paper seeks to explain the significant variation in lifespans among industry cartels. I report two principal findings. First, industry cartels tend to be less stable, less recurrent and less likely to learn with experience than previous empirical research has suggested. The literature also has tended to obscure the relation between cartel objectives, organization and stability. Second, using a survival analysis, cartel stability is shown to vary predictably and significantly with enforcement costs, as reflected in price variability, cartel market share, business cycle timing in several markets, and the existence of parallel agreements among rivals. Finally, cartel stability declines with both tenure and repeat experience. Overall, the results generally support an information–based theory of cartel stability first proposed by George Stigler (1964).

I. Introduction

The history of collusive agreements ranges from industry cartels that survive for decades to short–lived agreements undermined by repeated price wars and frequent entry. This paper strives to explain variation in cartel lifespans empirically and, in the process, it reassesses several important stylized facts concerning the stability of collusion. George Stigler (1964) first identified the policing of collusive agreements as a problem in the economics of information. When cartels cannot monitor firms' strategies directly they must rely on indirect, probabilistic information to detect and punish cheating. It is the combination of imperfect information and industry uncertainty that threatens cartel stability by raising enforcement costs. Stigler linked enforcement costs to specific product, buyer and seller characteristics, and recent game theoretic extensions have explored the influence of business cycle timing and opportunities for multi–market collusion.[1]

While these theories offer sharp predictions, empirical tests have provided relatively weak and often mixed support for several core hypotheses. Disappointing empirical results may be attributed in part to researchers' choice of cartel samples: prosecuted price–fixing agreements and international cartels.[2] Both samples introduce selection and measurement error problems that bias and limit the power of empirical tests. First, prosecuted price–fixing conspiracies tend to over–estimate cartel enforcement costs because of collusive failures' greater visibility to antitrust authorities.[3] International cartels introduce the opposite selection bias, as governments frequently subsidize enforcement costs by making agreements legally binding or through direct

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1 See Green and Porter (1984), Rotemberg and Saloner (1986), Telser (1980) and Bernheim and Whinston (1990).

2 Posner (1970), Hay and Kelley (1974), Asch and Seneca (1976) and Fraas and Greer (1977) analyze prosecuted price–fixing cases to identify industry characteristics that are conducive to collusion. Suslow (1988) and Marquez (1992) analyze collusive stability directly in samples of international cartel agreements, and Jacquemin *et.al.* (1981) analyze Japanese export cartel stability. These seven studies offer mixed conclusions about the relationships between cartel stability (or frequency) and demand growth and variability, industry concentration, cartel membership and industry coverage.

3 Hay and Kelley (1974, 26) and Posner (1970, 410) find that prosecution rates are higher for conspiracies in unconcentrated industries and for those with multiple participants. Economic theory predicts that both characteristics also will tend to raise cartel enforcement costs.

government participation.[4] These opposing selection biases may partially account for the literature's mixed empirical findings. Second, in the absence of complete times series to date cartel episodes, samples of prosecuted price–fixing and international cartels frequently have mistaken intermittent episodes as uninterrupted collusion. Measurement error in cartel dating may partially explain the generally weak statistical significance of previous empirical tests.[5]

Departing from previous approaches, this paper re–examines the determinants of cartel stability and finds stronger support for an information–based theory of enforcement. In the process, the paper also modifies several stylized facts from the prior cartel literature. I exploit an unusual data set of legal, non–government–enforced cartel agreements established under the *Webb–Pomerene Export Trade Act of 1918*. Export cartels receive antitrust immunity to coordinate export pricing, set quotas, designate exclusive trading areas, pool sales information, and centralize marketing and distribution. While they are exempt from antitrust scrutiny, *Webb–Pomerene* cartels are neither enforceable in court nor directly government–assisted. These legal, private agreements thus avoid the sample selection problems of prosecuted and government–assisted cartels. A complete chronology of *Webb–Pomerene* agreements also substantially lessens past measurement problems by identifying the beginning and ending years of active industry cooperation.

A final advantage of the *Webb–Pomerene* sample lies in its ability to expand analysis beyond classical price–fixing conspiracies, which have been the dominant focus of empirical research. Previously, I have found evidence that some *Webb–Pomerene* cartels did successfully restrict

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[4] The Japanese export cartel agreements analyzed by Jacquemin *et. al.* (1981) were usually extended through government fiat to non–cartel members. Thirty percent of Suslow's (1988) sample of international cartels were directly or indirectly government–assisted, and during her period of analysis "legal systems operated to enforce contractual agreements among cartel members" (Griffin (1989, 180)). Suslow's regressions confirm that cartel stability is positively correlated with government support. The effect is not always significant, however, which may reflect endogeneity.

[5] Measurement problems are clearly recognized by previous authors including Posner (1970, 399) and Suslow (1988, 13). Suslow (1988, 20, 29–30) finds strong evidence that lifespans tend to be overstated for international cartels classified as single–episode cartels.

industry exports to raise price, and were able to do so for extended periods of time.[6] In many other cases, however, *Webb–Pomerene* cartels have lowered price and expanded industry exports.[7] The fact that these cartels operated for an average of 15.5 years disputes the hypothesis that they were simply failed price–fixing agreements. Instead, these cartels' primary function appears to have been to co–ordinate joint investments in marketing and distribution on behalf of small exporters (see Dick (1992, 105–08) for additional evidence). While joint–investment cartels will not face entry threats if they do not earn super–normal returns, in general, they must still monitor against internal free–riding that would undermine joint–investment incentives.[8] It follows that the same characteristics that raise or lower enforcement costs for price–fixing cartels will guide stability among joint–investment cartels. For reasons discussed below, however, the two groups of cartels will tend to adopt different forms of internal organization, which in turn may affect their stability. By including both groups or cartels, the *Webb–Pomerene* sample can assist in identifying the relationship between cartels' objectives, organization and stability.[9]

Lastly, the *Webb–Pomerene* sample raises two important issues of methodology. First, a cartel's death may have very different interpretations depending on its primary objective. Dissolution of a price–fixing agreement indicates a failure to sustain price above the pre–cartel level or to contain defection. Dissolution of a joint–investment cartels may indicate an enforcement failure, or it may simply reflect natural evolution. Cartels that succeed in exploiting

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6 Specifically, Carbon Black Export, Inc. and Sulexco raised U.S. export prices for carbon black and crude sulphur by an average 6.5% in years that the cartels were active by restricting export volume by an average 14.6%. Carbon Black's longest uninterrupted run extended beyond 35 years, and Sulexco had runs of 13 and 29 years. For additional details, see Dick (1992).

7 Dick (1992) found that export cartels in canned milk, dried fruit, metal laths, milled rice and paperboard raised industry export volume by an average 59.5% in active years and lowered export costs (prices) by an average 11.5%. Finally, within the sample of 16 *Webb–Pomerene* cartels studied, Dick (1992) found that the remaining nine cartels were unsuccessful at sustaining export price or quantity effects.

8 The Courts have recognized *Webb–Pomerene* cartels' horizontal restraints as "essential to [a cartel's] stability and to preventing its members from taking individual selfish advantage of the knowledge and opportunities that have come to them as a group" (United States v. Minnesota Mining and Manufacturing Co. 92 F. Supp. 947, 965 (D. Mass. 1950)).

9 Suslow (1988) offers what appears to be the only other attempt to estimate the effect of internal organization on cartel stability. Suslow treats cartel organization as exogenous throughout her analysis.

scale economies in marketing and distribution will eventually outlive their original purpose as average member firm size grows. The survival methodology adopted in this paper allows for 'natural' deaths by letting cartels' hazard rate vary with tenure and prior experience. If joint–investment cartels' success erodes their rationale for operation, the probability of dissolution should depend positively on cartel tenure and the duration of past experience.

Second, beginning with Stigler (1964), the literature has focused primarily on enforcement of illegal collusive agreements. In this environment, colluding firms face a tradeoff since while superior information gathering and dissemination will assist cartel monitoring, it will also tend to raise the probability of antitrust detection. *Webb–Pomerene* cartels' antitrust exemption eliminates this tradeoff, and raises the question of whether Stigler's methodology of linking enforcement costs to cartel information remains appropriate. Stigler himself suggests that it does. He notes that even in the absence of absence of antitrust concerns, cartels generally will have to rely upon incomplete and potentially misleading information when monitoring members. Stigler (1964, 47) cites the example of open price associations, which were legal during some periods, and remarks that even "actual inspection of the accounting records of sellers ... gives only limited assurance that the price agreement is adhered to. Ultimately there is no substitute for obtaining the transaction price from buyers." Thus, even among *Webb–Pomerene* cartels that centralize all exports through a joint sales agent — approximately 60% of the sample — monitoring will tend to be imperfect and therefore costly.

I begin by examining the raw data for a sample of 111 cartel episodes spanning nearly a fifty–year period. I find that studies of prosecuted and government–assisted cartels have tended to overstate average cartel lifespan and the frequency and stability of repeated collusion. By ignoring differences in cartel objectives, the previous literature also obscures the relationship between internal cartel organization and stability. Next, I apply a survival approach to examine cartel dynamics. Following the theoretical literature, I specify a cartel's hazard rate as a function of its enforcement costs, which are related to buyer, seller and product characteristics, business cycle timing, and opportunities for multi–market collusion. Overall, the results generally support

an information–based theory of cartel stability as first proposed by Stigler (1964). Cartel stability varies predictably and significantly with price variability, cartel market share, business cycle timing in several markets, and the existence of parallel agreements among rivals. Finally, after controlling for these and other industry characteristics, cartel stability declines with both tenure and repeat experience.

The organization of the paper is as follows. Section II explains the *Webb–Pomerene* antitrust exemption, introduces the cartel sample, and re–interprets stylized facts concerning cartel stability. Section III develops a survival model, which is then applied in Section IV to test the information theory's predictions. Section IV also contrasts the *Webb–Pomerene* findings with conclusions drawn earlier from prosecuted and government–assisted cartels. The comparisons assist in identifying the separate effects of antitrust and industrial policy on cartel stability. A brief conclusion follows in Section V.

II. *Webb–Pomerene* Cartels in Export Trade

A. The Antitrust Exemption

The *Webb–Pomerene Export Trade Act of 1918* grants antitrust immunity to U.S. exporters to form industry cartels for overseas trade. The Act exempts cartels from the *Sherman Act*'s prohibition against contracts, combinations and conspiracies in restraint of trade, and from the *Clayton Act*'s Section 7 regulation of mergers that reduce competition. Similar exemptions exist in most other industrialized countries and, partially as a result, importing governments have generally chosen not to apply national antitrust laws to *Webb–Pomerene* cartels selling in their market (see, generally, Organization for Economic Cooperation and Development (1974)). Congress identified two primary motivations for granting antitrust immunity to export cartels. First, it was expected that "members would be able to offer and receive higher selling prices than they would were they forced to compete against each other for export sales" (U.S. Federal Trade Commission (1967, 6)). Second, small exporters could lower their selling costs by "spreading

overhead and eliminating duplicate sales organizations, or by obtaining lower rates on export services such as insurance and freight" (U.S. Federal Trade Commission (1967, 6)).

Webb–Pomerene associations receive antitrust immunity automatically upon filing with the Federal Trade Commission (FTC) the names and addresses of their members and, for incorporated associations, a copy of their articles of agreement. Failure to register within 30 days of formation exposes the cartel to standard antitrust oversight and penalties. To retain their registered status, cartels must satisfy extremely modest annual reporting requirements. The FTC sends a brief questionnaire each year to all registered cartels asking the value of their exports and their primary functions (Organization for Economic Cooperation and Development (1974, 15)). Cartel data for this study are drawn in part from an FTC summary of these annual filings that identifies when registered cartels were active. The Act's minimal compliance costs and its requirement that cartels register to receive antitrust immunity suggests that the *Webb–Pomerene* population should account for virtually all horizontal export agreements.

Table I summarizes *Webb–Pomerene* cartels' most common functions. Four–fifths of the agreements set a common price and/or allocated markets, and slightly more than one–half centralized export distribution through a joint foreign sales office. Other common cartel functions include market research, consolidation of freight and insurance charges, negotiation with foreign governments and international agencies, and promotional services. Cartels also are permitted to adopt a wide range of ancillary restraints to enforce export cooperation, including refusals to deal, restrictions on members' withdrawal from the cartel, issuing shares in a joint stock company to members, and adoption of exclusive dealing contracts (U.S. Federal Trade Commission (1967, 20)).

The *Webb–Pomerene Act* places few limitations on cartel activity, and the restrictions that do exist — while they might appear broad — have been narrowly interpreted and infrequently enforced (U.S. Federal Trade Commission (1967, 20–21)). First, the Act prohibits cartels from influencing domestic market prices. Both the FTC and the Courts have been extremely reluctant to enforce this provision, however, recognizing that it would undermine the exemption's primary

objective. There also is no evidence that firms feared that export cooperation would increase antitrust scrutiny of their domestic activities. Larson (1970, 499) notes that "the FTC has a history of lax administration of the Act" regarding domestic spillovers, and the courts explicitly have rejected arguments that *Webb–Pomerene* cartels are liable for ancillary restraints of domestic trade stemming from export cooperation.[10] Second, the Act prohibits cartels from directly constraining non–members' actions. By sanctioning exclusive dealing agreements that exclude sales by non–member firms, however, the Act effectively renders this second restriction inoperative. Third, the Act prohibits agreements between *Webb–Pomerene* cartels and foreign producers.[11] During the 47–year period that I study, the FTC investigated just twelve cartels for possible violation of this provision, and four of these investigations were ultimately suspended or dismissed (U.S. Federal Trade Commission (1967, 109)). Section IV tests whether parallel foreign agreements or domestic market factors influenced the stability of *Webb–Pomerene* export cartels.

Finally, while *Webb–Pomerene* agreements are exempt from antitrust prosecution, unlike many other export and international cartels they receive neither direct nor indirect government

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[10] Posner and Easterbrook (1981, 76) note that the "courts have permitted an entire industry to be organized into an export association despite any effect that competition (or lack of competition) in the world market may have on the domestic market." In United States v. Minnesota Mining and Manufacturing Co. (92 F. Supp. 947, 965 (D. Mass. 1950)), a case involving an abrasives cartel covering four–fifths of U.S. exports, the District Court concluded:

> "Now it may very well be that every successful export company does ... bring the members of the enterprise so closely together as to affect adversely the members' competition in domestic commerce. Thus every export company may be a restraint. But if there are only these inevitable consequences an export association is not an unlawful restraint. The Webb–Pomerene Act is an expression of Congressional will that such a restraint shall be permitted. And the courts are required to give as ungrudging support to the policy of the Webb–Pomerene as to the policy of the Sherman Act. Statutory eclecticism is not a proper judicial function."

[11] In an early advisory opinion, the FTC initially permitted *Webb–Pomerene* participation in international cartel agreements provided that "the action of this organization did not reflect unlawfully upon domestic conditions" (U.S. Federal Trade Commission (1967, 103–04)). This also was the position adopted by the courts until 1949 when international agreements were brought back under Sherman Act antitrust scrutiny. For additional discussion of this evolution, see Hoff (1958, 149–52).

assistance.[12] The agreements are not legally enforceable in U.S. courts and the federal government has never intervened to directly assist enforcement. *Webb–Pomerene* cartels must rely exclusively upon private self–enforcement. Penalties for violation typically range from reductions in assigned export market shares to expulsion from the cartel (Notz and Harvey (1921, 291–92)).

B. The Cartel Sample

An FTC review of *Webb–Pomerene* cartels released in 1967 summarizes information on all export cartels registered any time between 1918 and 1965. Based upon these cartels' annual filings, the FTC classifies a cartel as "active" in a particular year if it performed one or more of the functions listed in Table I for at least six consecutive months. (On average, about three–quarters of registered cartels were active in any given year.) This paper's analysis focuses exclusively upon active cartel episodes. Complete data on industry characteristics were available for a sample of 111 active cartel episodes covering 93 industries. A Kolmogorov–Smirnov test concludes that the sample is an unbiased drawing from the underlying population of 147 cartel episodes between 1918 and 1965. Consistent with the previous empirical literature, I treat a cartel episode as the basic unit of analysis, with its beginning and ending dates established by the FTC's active cartel definition. Recurrent efforts to collude within one industry thus are treated as distinct episodes, allowing me to analyze stability across repeat experiences.[13] Finally, while the FTC chronology ends in 1965 with 20 cartel agreements still in operation, the survival model developed in Section III is well suited to analyzing event histories with censored data.

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[12] Export cartel agreements have been legally enforceable to varying degrees in Germany and the Netherlands, and continue to be closely state–controlled in Japan (Organization for Economic Cooperation and Development (1974, 31, 49)). Footnote 4 characterized government support among international cartel samples.

[13] The validity of this approach is discussed later in this section, *infra*.

Table II summarizes life table data for the full cartel sample, and Figures 1 and 2 plot the estimated survival and integrated hazard functions.[14] *Webb–Pomerene* cartels have operated for widely varying durations, ranging from one to thirty–seven years. Cartel lifespans also are strongly skewed toward brief durations, with one–quarter of all episodes ending within two years. It is noteworthy, however, that an almost identical proportion of cartel episodes continue for 15 or more years. Most other cartel samples have displayed similar degrees of dispersion and skewness in lifespans. The legal, private cartels in the *Webb–Pomerene* sample, however, differ in several important respects from stylized facts drawn for prosecuted and government–assisted cartels. These pertain to average cartel stability, the frequency of repeated collusion, the existence of cartel learning effects, and the link between cartel objectives, internal organization and stability.

Finding 1: Collusion tends to be less stable than the literature has suggested. Based on life table estimates, which adjust raw data to reflect censoring, the median *Webb–Pomerene* cartel survived for approximately 5.3 years. Previous estimates found in the literature are almost uniformly higher. Life tables indicate that government–enforced Japanese export cartels have a median lifespan of approximately 8.5 years, and an 82% probability of outliving the median *Webb–Pomerene* cartel (Jacquemin *et. al.* (1981)). The median lifespan for a sample of international cartels, one–third of which were directly or indirectly government–supported, was 10.7 years, and fully two–thirds of these cartels outlived the median *Webb–Pomerene* cartel (Suslow (1988)).[15] Among previous cross–sectional studies of cartel stability, only Marquez (1992) reports a median lifespan (5.2 years) as low as the *Webb–Pomerene* value.[16]

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[14] The survival function S(t) measures the probability that a cartel episode will continue at least until date t. The integrated hazard function is calculated as $H(t) = -\ln S(t) = \int_0^t h(\tau)\, d\tau$ where $h(\tau)$, the hazard function, measures the probability that a cartel episode will end at date τ conditional upon it having lasted until that date.

[15] The summary statistics reported by Suslow (1988, Table 2) do not control for censoring, which affects 40% of her sample and tends to understate actual cartel lifespans. The Jacquemin *et. al.* (1981) sample is unaffected by censoring.

[16] Marquez (1992) does not identify the extent of government enforcement assistance within his sample. Among domestic cartels, Posner (1970, 399–401) estimates an average lifespan of 7.0 years for prosecuted price–fixing conspiracies. Antitrust prosecution censors each cartel's

Finding 2: Expired cartels tend to re–organize less frequently than the literature has suggested. In only 17% percent of industries (16 out of 93) did *Webb–Pomerene* cartels re–form after a dissolution. By contrast, repeat rates among international cartels have ranged from 36% (Suslow (1988, 35–37)) to 64% (Marquez (1992, 2a)). The first fraction exceeds the *Webb–Pomerene* repeat rate at the 5% significance level, and the second at the 1% level. *Webb–Pomerene* cartels' extremely low repeat frequency suggests that dissolutions reflect a breakdown of discipline (followed nearly always by permanent punishment) or a 'natural' death as the cartel outlives its objective, rather than short–term reversions to non–cooperation as punishment in a trigger–price strategy equilibrium.[17] An explanation for *Webb–Pomerene* cartels' low repeat frequency is suggested by the next finding.

Finding 3: Cartels do not grow more stable with experience or tenure. First, after adjusting for censoring, the median lifespan among first–time *Webb–Pomerene* cartels (5.5 years) is statistically indistinguishable from the median lifespan in subsequent episodes (4.7 years). Log–rank and Generalized Wilcoxon tests also both fail to reject the hypothesis that initial and repeat episodes follow the same distribution of cartel lifespans. Second, while cartels with above–average duration in their first episode are more likely to re–form after a dissolution, as a group these cartels revert to the sample's median duration in their later episodes. Finally, *Webb–Pomerene* cartels also do not grow more stable with tenure. Assuming a Weibull survival distribution, cartels' estimated hazard function is $h(t) = 0.0923t^{.009}$. The time exponent is insignificantly different from zero, so we cannot reject the hypothesis of a constant cartel hazard rate. Table 2's raw data do suggest, however, significant heterogeneity in cartel hazard rates. After controlling for heterogeneity in industry and market characteristics (see Section IV), I find

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lifespan and introduces a selection bias in favor of short–lived cartels. Working in the opposite direction is measurement error from classifying intermittent episodes as continuous collusion. The net effect is ambiguous *apriori*.

17 While the data's annual frequency may obscure more subtle dynamics consistent with Green and Porter's (1984) trigger–price model, the fact that punishment phases in multi–episode cartels vary widely in length within a given industry also appears inconsistent with their prediction of a uniform punishment length.

that *Webb–Pomerene* cartels are significantly *more* susceptible to collapse the longer that they have been in operation, and the more extensive and recent is their prior experience.

Studies of international cartels have suggested that cartels grow more stable with experience. Suslow (1988) and Marquez (1992), for example, report that median cartel lifespans rose between 25% and 144% after an industry's initial experience with collusion.[18] Selection biases again can partially explain discrepancies between *Webb–Pomerene* cartels and evidence for international cartels. Suslow (1988, 28) reports both that government support is more frequent among multiple than single–episode international cartels, and that its incidence rises with each subsequent episode.[19] These patterns will bias upwards both the probability of repeated collusion and average cartel lifespan among later episodes. The absence of these biases in the *Webb–Pomerene* sample can explain its constant hazard rate, but it alone cannot explain why stability falls with a cartel's prior experience. Potential causes of declining stability will depend on a cartel's primary objective. Successful price–fixing cartels may become less stable if supra–competitive margins lead to demand substitution, increased chiseling, industry entry or learning by buyers to extract secret price cuts. Stability should decline among joint investment cartels as growth in average member exports eventually allows firms to exhaust economies of scale internally.

Finding 4: Cartels often forego comprehensive monitoring of members' transactions. While cartels may organize in a variety of ways to enhance monitoring, Stigler (1964, 45) identified the joint sales agency as second only to formal merger in its ability to process transaction information efficiently to assist cartel enforcement. Despite this advantage, fewer than 6% of domestic price–fixing conspiracies, only one–quarter of international cartels, and (based on a

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[18] Among international cartels with multiple episodes, Marquez (1992, 2a) reports that the median lifespan rose from 4 to 5 years after the initial attempt, although the rise was not statistically significant. Suslow's (1988, 15, 35–37) sample identifies a corresponding increase in median duration from 2 years and 10 months to 6 years and 11 months, which was statistically significant.

[19] Ten percent of single–episode and 48% of multiple–episode cartels in Suslow's sample were government–assisted. Among multiple–episode cartels, assistance was received in 40% of first episodes, in 50% of second episodes, and in 60% of third episodes.

sub–sample of 69 episodes) fewer than 60% of *Webb–Pomerene* cartels acted as joint sales agents (Fraas and Greer (1977, 34), Posner (1970, 400), Suslow (1988, 27)).[20] Lower adoption rates among the first two groups may be explained, respectively, by firms selecting less visible enforcement methods to avoid antitrust detection, and joint sales agencies' diminished comparative advantage when governments assist cartel enforcement. Neither hypothesis, however, can explain why more than 40% of legal, self–enforcing *Webb–Pomerene* cartels chose not to centralize their sales, requiring them to enforce their agreements using less direct or complete price and quantity information.

Stigler (1964, 45) suggests a potential explanation when he identifies the joint sales agency's "economic limitations — it is ill suited to custom work and creates serious administrative costs in achieving quality standards, cost reductions, product innovations, etc." These limitations are most important in industries where flexible coordination offers firms significant scope for quality enhancements and cost savings through joint investments. Thus, at the margin, joint sales agencies should be observed more frequently among price–fixing cartels than among joint–investment cartels. This conclusion is consistent with evidence reported by Dick (1992, 106) that *Webb–Pomerene* cartels with joint sales agents tended to restrict exports, while those without centralized selling were expansionary. It is also consistent with the slight tendency for cartels covering relatively less homogeneous products not to adopt a joint sales agency, as these cartels would find the agency's limitations relatively more costly.

Finding 5: Finally, cartels operating joint sales agencies tend to be shorter–lived. Cartels that centralized sales had an estimated median lifespan of 4.5 years as compared with 7.4 years for cartels whose members continued to ship individually. While the relatively small number of observations makes this difference statistically insignificant, the ranking of median lifespans is contrary to expectations. Finding 4 suggests an explanation: while joint–investment cartels should be less likely to centralize sales, they may be more inherently stable. While these cartels

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20 As explained in further detail in Section IV.B, data on whether a cartel formed a joint sales agency were available for 69 of the 111 episodes. Of this sub–sample, 40 (or 58%) formed a joint sales agency.

must monitor against free–riding like price–fixing cartels, provided that they do not earn super–normal returns they will not face entry threats. Pirrong (1992, 101) has argued that these lower enforcement costs among surplus–enhancing cartels creates "survival value." If joint–investment cartels do enjoy survival value, this may offset their higher monitoring costs from operating without a joint sales agency, and thus may explain their longer average lifespan. In Section IV, I find weak evidence consistent with a survival value effect.

To summarize, cartels tend to be less stable, less recurrent and less likely to grow more stable with experience than previous empirical research has suggested. By overlooking differences in cartel objectives, the literature also has obscured the link between internal cartel organization and stability. I explore these issues more systematically in the remainder of the paper.

III. Application of the Survival Model to Cartel Duration

Following Suslow (1988), I adopt a survival model to study cartel longevity.[21] The unit of observation is a cartel episode, whose length is measured in years. Let T_i, a continuous random variable, denote a cartel episode's length ($i = 1, \ldots, N$). The probability of a cartel episode continuing to time τ is given by the survivor function

$$(1) \qquad S(\tau) = \text{Prob}\,(T \geq \tau) = \int_{\tau}^{\infty} f(x)\,dx = 1 - F(\tau)$$

where $f(\,)$ and $F(\,)$ are the probability and cumulative distribution functions for T_i, respectively. The probability that a cartel episode ends at time τ, given that it has survived until that time, is defined by the hazard or failure rate

$$(2) \qquad h(\tau) = \frac{f(\tau)}{S(\tau)} = \frac{f(\tau)}{1 - F(\tau)} \; .$$

As Section II indicated, ending dates for some cartel episodes in the sample are right–censored by the conclusion of the FTC chronology in 1965. If C_i is the censoring date for the i–th episode, the cartel's actual duration is observed only if $T_i \leq C_i$. Define

$$t_i = \min \{T_i, C_i\}$$

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21 Kiefer (1988) summarizes recent economic applications of survival models. Comprehensive treatments appear in Kalbfleisch and Prentice (1980) and Cox and Oakes (1985).

and

$$\delta_i = \begin{cases} 1 & \text{if } T_i \leq C_i \quad \text{(no censoring)} \\ 0 & \text{if } T_i > C_i \quad \text{(censoring)} \end{cases} .$$

The joint probability density function of t_i and the censoring indicator variable δ_i is then

$$f(t_i)^{\delta_i} \; S(C_i)^{1-\delta_i} \; .$$

When the pairs (t_i, δ_i) are independent, the likelihood function can be written as

$$(3) \qquad L = \prod_{i=1}^{N} f(t_i)^{\delta_i} \; S(C_i)^{1-\delta_i} \; .$$

Expressing the likelihood function in terms of a vector $\mathbf{x}$ that describes cartels' informational environment and a vector $\boldsymbol{\theta}$ of parameters to be estimated, the function in (3) becomes

$$(4) \qquad L = \prod_{i=1}^{N} f(t_i ; \boldsymbol{\theta}, \mathbf{x}_i)^{\delta_i} \; [\, 1 - F(C_i ; \boldsymbol{\theta}, \mathbf{x}_i)]^{1-\delta_i} \; .$$

The first term reflects the probability that the cartel episode ends at time t_i, and the second captures the probability that this event occurs after the censoring date C_i. Applying logarithms and using the definition in equation (1), the log–likelihood function is

$$(5) \qquad \ln L(\boldsymbol{\theta}) = \sum_{i=1}^{N} \delta_i \ln f(t_i ; \boldsymbol{\theta}, \mathbf{x}_i) + \sum_{i=1}^{N} (1-\delta_i) \ln S(t_i ; \boldsymbol{\theta}, \mathbf{x}_i) \; .$$

Section IV estimates the log–likelihood function in semi–parametric and parametric forms. The primary advantage of semi–parametric estimation is the absence of a distributional assumption regarding cartels' ending dates. The proportional hazard rate model instead factors the hazard function $h(t, \mathbf{x}, \boldsymbol{\beta}, h_0)$ as $h_0(t)\, \phi(\mathbf{x}, \boldsymbol{\beta})$, where $h_0(t)$ is a baseline hazard rate and $\phi(\mathbf{x}, \boldsymbol{\beta})$ is a shift factor that depends on cartel information characteristics $(\mathbf{x})$ and parameters for estimation $(\boldsymbol{\beta})$. If $t_i = \{t_1, \ldots, t_k\}$ is the set of K distinct cartel ending times among the N observations, and if R_i denotes the index set of cartels at risk just prior to time t_i (i.e., cartels with duration $T_j \geq t_i$), then we may estimate the likelihood function in (5) as

$$(6) \qquad \ln L(\boldsymbol{\beta}) = \sum_{i=1}^{K} \Big[\sum_{j \in t_i} \mathbf{x}_j \boldsymbol{\beta} - m_i \ln \sum_{j \in R_i} \exp(\mathbf{x}_j \boldsymbol{\beta}) \Big]$$

where $m_i \geq 1$ is the number of cartel episodes ending at the same date t_i (the number of "ties").

A disadvantage of the proportional hazard model is that, without a distributional assumption, it is possible to identify only the relative impact of covariates on cartels' duration.

Parametric estimation avoids this constraint by relaxing the assumption that the ratio of hazard rates for any two cartel episodes is constant through time. To do so, it is necessary to specify a general functional form for the hazard function. The Weibull distribution is recommended by the raw data and also offers one of the least constraining choices, since it can accommodate a constant, increasing or decreasing hazard rate. Using equations (1) and (2), rewrite $f(t_i; \theta, x_i)$ as $h(t_i; \theta, x_i)\, S(t_i; \theta, x_i)$ and $\ln S(t_i; \theta, x_i)$ as $- H(t_i; \theta, x_i)$, where $H(\,)$ denotes the integrated hazard function. The log–likelihood function in (5) then can be restated as

$$(7) \qquad \ln L(\theta) = \sum_{i=1}^{N} \delta_i \ln h(t_i; \theta, x_i) - \sum_{i=1}^{N} H(t_i; \theta, x_i) \; .$$

The Weibull distribution specifies $h(t_i; \theta, x_i) = \gamma\alpha t^{\alpha-1}$ and $H(t_i; \theta, x_i) = \gamma t^{\alpha}$ where $\gamma = \exp(x'\beta)$. The conditional probability of cartel failure is increasing, decreasing or constant with respect to tenure as $\alpha > 1$, $\alpha < 1$ or $\alpha = 1$.

IV. Testing the Information Theory

Stigler (1964) linked cartels' enforcement costs to specific product, buyer and seller characteristics and recent extensions have explored the influence of business cycle timing and opportunities for multi–market collusion. I now use Section III's survival methodology to quantify the impact of these characteristics on cartel stability. Part A reviews the literature's predictions, explains how I test them within the *Webb–Pomerene* sample, and reports survival regression results for the 111 cartel episode sample. Table III explains the construction and timing of regression variables, and summarizes their descriptive statistics and predicted signing.[22] Recall that the dependent variable reflects the conditional probability of cartel failure, and thus is negatively correlated with cartel stability.

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22 In addition to the independent variables described in Part A, I include in all regressions a dummy variable WW2 equal to one if the cartel episode ended between 1939 and 1945. WW2 enters negatively, and is occasionally statistically significant. The variable should be interpreted simply as a control.

I estimate both the proportional hazard and Weibull survival models, and report their results in Tables IVa and IVb, respectively. With very few exceptions, the sign and statistical significance of the regressors are consistent between the two models. I also estimated a variation of the Weibull model that allowed for heterogeneity in individual cartels' survival distributions following a Gamma distribution.[23] In each case, it was possible to confidently reject the hypothesis that heterogeneity remained after controlling for observed industry and market characteristics. Therefore, I report only the results for the original two models. The survival analysis begins by focusing upon export market characteristics (column 1 in each table) and then adds domestic market factors to explore multi–market collusion effects (columns 2 and 3). Finally, Part B analyzes a representative sub–sample of 69 episodes that identify whether a cartel acted as a joint sales agent. The regression results, reported in Tables Va and Vb, are consistent with those from the full sample and shed light on the relationship between cartel objectives, internal organization and stability.

A. Predictions and Empirical Results

(i) Buyer Characteristics

Predictions Stigler (1964, 47–48, 51) predicted that cartel enforcement costs will be higher when buyers are large relative to the size of the market, make few repeat purchases, and have a high rate of turnover. Large buyers limit the number of price signals received by cartel members, while product switching and buyer turnover introduce noise into the cartel's monitoring task.[24] Each of these buyer characteristics hinders detection of pricing violations and thus should raise

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23 Specifically, the survival function was modified under the Weibull assumption from $S(t) = \exp(-\gamma t^{\alpha})$ to $S(t|v) = v\{\exp(-\gamma t^{\alpha})\}$ where the random variable v is the heterogeneity effect, assumed to follow a Gamma distribution.

24 Noting that "[n]o one has yet invented a way to advertise price reductions which bring them to the attention of numerous customers but not to that of any rival," Stigler (1964, 47) predicted that "oligopolistic collusion will often be effective against small buyers even when it is ineffective against large buyers." Arguing that "[w]hat is possibly due to random fluctuations in the short run cannot with equal probability be due to chance if repeated," Stigler (1964, 55) also predicted that enforcement costs will vary directly with the frequency of buyer turnover and inversely with repeat purchase frequencies.

the probability of cartel failure. As a proxy for average buyer size, I use the percentage of U.S. industry exports purchased by the four largest importing countries (BUYCR). The implicit assumption, forced by data limitations, is that firms make export pricing decisions and cartels monitor these prices at the country level. In the absence of direct information on repeat purchase probabilities and buyer turnover, I exploit the strong, negative correlations between the length of buyer–seller relationships and variability in industry demand and price (Carlton (1986, 638)). I create two variables for this purpose. EXGROW equals the absolute, average annual growth rate in industry export volume during the cartel's lifespan. EXPVAR equals the coefficient of variation in quarterly export prices during the cartel's lifespan. To avoid possible endogeneity concerns, I calculate both variables by assigning cartelized products to one of four broader categories that will be unaffected by individual cartel actions: foods, crude materials, semi–manufactures or finished manufactures. While this construction assures that EXPVAR and EXGROW will be exogenous measures of the noisiness of cartels' monitoring environments, this comes unavoidably at the cost of potentially ignoring commodity–specific information. The theory predicts that these three buyer characteristics will enter positively into the estimated hazard function.

Empirical Results The regressions offer some support for Stigler's (1964) predictions regarding buyer characteristics. Cartel instability varies positively and always significantly with EXPVAR, which supports the hypotheses that export price variability and, by association, buyer turnover raise enforcement costs. EXPVAR's significance also lends support to Green and Porter's (1984) emphasis upon imperfect price information in explaining cartel dynamics. Variability in aggregate demand (EXGROW) always has the predicted positive sign, but its statistical significance is eliminated upon including a measure of domestic market variability in columns (2) and (3). Finally, average buyer size (BUYCR) has a consistently positive signing and is significant at the 5% level in the full specification (column 3). Consistent with Stigler's prediction, collusion appears somewhat less stable when buyers are large relative to the size of the market.

The *Webb–Pomerene* sample identifies a more consistent relationship between cartel stability and buyer characteristics than others have reported for prosecuted and government–assisted industry agreements. Asch and Seneca (1975), Fraas and Greer (1977) and Jacquemin *et. al.* (1981) each found a generally insignificant correlation between the frequency or stability of collusion and buyer turnover, as proxied by either industry sales growth or demand variability. Among previous studies, only Suslow (1988) reports a consistently significant, negative effect from demand variability on cartel stability. The absence of a correlation between cartelization and buyer characteristics in most prior analyses may be attributable to the contaminating influence of government cartel policy. Government assistance to cartels operating in volatile markets will artificially weaken the estimated relationship between stability and buyer turnover or demand variability. Among illegal agreements, demand variability simultaneously lowers cartels' expected lifespan and the expected probability of antitrust detection, thus weakening the anticipated correlation between cartel stability and demand variability.

(ii) Seller Characteristics

Predictions Stigler identified cartel membership, information pooling and prior experience as seller characteristics that affect enforcement costs. Stigler (1964, 49–51) predicted that cartel enforcement costs rise with the number of colluding firms to reflect the increased difficulty of detecting deviations from rivals' assigned market shares. More recent game theoretic analyses, however, suggest that cartel stability may be relatively insensitive to the number of conspirators, provided that market demand grows proportionately with cartel membership (Green (1980) and Lambson (1984)). To identify the relationship if any between stability and size, I include a variable CARTELN equal to the number of firms in the cartel.

Stigler (1964, 51) also predicted that the effect of cartel membership will be mitigated by information pooling, which facilitates detection of less extreme price–cutting violations. Information pooling should rise with average firm size, the cartel's share of industry exports, and the industry's share of world exports. Average firm size is measured by the four–firm industry

concentration ratio, SELLCR. I measure cartels' industry export share with a dummy variable WPCOVER that equals one for cartels covering at least 50% of U.S. industry exports and zero otherwise.[25] I also experiment with an interactive variable (1–WPCOVER) x CARTELN to test the hypothesis that stability will be more sensitive to the number of colluding firms when the cartel covers only a small fraction of total industry exports. Finally, I include a variable EXSHARE that equals the U.S. industry's share of world exports. With the exception of the interactive term, the information pooling variables are expected to enter negatively.

A final seller characteristic is the cartel's prior experience with collusion. If cartels become more adept at detecting cheating with experience, perhaps by developing more precise estimates of industry demand or rivals' costs to assist monitoring or by adapting enforcement strategies, stability should rise with cartel tenure and across subsequent episodes. Cheating firms also may learn with time how to avoid detection, however, making the effect of learning theoretically ambiguous. Furthermore, in price–fixing cartels any tendency towards increasing stability may be undermined by learning or substitution among buyers or by supply responses to supra–competitive margins, while in joint–investment cartels the rationale for continued operation will be gradually eroded by growth in average firm size. I test for a net experience effect in two ways. First, I investigate whether cartels' estimated hazard rate varies with their tenure after removing the effects of industry–specific characteristics. If cartels become more stable with the passage of time, they will display a falling hazard rate. Second, I create a variable EXPERIENCE measuring the length of an industry's previous cartel episode(s), depreciated for each year elapsed since the prior episode(s).[26] The variable tests the joint hypothesis that (i) stability depends upon the extent of prior success in colluding (proxied by the length of previous episodes), and (ii) that recent and distant experience may have different impacts on cartel

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[25] For most episodes, it was only possible to identify cartel coverage using this binary scale. *Webb–Pomerene* cartels' coverage ranged from as high as 70–80% for industries such as sulphur and carbon black to as low as 3% for industries such as powdered milk and milled flour (U.S. Federal Trade Commission (1967, 41)).

[26] I experimented with several arbitrary annual depreciation rates: 20% performed slightly better than 10% and significantly better than with no depreciation. Note that EXPERIENCE equals zero for cartels' first episode.

stability. EXPERIENCE will enter negatively if cartel learning effects dominate, and positively if offsetting forces prevail.

Empirical Results Table IVa indicates that seller characteristics have a mixed effect on cartel stability. Contrary to expectations, the probability of cartel failure is insignificantly related to both average firm size (SELLCR) and the industry's world export market share (EXSHARE).[27, 28] Proportional hazard regressions that stratified the sample on the basis of the industry's concentration ratio and export market share also found no significant effect from either cartel characteristic. Consistent with the information pooling hypothesis, however, cartels that cover at least one–half of their industry's exports (WPCOVER = 1) have a significantly lower probability of failure. In three of four cases, cartel membership (CARTELN) does not enter significantly except when interacted with the WPCOVER dummy. The significant, positive coefficient on (1 – WPCOVER) x CARTELN in column (3) of Tables IVa and IVb indicates that large membership cartels with limited industry market shares have a particularly high probability of failure. This result is consistent with Stigler's conjecture that information pooling will tend to mitigate the effect of cartel membership on stability, and with Green (1980)

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[27] The SELLCR variable is a proxy for average firm size within the entire industry, which may differ from average cartel member size. It was possible to identify for a group of 14 episodes the number of cartel members in the industry's top 5 and top 10 producers (U.S. Federal Trade Commission (1967, 111)). Neither variable, however, proved to be correlated significantly with cartel duration, suggesting that the regression results for SELLCR are not explainable solely by proxy variable problems.

[28] Cartel heterogeneity may be a more important determinant of cartel stability than average firm size or concentration. Wiggins and Libecap (1987) predict that small firms have the greatest incentive to chisel and, if their collective market share is large, they may undermine cartel discipline. While data limitations severely constrained testing this hypothesis, casual empiricism lends some support. Larson (1970, 470–71) reports that 71% of *Webb–Pomerene* firms with assets less than $1 million belonged to agricultural commodity cartels, and these smallest firms represented more than one–half of agricultural cartel membership. Consistent with Wiggins and Libecap (1987), agricultural cartels had the highest frequency of dissolution within the first five years of operation and one of the lowest frequencies of survival beyond 20 years (Federal Trade Commission (1967, Table II–6)). More systematic empirical analysis, however, was inconclusive. As one measure of heterogeneity, I included the number of states in which cartel members were located. Geographic dispersion may raise cartel enforcement costs either directly by increasing monitoring expenditures or indirectly by increasing the likelihood of input cost differences among firms. The variable always had the predicted positive sign, but it was never statistically significant at standard levels. A second heterogeneity measure, the ratio of the industry's four largest to four smallest firms' outputs, also failed to enter significantly.

and Lambson's (1984) conclusion that proportionately larger demand is required for cartels to remain stable as membership grows.[29]

Previous studies have yielded mixed and weak conclusions regarding the impact of these seller characteristics on the frequency and stability of collusion. Among government–assisted cartels, the probability of failure was found to be positively but insignificantly related to membership size, negatively but often insignificantly related to market share, and ambiguously related to seller concentration (Jacquemin *et.al.* (1981), Suslow (1988) and Marquez (1992)). Hay and Kelley (1974) and Asch and Seneca (1975) report similar, mixed results for prosecuted price–fixing agreements. With the important exception of cartels' industry coverage, seller characteristics identified by classical oligopoly theory appear empirically to have little systematic effect on cartel stability.

Finally, EXPERIENCE provides perhaps the most interesting insight into cartel dynamics, and offers the sharpest contrast with previous conclusions in the literature. This variable enters positively and highly significantly in five of the six regressions in Tables IVa and IVb, allowing us to reject with confidence the hypothesis that cartels become more stable with experience. To the contrary, repeat cartels tend to break up *sooner* than first–time cartels.[30] Furthermore, this decline in cartel stability is more pronounced the longer and the more recent is the industry's experience with collusion. Both the regression coefficient and its statistical significance fell when EXPERIENCE was replaced with a dummy variable simply identifying whether a cartel had a prior episode.

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[29] The raw data also reflect this interaction between cartel membership and industry coverage. The 160–member Millers Export Association controlled only a small fraction of U.S. flour exports and collapsed after one year. The Textile Export Association, by contrast, controlled well over half of all cotton and rayon textile exports and had active episodes of 11 and more than 19 years despite having 121 members. Madhavan, Masson and Lesser (1993) also have recently documented the stability of collusive agreements with large memberships when cartel market share is high.

[30] A decline in average lifespan does not necessarily imply a fall in the cartel's cumulative monopoly profits or efficiency rents. Short–lived cartels may be successful when judged by a profit/rent–extraction criterion. A profitable cartel's break–up does imply, however, that enforcement costs exceeded its expected future profits or rents.

Webb–Pomerene cartels also become less stable the longer they have been active within an episode. Under the assumption that cartel hazard rates follow a Weibull distribution $h(t; \theta, x) = \gamma\alpha t^{\alpha-1}$, Table IVb's range of estimates for α (from 1.5 to 1.7) implies that the conditional probability of failure rises proportionately with the square root of cartel tenure.[31] Competitive supply responses, entry and buyer substitution may explain diminishing stability among successful price–fixing cartels, and growth in average firm size may provide an explanation for joint–investment cartels. The *Webb–Pomerene* results contrast sharply with Marquez (1992), who found no direct effect of experience on cartel lifespan and Suslow (1988), who found that the probability of cartel failure fell with experience and was unrelated to tenure. Suslow's observation that government cartel support is more common among repeat cartels, and rises across subsequent episodes, may explain these discrepancies.[32]

(iii) Product Characteristics

Predictions Stigler (1964, 45, 59) predicted that cartels will incur lower enforcement costs for homogeneous products and products with relatively stable demand conditions. Product homogeneity narrows firms' scope for non–price competition in product quality and servicing and therefore should allow the cartel to simplify enforcement. I test this hypothesis with a dummy variable HOMOG equal to one for homogeneous goods cartels, and expect it to enter negatively.[33] Greater demand variability raises the cost of distinguishing between random deviations from cartel targets and deviations induced by price–cutting. To test this hypothesis, I use again the variable EXPVAR, measuring the coefficient of variation in quarterly export prices during the cartel's lifespan, and expect it to raise the probability of cartel failure.[34] Finally, the

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31 The tendency for cartels to become less stable with tenure, i.e., to exhibit positive duration dependence, will be understated by Table IVb if unobserved heterogeneity remains in the data (Kiefer (1988, 672)).

32 Among multiple–episode international cartels in Suslow's (1988, 28) sample, governments assisted enforcement in 40% of first episodes, 50% of second episodes, and 60% of third episodes. Suslow and Marquez both measure prior cartel experience with a dummy variable.

33 In the sample, these primarily include cartels for industrial raw materials and unprocessed agricultural products.

34 Recall that EXPVAR is measured at an aggregated level to avoid endogeneity concerns.

ability of cheating firms to expand short–run output will be limited by their installed capacity. Collusion therefore should tend to be more stable for products in industries with high fixed capital intensity.[35] As an inverse proxy for firms' short–run elasticity of supply, I include the ratio of fixed (plant) capital to output, CAPITAL, and expect it to enter negatively.

Empirical Results As earlier noted, cartel instability is always positively and significantly related to export price variability, EXPVAR. The remaining product characteristics receive less empirical support. HOMOG always has the anticipated negative coefficient but is never statistically significant.[36] HOMOG's insignificance may reflect the fact that entry is likely to be easier in homogeneous products industries, thereby undermining the stability of a price–fixing cartel in precisely those cases that monitoring costs should be lowest. Alternatively, the primary influence of product homogeneity may be felt at the cartel formation stage. As the theory predicts, compared with U.S. exports as a whole over the sample period, *Webb–Pomerene* exports were much more heavily concentrated in homogeneous goods industries covering agriculture, raw or semi–finished materials, and simple manufactures.[37] Finally, like HOMOG, CAPITAL enters with the expected signing but is never close to being statistically significant. The regressions suggest that the likelihood of cheating is relatively insensitive to industry capital intensity.

Among product characteristics, homogeneity has received the most attention in past studies. Jacquemin *et. al.* (1981) and Asch and Seneca (1975) report that homogeneity increases stability significantly among government–assisted cartels, yet only marginally among prosecuted price–fixing agreements. Both findings are sensibly reconciled with the *Webb–Pomerene* results. First, homogeneous goods cartels should be more successful in petitioning for government

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35 Telser (1988, 210) draws a concurring prediction, arguing that illiquid capital acts as a hostage to good behavior by cartel participants.

36 Estimation with a time–varying coefficient for HOMOG also failed to find an effect even in early years of cartel operation.

37 Approximately one–third of the cartels sold agricultural and food products, another third exported raw or semi–finished materials (lumber, phosphate, sulphur), and an additional 10% exported simple manufactures (pencils, buttons) (U.S. Federal Trade Commission (1967, App. C2)).

support because of their greater commonality of interests, which will strengthen the link between homogeneity and stability. Second, while homogeneous goods cartels should have lower enforcement costs reflecting their simplified pricing rules, this advantage will be dampened by the fact that product homogeneity also facilitates detection of illegal horizontal conspiracies by antitrust authorities.

(iv) Business Cycle Timing

Predictions Recent extensions of Stigler's theory identify business cycles as an additional determinant of the timing of cartel breakdowns, although they disagree over the direction of effect. Green and Porter (1984) suggest that colluding firms will revert to non–cooperative pricing when there is an unexpected downturn in industry demand causing industry price to fall below the cartel's trigger value. Furthermore, if some cartel members are likely to have exited from the industry by the end of the next downturn, then the expected horizon over which the potential colluders can cooperate is shortened. By introducing a last period problem, the standard endpoint unraveling result should arise, and thus cartels should be less stable during downturns. Rotemberg and Saloner (1986) argue instead that collusive prices are more difficult to sustain when demand is high relative to its trend. They note that the expected gain from cheating is larger during booms, while punishment is less costly because it occurs in the future when demand is likely to have reverted to its trend.

To determine the effect of business cycle timing on cartels' stability, I create three dummy variables that equal one if an industry was in a peak–to–trough period in the cartel's final year, and equal zero if the cartel dissolves in a trough–to–peak period. The three dummies differ according to the choice of reference series and the industry. EXPCYCLE links cartel dissolutions to upturns and downturns using real export price movements. EXQCYCLE is a corresponding dummy based on cycles in export quantities. To assure that both variables were exogenous with respect to the cartel's pricing and output decisions, the export price and volume series were defined for broad product categories: foods, crude materials, semi–manufactures or

finished manufactures.[38] The final series is GNPCYCLE which dates GNP cycles for the United Kingdom, the primary market for *Webb–Pomerene* exports. To the extent that firms did not anticipate deviations from trends in export prices, export volumes or foreign income, the reference cycles allow a test of the Green–Porter and Rotemberg–Saloner hypotheses. Green and Porter predict that the business cycle timing dummies should enter positively, while Rotemberg and Saloner predict the opposite. To the extent that the deviations were expected, it is possible to test the endpoint unraveling hypothesis. Positive coefficients will lend support to this hypothesis.

Empirical Results Tables IVa and IVb indicate that cartels were more susceptible to collapse during demand downturns. The export price cycle timing dummy EXPCYCLE enters positively and is significant at the 1% level or lower in each regression. Regressions based on EXQCYLE and GNPCYCLE yielded qualitatively similar results. The regressions thus provide strong support for either Green and Porter's (1984) model or the endpoint unraveling hypothesis. It is also noteworthy that EXPCYCLE's coefficient and statistical significance both rise when domestic market cycles are introduced (c.f. cols. (1) and (2), as discussed shortly). The result implies that business cycle timing effects are market–specific. Finally, the regression results also are consistent with Suslow's (1988) findings among international cartels, which suggests that government assistance to trade cartels is not strongly correlated with international business cycle timing.

(v) Multi–Market Collusion Opportunities

Predictions Cartels may lower enforcement costs by linking monitoring and punishment across multiple markets so as to pool incentive constraints (Telser (1980, 41–42), Bernheim and Whinston (1990, 3)). In this way, collusive agreements that might not be stable in any single market may be made self–enforcing by linking markets together. I analyze potential multi–

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[38] These two series also capture implicitly the impact of exchange–rate movements in the pre–Bretton Woods era that could influence export cartel stability.

market links between *Webb–Pomerene* cartels and foreign producers, and across *Webb–Pomerene* firms' domestic and export markets. First, as earlier noted, the FTC investigated 12 cartels for entering into quid–pro–quo agreements with foreign producers to coordinate output restrictions. I test whether these alleged agreements lowered *Webb–Pomerene* enforcement costs by including a dummy variable INTL equal to one for the twelve investigated cartels. If multi–market effects are present, INTL should enter negatively. However, if U.S. exporters free–ride on international cartels to avoid the costs of formal collusion, INTL should enter positively.

Second, firms might have used (legal) *Webb–Pomerene* agreements to facilitate (illegal) collusion domestically. Firms caught cutting price domestically could be punished, for example, by reducing their allotted export market share or with equivalent sanctions. While *Webb–Pomerene* cartels can facilitate tacit domestic agreements, their enforcement assistance will be indirect and potentially incomplete. This suggests that the cartels' primary effect will be to lower enforcement costs among viable domestic agreements rather than to assure self–enforcement on their own.[39] According to this hypothesis, therefore, *Webb–Pomerene* cartels should more frequently be active when tacit collusion is viable domestically.

Viability of domestic collusion depends upon the same group of predictors used to explain export cartel stability. While many of these industry characteristics do not vary across markets (eg., product homogeneity, durability and seller concentration), others such as demand variability, business cycle timing and information pooling must be re–defined for firms' domestic market. I measure domestic demand variability by the coefficient of variation in quarterly U.S. industrial output during the cartel episode, INDVARQ, and expect it to enter positively. To capture the effect of domestic business cycle timing, which the theory cannot sign *apriori*, I introduce a dummy variable INDCYCLE equal to one if a cartel dissolves in a peak–to–trough period for U.S. industrial output. To assure exogeneity, INDVARQ and INDCYLE were constructed by assigning each cartel to one of three broad sectors: mining, non–durable

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[39] The cartels' minimal FTC–compliance costs also suggest that even when domestic cartels could be entirely self–enforcing, firms are still likely to avail themselves of the *Webb–Pomerene* exemption to lower private enforcement costs.

manufacturing or durable manufacturing. To measure opportunities for information pooling, I constructed a dummy variable DTACOVER measuring the percentage of an industry's shipments covered by a domestic trade association. DTACOVER will enter negatively (positively) if export cartels and domestic trade associations are complementary (substitute) information sources. Finally, a cartel's ability to punish domestic price–cutting should increase with the relative size of the export market as measured by the industry's export orientation ratio, EXORIENT. A cartel may obtain a given level of deterrence with a smaller monitoring expenditure the higher is EXORIENT, implying that this variable should enter negatively in the estimated hazard function.[40]

Empirical Results Columns (2) and (3) in Tables IVa and IVb provide some evidence of multi–market links, and confirm previous researchers' finding of a positive relationship between the number of markets that a cartel spans and its success with collusion (Asch and Seneca (1975) and Jacquemin *et.al.* (1981)). First, export cartels were consistently more likely to fail when demand in the domestic industry was more variable, as measured by INDVARQ. Second, cartels were less prone to collapse in industries with a parallel agreement among foreign producers (INTL = 1). Third, the nearly significant negative coefficient on INDYCLE indicates that *Webb–Pomerene* cartels may have been more likely to be active during domestic downturns. One interpretation consistent with the signing is that export cartels act as an escape valve for excess supply that would otherwise destabilize a tacit domestic agreement during an industry downturn. Despite providing generally strong support, the results identify two inconsistencies with the multi–market collusion hypothesis. Neither the industry export orientation ratio (EXORIENT) nor domestic trade associations' industry coverage (DTACOVER) significantly affects export cartel instability, contrary to predictions.

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[40] As EXORIENT reaches some critical level, however, firms may shift their primary focus for collusion to the export market.

B. The Effect of Cartels' Internal Organization

Stigler's characterization of the joint sales agency as an efficient processor of information implies that centralizing sales should lower cartel enforcement costs. Section II.B argued, however, that the decision to centralize sales should vary with the cartel's primary objective, and this may have a secondary influence on stability.[41] Analyzing a sub–sample of 69 episodes that identify whether a cartel acted as a joint sales agent, this section explores the link between internal cartel organization and stability. The proportion of cartels in the sub–sample adopting joint sales agencies (58%) mirrors almost exactly the proportion in the full *Webb–Pomerene* population (56%; U.S. Federal Trade Commission (1967, 48)), allowing inferences to be drawn for the cartel population from this section's analysis. When cartel organization is treated as exogenous, I find that adoption of a joint sales agency has a negative but insignificant effect on the probability of cartel failure. When a cartel's choice is decomposed into a portion explained by its objectives and a random component, however, I find slightly stronger evidence that joint sales agencies lower enforcement costs and weak evidence that joint–investment cartels have "survival value." Regression results for the remaining independent variables generally confirm results for the full sample.

I begin by treating the cartel's decision whether to adopt a joint sales agency as exogenous with respect to industry characteristics. Column (1) in Tables Va and Vb reports survival models under this hypothesis. The dummy variable JSA equals one for episodes in which the cartel operated as a joint sales agent. The dummy enters negatively in both regressions, but is only statistically significant at the 10% level in the proportional hazard model. This concurs with Suslow's (1988) general failure to find a significant link between internal cartel organization and stability when organization is treated as exogenous to industry characteristics.[42]

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[41] Data limitations only allowed identification of whether a cartel adopted a joint sales agent. To the extent that other organization structures such as exclusive territories or assigning buyers to sellers are close substitutes for joint sales agents, the regression results will be weakened. However, the data do allow us to test Stigler's conjecture that the joint sales agency offers colluding firms a relatively more efficient enforcement means.

[42] Suslow found that the existence of a patent sharing agreement was the only organizational characteristic to have a consistently significant effect on cartel stability.

Next, I allow for endogeneity by linking a cartel's decision to act as a joint sales agent to its primary objective. Section II.B argued that a cartel's decision to centralize sales should tend to correlate positively with the relative weight that it attaches to price–fixing. To separate endogenous and exogenous influences on cartel stability, I adopt a two–stage approach. In the first stage, I estimate a probit relation between the dummy variable JSA and two industry characteristics correlated positively with the cartels' observed success in raising export prices — the industry's concentration ratio and its share of world exports (Dick (1992, 106)).[43] The probit results decompose a cartel's decision to adopt a joint sales agency into a portion explained by these characteristics (the predicted value) and a random component (the error term).

In second stage survival regressions, I introduce the probit's predicted value to test the hypothesis that cartels whose primary objective is to raise price should tend to be less stable, *ceteris paribus*. These regressions are reported in Column (2) of Tables Va and Vb. The predicted value of JSA enters positively in both cases, and is significant in the Weibull model at the 15% level. While very weak, the results are nonetheless suggestive that joint–investment cartels — those with a predicted JSA equal to zero — may enjoy "survival value" that cannot be attributed solely to their industry characteristics. Such a conclusion would be consistent with the raw data reported in Section II.B.

In a final set of regressions, I use the probit's error term to test whether joint sales agencies lower enforcement costs after expunging the influence of cartel objectives. These results are reported in column (3) of Tables Va and Vb. The unexplained portion of the JSA dummy enters negatively in both models, its significance increases relative to column (1) which treats JSA as exogenous, and the variable is now significant at the 9% level in the proportional hazard model.

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[43] The probit estimation yielded: JSA = –0.0032 SELLCR + 1.6288 EXSHARE
(–0.71) (1.77)
where the numbers in parentheses are t–statistics. Export market share is thus a significant determinant of the cartel's decision to adopt a joint sales agency (significant at the 8% level), but seller concentration is not. The model correctly predicts the dependent variable for 42 out of 69 observations, or 61% of the time. Note that while SELLCR and EXSHARE have been identified as significant determinants of cartels' ability to raise price (Dick (1992)), Section IV indicates that they do not predict cartel duration well. In this sense, successful cartels can be short–lived, and vice versa.

The results thus offer mild support to Stigler's assertion that centralizing sales tends to lower cartel enforcement costs.

V. Conclusion

This paper has examined empirically the link between cartel information, enforcement costs and stability. Cartel lifespan was found to vary significantly and in the predicted direction with price variability, cartel market share, business cycle timing in several markets and the existence of parallel agreements among rivals. After controlling for these and other industry characteristics, cartel stability also declines with tenure and repeat experience. While the paper has focused upon explaining relative cartel stability, the data also shed light on the absolute cost (net of expected gain) of sustaining collusion in these industries. The cost appears quite high: between 1918 and 1965, no more than 42 *Webb–Pomerene* cartels ever operated concurrently and these cartels accounted on average for just 4.8% of total U.S. merchandise exports (U.S. Federal Trade Commission (1967, 23, 87)). At least one explanation may be offered for firms' infrequent use of the *Webb–Pomerene* exemption. Specialized export brokers may offer firms many of the same advantages without the costs of cartel enforcement. Independent export brokers were in fact the most common marketing channel for small U.S. exporters during this period (Greene (1968)).

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Figure 1: Webb–Pomerene Survival Function

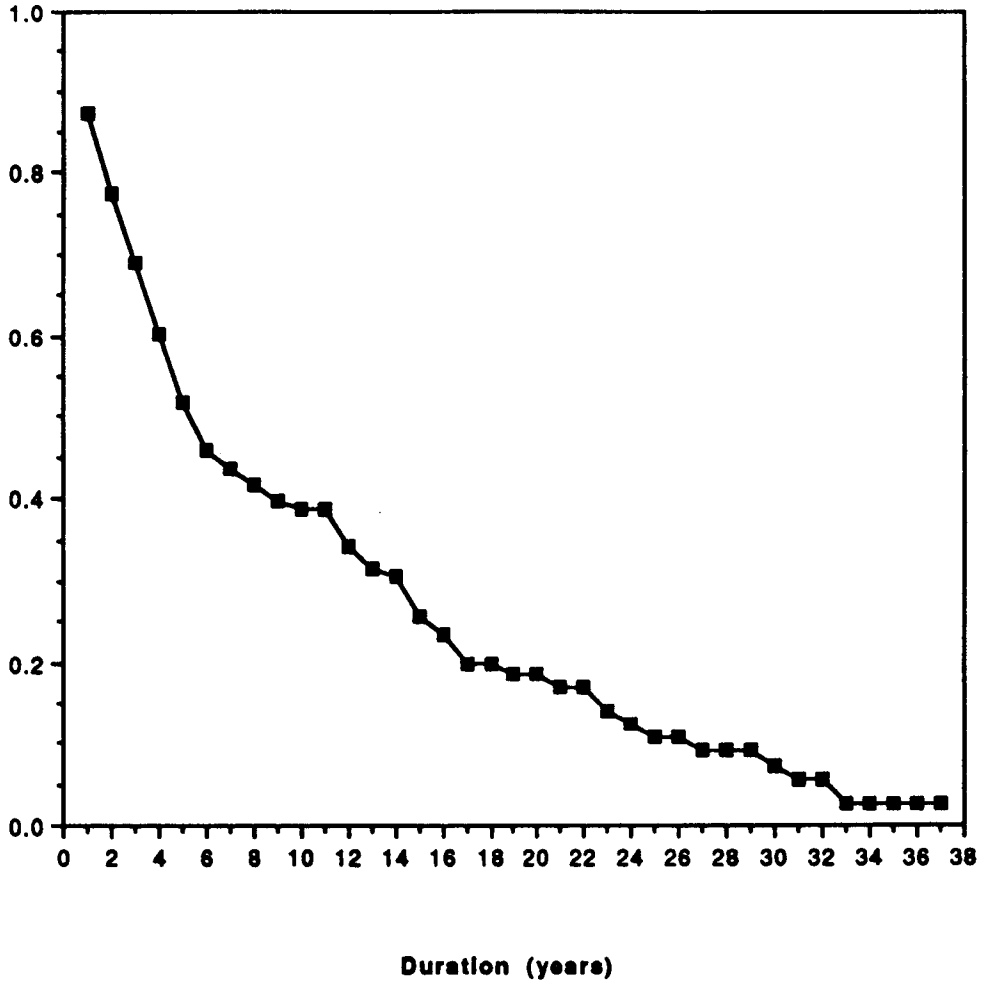

Figure 2: Webb–Pomerene Integrated Hazard Function

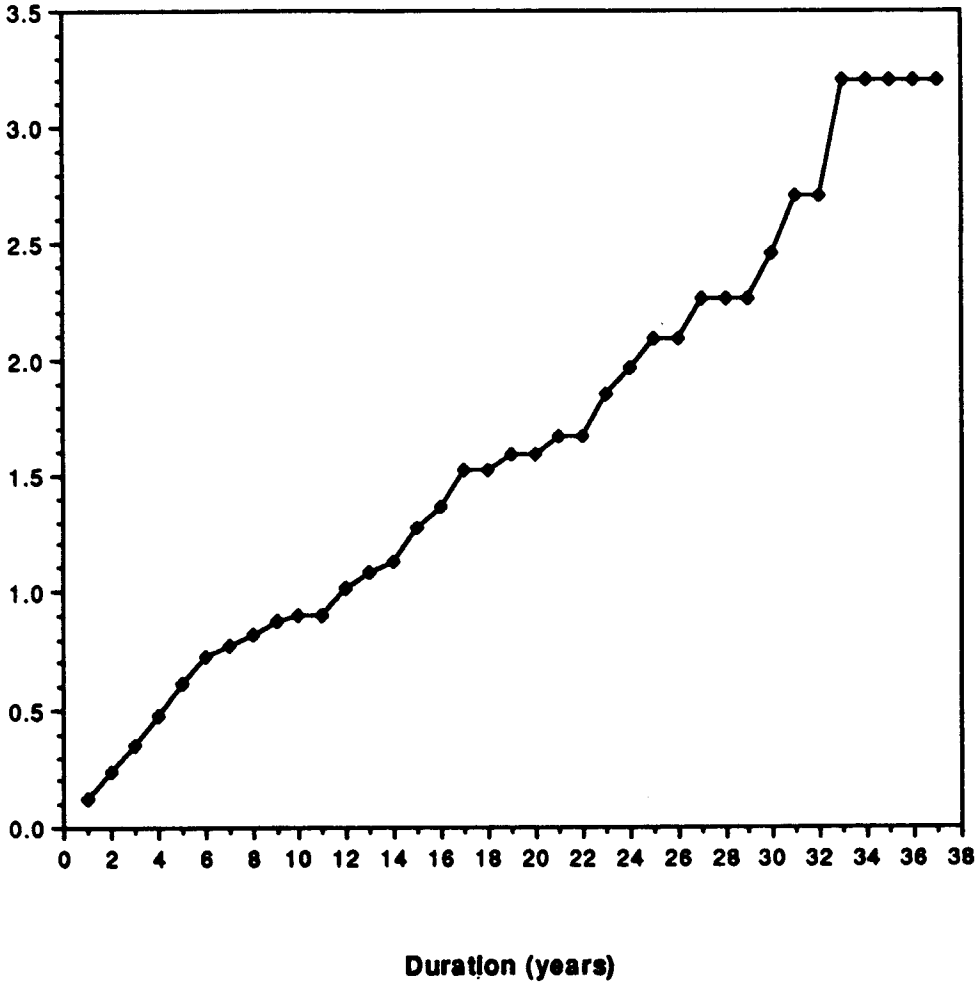

Table I

Functions Performed by 23 *Webb–Pomerene* Cartels in 1962

Function	No. of associations
Price setting and market allocation	19
Foreign sales offices and/or agents	13
Market research and information	9
Sales to U.S. government agencies	9
Freight and insurance	8
Negotiating with foreign governments and agencies	7
Promotional activities	6
Publications	6
Representing members before U.S. agencies	5
Statistical services	4
Engineering and related services	4
Distributing and licensing activities	4
Market development	3
Foreign storage facilities	2
Financing	2
Credit information	1

Source: United States Federal Trade Commission (1967, 48).

Note: Data are based on 23 (out of 26) *Webb–Pomerene* cartels that were active in 1962. Each association performed two or more of the functions listed.

Table II: Life Tables for Full Sample

Duration (≤ years)	At Risk	Exited	Censored	Hazard Rate	Survival Rate	(Standard Error)
1	111	14	1	.1261	.8739	(.0315)
2	96	11	2	.1146	.7737	(.0398)
3	83	9	2	.1084	.6898	(.0442)
4	72	9	0	.1250	.6036	(.0471)
5	63	9	2	.1429	.5174	(.0484)
6	52	6	0	.1154	.4577	(.0485)
7	46	2	0	.0435	.4378	(.0484)
8	44	2	2	.0455	.4179	(.0482)
9	40	2	1	.0500	.3970	(.0480)
10	37	1	1	.0270	.3863	(.0479)
11	35	0	1	.0000	.3863	(.0479)
12	34	4	1	.1176	.3408	(.0474)
13	29	2	0	.0690	.3173	(.0469)
14	27	1	0	.0370	.3056	(.0466)
15	26	4	0	.1538	.2586	(.0450)
16	22	2	1	.0909	.2350	(.0439)
17	19	3	1	.1579	.1979	(.0418)
18	19	0	0	.0000	.1979	(.0418)
19	15	1	0	.0667	.1847	(.0411)
20	14	0	2	.0000	.1847	(.0411)
21	12	1	0	.0833	.1693	(.0404)
22	12	0	0	.0000	.1693	(.0404)
23	11	2	0	.1818	.1386	(.0385)
24	9	1	0	.1111	.1232	(.0372)
25	8	1	1	.1250	.1078	(.0356)
26	6	0	0	.0000	.1078	(.0339)
27	6	1	0	.1667	.0898	(.0339)
28	5	0	0	.0000	.0898	(.0339)
29	5	0	0	.0000	.0898	(.0339)
30	5	1	0	.2000	.0718	(.0315)
31	4	1	0	.2500	.0539	(.0283)
32	3	0	1	.0000	.0539	(.0283)
33	2	1	0	.5000	.0269	(.0237)
34	1	0	0	.0000	.0269	(.0237)
35	1	0	0	.0000	.0269	(.0237)
36	1	0	0	.0000	.0269	(.0237)
37	1	0	1	.0000	.0269	(.0237)

Table III

Summary of Regression Variables

(111 observations, except as noted)

Mnemonic	Description (source)	Predicted Effect on Cartel's Hazard Rate	Mean	Standard Deviation
Dependent variable				
LENGTH	Cartel episode length in years (1)		8.75	8.45
Buyer characteristics				
BUYCR	Percent of U.S. exports sold to four largest importing countries (6)	+	56.35	18.72
EXGROW	Average absolute annual growth rate in industry export volume during cartel episode, by product category [1] (4, 11)	+	0.10	0.17
EXPVAR	Coefficient of variation in quarterly export prices during the cartel episode, by product category [1] (4, 11)	+	0.03	0.03
Seller characteristics				
CARTELN	Number of firms in cartel (1)	+	18.32	23.97
WPCOVER	Dummy = 1 for cartels covering ≥ 50% of industry exports (1, 8)	–	0.38	0.49
SELLCR	4–firm domestic industry concentration ratio (2)	–	58.33	26.85
EXSHARE	U.S. share of world exports (6, 8)	–	0.26	0.19
EXPERIENCE	Length of prior cartel episode(s) discounted 20% annually during interim period (1)	?	0.81	2.52
JSA	Dummy = 1 for cartels adopting a joint sales agency[2] (1, 8)	–	0.58	0.50
Product characteristics				
HOMOG	Dummy = 1 for homogeneous goods	–	0.47	0.50
CAPITAL	Ratio of fixed capital to output (13)	–	0.48	0.44

[1] Each cartel episode is assigned to either foods, crude materials, semi–manufactures or finished manufactures.

[2] Data are available for 69 observations.

Business cycle timing				
EXPCYCLE	Dummy = 1 if cartel episode ends in a peak–to–trough period for export price, by product category [1] (4, 11)	?	0.54	0.50
EXQCYCLE	Dummy = 1 if cartel episode ends in a peak–to–trough period for export quantity, by product category [1] (4, 11)	?	0.32	0.47
GNPCYCLE	Dummy = 1 if cartel episode ends in a peak–to–trough period, based on United Kingdom GNP cycle (5, 9, 12)	?	0.32	0.47
Multi–market collusion opportunities				
EXORIENT	Industry export orientation ratio (exports / total shipments) (3)	–	0.06	0.05
INDVARQ	Coefficient of variation in quarterly U.S. industrial output during the cartel episode, by product category [3] (7)	+	0.03	0.04
INDCYCLE	Dummy = 1 if cartel episode ends in a peak-to-trough period for U.S. industry output, by product category [3] (7)	?	0.22	0.41
DTACOVER	Percentage of industry's sales covered by a domestic trade association (10)	?	49.52	10.43
INTL	Dummy = 1 if an international cartel exists in the industry during cartel episode (8)	–	0.14	0.34
Control variable				
WW2	Dummy = 1 if the cartel episode ends between 1939 and 1945		0.18	0.39

Note: CARTELN and WPCOVER pertain to the start of the cartel episode. BUYCR, SELLCR, CAPITAL, EXSHARE, EXORIENT and DTACOVER are measured as close as possible to the middle of the cartel episode. Timing for the remaining variables pertains to entire length of cartel episode.

Legend for Data Sources: (1) United States Federal Trade Commission (1967), (2) Temporary National Economic Committee (1941a), (3) United States Department of Commerce (1969), (4) United States Department of Commerce (1972), (5) Mitchell (1988), (6) United Nations (1952), (7) United States Board of Governors of the Federal Reserve System (1972), (8) United States Senate (1946), (9) Moore and Zarnowitz (1986), (10) Temporary National Economic Committee (1941b), (11) Mintz (1967), (12) Burns and Mitchell (1946), and (13) Creamer, Dobrovolsky and Borenstein (1960).

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[3] Each cartel episode is assigned to either mining, non–durable manufacturing or durable manufacturing.

Table IVa — Proportional Hazard Rate Regressions — Full Sample

(111 observations; t–statistics in parentheses)

Variable	(1)	(2)	(3)
BUYCR	0.010	0.011	0.018**
	(1.20)	(1.30)	(1.99)
EXGROW	1.088*	0.454	0.477
	(1.86)	(0.70)	(0.78)
EXPVAR	11.157***	11.515***	12.786***
	(3.69)	(3.31)	(3.68)
CARTELN	–0.31 E–2	–0.74 E–2	. . .
	(–0.46)	(–1.05)	
WPCOVER	–0.908***	–0.542*	. . .
	(–3.01)	(–1.65)	
(1–WPCOVER) x CARTELN	. . .	. . .	0.015**
			(2.27)
EXSHARE	0.340	0.756	1.060*
	(0.55)	(1.17)	(1.65)
SELLCR	0.47 E–2	–0.65 E–3	0.43 E–2
	(0.86)	(–0.10)	(0.73)
EXPERIENCE	0.108***	0.138**	0.091
	(2.91)	(2.26)	(1.56)
HOMOG	–0.408	–0.206	–0.088
	(–1.39)	(–0.65)	(–0.28)
CAPITAL	–0.120	–0.074	–0.134
	(–0.36)	(–0.21)	(–0.36)
EXPCYCLE	0.828***	1.077***	1.099***
	(2.91)	(3.34)	(3.48)
EXORIENT	. . .	3.490	2.110
		(1.35)	(0.81)
INDVARQ	. . .	12.394***	12.433***
		(3.33)	(3.36)
INDCYCLE	. . .	–0.476	–0.470*
		(–1.49)	(–1.45)
DTACOVER	. . .	0.51 E–3	–0.35 E–2
		(0.03)	(–0.22)
INTL	. . .	–0.929**	–1.038**
		(–2.02)	(–2.41)
WW2	–0.340	–0.703**	–0.840***
	(–1.12)	(–2.03)	(–2.51)
Log–likelihood	–326.79	–316.98	–317.49

Note: *, **, and *** [illegible]

Table IVb — Weibull Distribution Survival Regressions — Full Sample

(111 observations; t–statistics in parentheses)

Variable	(1)	(2)	(3)
BUYCR	0.77 E–2	0.69 E–2	0.012**
	(1.36)	(1.38)	(2.21)
EXGROW	0.740*	0.316	0.301
	(1.83)	(0.74)	(0.69)
EXPVAR	9.116***	9.001***	10.151***
	(4.80)	(3.53)	(4.01)
CARTELN	–0.44 E–2	–0.64 E–2**	. . .
	(–1.34)	(–2.20)	
WPCOVER	–0.679***	–0.394**	. . .
	(–3.57)	(–1.94)	
(1–WPCOVER) x CARTELN	. . .	. . .	0.005**
			(2.04)
EXSHARE	0.226	0.503	0.796*
	(0.49)	(1.19)	(1.74)
SELLCR	0.41 E–2	–0.10 E–3	0.40 E–2
	(1.06)	(–0.02)	(1.04)
EXPERIENCE	0.086***	0.091***	0.052*
	(2.52)	(2.73)	(1.64)
HOMOG	–0.330	–0.140	–0.032
	(–1.42)	(–0.59)	(–0.13)
CAPITAL	–0.088	–0.082	–0.165
	(–0.41)	(–0.36)	(–0.73)
EXPCYCLE	0.609***	0.708***	0.747***
	(2.82)	(3.81)	(3.92)
EXORIENT	. . .	2.266	1.344
		(1.30)	(0.87)
INDVARQ	. . .	9.089***	9.698***
		(3.77)	(4.12)
INDCYCLE	. . .	–0.394*	–0.386
		(–1.82)	(–1.50)
DTACOVER	. . .	–0.80 E–3	–0.60 E–2
		(0.07)	(–0.54)
INTL	. . .	–0.395	–0.521*
		(–1.30)	(–1.72)
WW2	–0.295	–0.586**	–0.721***
	(–1.15)	(–2.11)	(–2.78)
Constant	–3.102***	–3.188***	–3.916***
	(–6.46)	(–4.98)	(–5.48)
α	1.504***	1.695***	1.610***
	(9.29)	(9.57)	(9.25)
Log–likelihood	–125.67	–114.06	–116.36

Table Va — Proportional Hazard Rate Regressions — Sub-sample

(69 observations; t-statistics in parentheses)

Variable	JSA (1)	Predicted JSA (2)	Unexplained JSA (3)
BUYCR	0.017	0.013	0.015
	(1.14)	(0.87)	(0.98)
EXGROW	0.483	0.773	0.504
	(0.56)	(0.92)	(0.59)
EXPVAR	35.980***	34.128***	35.544***
	(3.94)	(3.70)	(3.84)
CARTELN	–0.57 E–2	–0.26 E–2	–0.46 E–2
	(–0.51)	(–0.24)	(–0.42)
WPCOVER	–1.069**	–0.925*	–1.073**
	(–2.14)	(–1.86)	(–2.13)
EXSHARE	2.443**	1.134	1.510
	(2.25)	(0.81)	(1.34)
SELLCR	–0.61 E–2	–0.18 E–2	–0.36 E–2
	(–0.44)	(–0.13)	(–0.26)
EXPERIENCE	0.169**	0.206***	0.184***
	(2.22)	(2.77)	(2.46)
HOMOG	0.837	0.527	0.713
	(1.54)	(0.98)	(1.33)
CAPITAL	–1.106	–1.000	–1.040
	(–1.53)	(–1.41)	(–1.44)
EXPCYCLE	1.971***	1.918***	1.907***
	(3.91)	(3.88)	(3.84)
EXORIENT	8.963**	8.704**	9.002**
	(2.20)	(2.20)	(2.24)
INDVARQ	22.575***	21.293***	22.333***
	(3.90)	(3.77)	(3.85)
INDCYCLE	–1.276***	–1.277***	–1.261***
	(–2.81)	(–2.79)	(–2.77)
DTACOVER	–0.019	–0.026	–0.021
	(–0.60)	(–0.86)	(–0.66)
INTL	–0.855	–0.777	–0.856
	(–1.57)	(–1.42)	(–1.57)
JSA / Predicted JSA Unexplained JSA	–0.675* (–1.60)	0.534 (0.99)	–0.509* (–1.69)
WW2	–1.321***	–0.838	–1.053***
	(–2.48)	(–1.58)	(–2.12)
Log–likelihood	–148.95	–149.71	–148.79

Table Vb — Weibull Distribution Survival Regressions — Sub–sample

(69 observations; t–statistics in parentheses)

Variable	JSA (1)	Predicted JSA (2)	Unexplained JSA (3)
BUYCR	0.52 E–2	0.47 E–2	0.56 E–2
	(0.76)	(0.71)	(0.86)
EXGROW	0.356	0.349	0.285
	(0.76)	(0.76)	(0.59)
EXPVAR	20.928***	20.318***	20.746***
	(3.30)	(3.56)	(3.40)
CARTELN	–0.41 E–2	–0.32 E–2	–0.42 E–2
	(–1.30)	(–1.07)	(–1.43)
WPCOVER	–0.415*	–0.426**	–0.459**
	(–1.72)	(–2.05)	(–1.96)
EXSHARE	1.123**	0.332	0.800
	(2.22)	(0.52)	(1.51)
SELLCR	–0.30 E–2	–0.30 E–3	–0.18 E–2
	(–0.43)	(–0.05)	(–0.28)
EXPERIENCE	0.095**	0.108***	0.097***
	(2.39)	(3.55)	(2.61)
HOMOG	0.362	0.270	0.360
	(1.04)	(0.84)	(1.12)
CAPITAL	–0.467	–0.449	–0.469
	(–1.17)	(–1.24)	(–1.25)
EXPCYCLE	0.904***	0.854***	0.857***
	(3.57)	(4.32)	(3.64)
EXORIENT	4.631*	4.438**	4.497**
	(1.93)	(2.34)	(1.94)
INDVARQ	9.834***	9.714***	9.863***
	(3.96)	(4.73)	(4.08)
INDCYCLE	–0.711***	–0.714***	–0.692***
	(–2.66)	(–2.77)	(–2.62)
DTACOVER	–0.011	–0.012	0.010
	(–0.80)	(–0.78)	(0.77)
INTL	–0.280	–0.310	–0.313
	(–0.88)	(–1.07)	(–0.98)
JSA / Predicted JSA / Unexplained JSA	–0.119	0.406	–0.186
	(–0.47)	(1.42)	(–0.92)

Table Vb continued

WW2	–0.651***	–0.438	–0.593***
	(–3.24)	(–1.98)	(–2.97)
Constant	–3.258***	–3.483***	–3.350***
	(–5.18)	(–5.46)	(–5.42)
α	2.339***	2.383***	2.372***
	(8.32)	(8.06)	(8.21)
Log–likelihood	–53.70	–52.45	–52.87

Note: *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.